LETTERS

Particle Physics and the Everyday World, Grant Pie and the Future of Research

I congratulate Pablo Jensen for his excellent essay "Particle Physics and Our Everyday World" (PHYSICS TODAY, July, page 58). It is unfortunate that too many physicists, especially particle physicists, have a very naive view of the reduction of one scientific theory to another.

A mathematical reduction of a theory is insufficient because many symbols change their meaning; theories on different levels also involve different concepts. Physicists know this intuitively but often pay no further attention to it. Ten years ago, I elaborated on that issue, but my paper reached only philosophers of science.[1] At that time, I believed that only they were often misinformed about it. Since then, I discovered that the same also holds true, unfortunately, for many of my fellow physicists.

To those who would like to see how the reduction of a physical theory works in detail, and how the concepts change drastically, I suggest they look at my reduction of Einsteinian to Newtonian gravitation theory.[2]

References

1. F. Rohrlich, Brit. J. Philos. Sci. **39**, 295 (1988).
2. F. Rohrlich, Found. Phys. **19**, 1151 (1989).

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As one who for the past 15 years has not been a participant in the grantsmanship battles, I believe I can make a few useful *sine ira et studio* comments on Pablo Jensen's essay.

It is sadly amusing, but perhaps not astonishing, that serious scientists waste their time belittling and deeming irrelevant for society certain fields of scientific endeavor (not theirs) while extolling the everyday-life importance of other fields (theirs).

I fear that this kind of anti-intellectualism—which brings back bad memories of the 1960s and early 1970s, when the battle cry of "relevance" and "societal needs" ruined most universities—is motivated by the fact that, whatever the reason, the grant pie is shrinking while the number of pie-hungry individuals is still increasing. It does not become scientists to tortuously invoke societal, epistemological, empirical and even philosophical arguments to influence the pie-division process. Regrettably, Jensen does precisely that in his essay. He severely attacks expensive particle (high-energy) physics (and, albeit less explicitly, other subfields of fundamental, basic research) while overemphasizing the singular importance of condensed matter physics and material science in what he calls the "everyday world."

I fully agree with Jensen that reductionism is passé and that holistic studies of "emergent" concepts and phenomena are increasingly important and necessary for our achieving an understanding of the world—and not only the everyday world. But I strongly disagree when he declares that "science is organized in rather *decoupled* layers" (emphasis added) and hence that fundamental science—for example, particle physics—"is practically irrelevant to understanding our everyday world."

Allow me to use three randomly chosen examples to show that the very deepest layers of physical research have enormous relevance to much higher levels of study and, ultimately, to "everyday" societal phenomena.

First, more than a century ago, James Clerk Maxwell established—with admirable insight, and by going well beyond the everyday world of physics—the concept of field theory and, in particular, the notion of electromagnetic waves. Without that revolutionary basic physics, we never would have had modern communications science, not to mention such societal achievements as radio, television and the Internet—and also including, unfortunately, the horrors that go with them.

Second, had not the greatest minds of our century developed the basic abstract framework of quantum

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theory some 70 years ago, we would never have developed laser science or the science of solid-state electronics.

Third, in 1905, when Albert Einstein developed the way-far-out theory of special relativity (having nothing to do with everyday life), who could have imagined that, by being able to rely on relativistic effects, we would now have an incredibly accurate Global Positioning System, or any working high-energy accelerators?

One realizes as well that strong interconnections among what Jensen calls the "autonomous layers of physics" also exist from the higher levels down to the basic levels. For example, high-energy particle physicists could never have built those enormous accelerators and detectors he mentions had there not developed a much higher level science of microwaves and power electronics and macroscopic technology for superconductivity.

Finally, I would like to point out some of the errors of fact in Jensen's essay. For example, he states that using special clever experiments to detect a nonzero dipole moment of the ammonium molecule is "in apparent contradiction to quantum mechanics," because the latter tells us that the ground state is a superposition of states with opposite dipoles. But he overlooks that it is precisely basic quantum theory that explains how a superposition can be reduced to a single eigenstate, and especially how potential barriers must be treated.

More generally, Jensen wants us to believe that the symmetries on the basic level are irrelevant, because they are often broken in many-particle systems. Actually, the breaking is not haphazard, but follows strict rules of the basic level. That's why we can systematically study ferromagnets, for example.

