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

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this third sense, a rubber band is more "elastic" than a steel wire because it is easier to stretch and will stand a proportionately greater elongation), in addition to the more general sense of (4) the property of returning to original dimensions after distortion. Why do these conflicting senses cause no problem and evoke no impassioned letters to physics journals? Simple! With no conscious effort by anyone, *elasticity* as the name of a *quantity* has quietly disappeared from our technical vocabulary: if elastic modulus (sense 1) is meant, one specifies shear modulus, bulk modulus, or whatever, as needed for the occasion; and "73% elasticity" in the context of partially elastic collisions refers unambiguously to sense 2.

With these examples to give perspective and to sharpen our concepts, we now come to the word *weight*. As in the examples noted above, there is no confusion with the nontechnical senses. The trouble is that the word refers to heaviness without any concern about whether its quantitative expression is m or $m\mathbf{g}$. After all, the word had already been current for centuries before Newton said "$\mathbf{F} = m\mathbf{a}$." Thus weight has two intrinsically different technical senses, and this double meaning is what causes all the confusion.

Confusion? *What* confusion?! Physicists and engineers—and others who understand $\mathbf{F} = m\mathbf{a}$ and the need for consistent units—have no problem in selecting m or $m\mathbf{g}$ as needed for the problem in hand. In the English System, saying that a body "weighs" 16 lb does not in the least specify whether the problem is to be solved with pounds force and slugs or with pounds mass and poundals; and in SI units, the weight is stated in kilograms even though newtons are required when the gravitational force enters the problem. So the only ones confused are the least able Physics I students—and their confusion comes much less from the double technical sense of the word *weight* than from their lack of understanding of $\mathbf{F} = m\mathbf{a}$. Surely this is inadequate to occasion the longstanding dispute.

As seen from the above examples of words with multiple technical senses, any genuinely confusing ones soon fall out of technical use. Thus *the continuing use of* weight *in technical discussions shows that this is harmless and useful for most purposes.* Weight in its ambiguous sense of either m or $m\mathbf{g}$ becomes awkward only in definitions, in sharp distinctions, in close association with mathematical expressions, and in other explicit formal statements. For example, the statement that "weight means either m or $m\mathbf{g}$" is fine if one is willing to speak algebra. But though m is readily replaced by the compact word "mass" when one wants to speak English (that is, there is no need to beat about the bush with some such phrase as "inertial property"), the only adequate synonyms for $m\mathbf{g}$ have been "gravitational force" (2 words, 6 syllables, 18 letters) or "gravitational attraction" (2, 8, 23) or "force of gravitational attraction" (4, 10, 30)! Though these circumlocutions say exactly what is meant, they have the feel of using a meat axe to kill a spider.

So why hasn't the awkwardness been resolved by officially ruling that, in the technical context of mechanics, weight is $m\mathbf{g}$, not m? It isn't for lack of trying! For example, such a ruling was made in the 1901 declaration of the General Conference on Weights and Measures; physics texts have long been unanimous on the point, and as recently as 29 January 1979 to 26 January 1982 the American Association of Physics Teachers censured National Bureau of Standards publications that accept the occasional use of weight as a synonym for mass. But the problem remains unresolved as of December 1982 with the Thomson and Goldman letters; objects are still "weighed" on a beam balance or a grocery scale (honest "weight," no springs) to determine *mass*. Why?! The reason is that *weight* is a long-established and widely used word that belongs to all speakers of English; a dictionary reflects their current usage of the word. Thus if some scientific, technical, or governmental body presumes to legislate a *technical sense that clashes with the everyday dictionary sense*, it can expect to be ignored even (most of the time) by most members of the profession(s) concerned, as illustrated by the long and futile rumpus over the use of the word *weight*. In fact, I doubt that even the US Congress has the Constitutional authority to legislate such matters (especially not for other English-speaking countries); and certainly I can't imagine that it would want to bite off such a can of worms—not even to provide an occasion for this lovely mixed metaphor. So what needs to be done?

What is wanted is a good monosyllabic Anglo-Saxon word that means $m\mathbf{g}$ and *not* m. Fortunately, such a word with exactly the right meaning exists. The noun *heft* (like the related verb *to heft*) derives from the verb *to heave*. It is the weight of a body, explicitly as measured by the force to lift it. Thus the proposed technical definition $heft = m\mathbf{g}$ is completely compatible with the ordinary dictionary senses. The fact that *heft* has largely fallen out of everyday use makes it all the more suitable for adoption in a specialized technical sense. The purpose of the foregoing argument is to suggest that the appropriate committees representing American physicists should consult with the appropriate governmental and international agencies to adopt *heft* as the explicit technical word for $m\mathbf{g}$—to be used at least (and perhaps almost exclusively) in the explicit formal statements mentioned above. But in less demanding discussions (even technical ones in mechanics) we should be free to continue to use *weight* casually in its widely useful everyday senses of *heaviness*, as expressed impartially by either the mass m or the heft $m\mathbf{g}$, and a *heavy object*, such as a paperweight or a calibrated 20-gram weight.

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Superconducting electronics

With reference to Theodore Hartwig's letter (March, page 102), I would like to note that the International Cryogenic Materials Conference to be held in Colorado Springs, 15–19 August 1983, will also feature another event of interest to solid-state physicists. The program for 18 August includes a one-day symposium on Materials and Processing for Superconducting Electronics that is devoted to refractory superconducting films and artificial tunneling barriers. The plenary paper by M. R. Beasley from Stanford University will be followed by sessions on Josephson tunnel junctions, films, barriers, film surfaces and interfaces. The detailed program of the symposium is available from me.

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Pseudo-QCD

In the news story, "A look at the future of particle physics" (January, page 19), many predictions of the standard model, claimed testable in present and future accelerators, are reported to have come out of two high-energy physics conferences during 1982. It should be pointed out that many of the predictions about the strong interactions are not honest predictions of the theory, since quantum chromodynamics (QCD), the component of the standard model essential to describe strong interaction dynamics, is still far from being solved. The problem arises from the fact that the perturbative part of QCD, which is calculable, only describes the behavior of quarks and gluons but not the behavior of hadrons; a prescription of how quarks and gluons turn into hadrons (called "hadronization") becomes necessary to predict observations. Many so-called pre-