In addition, Jensen is quite wrong when he says that "breaking matter with higher and higher energies will give you more and more 'fundamental' particles. However . . . there is no theory of everything . . . at high energies and . . . this increasingly expensive race will never end." In fact, in the past 25 years we have obtained a marvelous unification—a simplification of our understanding of the deep layers of basic physics. Instead of hundreds of elementary particles, we now have a tiny set of basic constituents and interaction fields.

Jensen also errs when he urges "(some) particle physicists [to] get out of their accelerator labs. . . ." Many of them are working far away from accelerators, trying to interpret the results of others, thinking up new ways of exploring the deepest mysteries of the universe—and often combining particle physics with astrophysics and even cosmology. (Are these also irrelevant sciences when it comes to everyday life?) And surely it is inadmissable to say, as Jensen does, that particle physicists (and, for example, cosmologists) should "notice that their findings are primarily relevant inside their own professional network" (whatever that term may mean). Is our world really in such a bad state that we all have to become explorers of only the "everyday world" and give up our ancient dreams of striving for an understanding of the fundamental, basic laws of nature?

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Skipped Source

A letter to the editor
Is a tantalizing particle
That makes one feel
one's missed
the boat
If one hasn't read the
article.
—Pat D'Amico

[From The Wall Street Journal*—
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Pablo Jensen cites Jorge Luis Borges's story "The Library of Babel" as illustrating the absurdity of imagining that everything is "contained" in the letters of the alphabet. An earlier story, "The Universal Library" by Kurd Lasswitz (1901),[1] deals with the same idea, but the earliest version of it seems to have originated in the writings of Catalan philosopher Ramón Lull (Raymond Lully) early in the 14th century. His chief work, *Ars magna Lulli*, was subsequently studied by (among others) Giordano Bruno, Gottfried Leibniz and Gustav Theodor Fechner—from whom Lasswitz got the idea. More recently, George Gamow discussed the idea in his *One, Two, Three . . . Infinity* (1947). Of course, Jonathan Swift also used it in *Gulliver's Travels* (1726). Borges is in good company!

Reference

1. See Willy Ley's translation, in C. Fadiman, ed., *Fantasia Mathematica*, Simon & Schuster, New York (1958).

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JENSEN REPLIES: It is striking that Paul Roman does not give a single example to support his belief in the relevance of particle physics to our understanding of the everyday world. His omission, in effect, confirms the main point of my essay. Although he rightly points out the importance of some pieces of fundamental research for technological *applications*, he proceeds to suggest, much to my surprise, that I was attacking basic research. But where in my essay is there evidence of any such attack?

Even more strange is the accusation of—in his words—my "overemphasizing the singular importance of condensed matter physics and material science." I did not even mention material science. Moreover, I never claimed that everyday-world relevance or applicability was a reliable indicator of the value of a research field or subfield. Rather, it has been others, notably Robert Cahn and Chris Quigg, who have appeared to be bothered by the use of that indicator, although I really do not know why—anymore than I understand why advocates of the importance of particle physics do not simply claim that it has intrinsic aesthetic or knowledge value.

Concerning Roman's charge that I have committed "errors of fact," I must emphasize that quantum mechanics is certainly useful for small molecules but not for large (biological) ones. Moreover, you wouldn't know what to calculate with quantum mechanics without first having higher level knowledge of the problem.[1] It is true that the Standard Model is a beautiful piece of physics, but is it really the end of the game? I do not share Roman's view that particle physicists are the only scientists tackling the "deepest mysteries of the universe," for I think that biologists and psychologists, among others, are focused on mysteries that are at least as deep and interesting. Of course, any views on this issue are really a matter of personal taste.

Lastly, I want to point out a couple of useful lessons to be drawn from Roman's concern that the "grant pie is shrinking" and especially from the cancellation of the Superconducting Super Collider, which was such a shock for particle physicists. The first is that we scientists should not take for granted that society will always fund our research. Some influential politicians argue that, as George Brown has noted, "We have already much of the knowledge and many of the technologies necessary to [solve many of the problems of our society]. The real problem . . . is implementation."[2]

The second lesson is that it is not only a matter of technology. Science is becoming increasingly too specialized and obscure for most people to comprehend readily, and many citizens are actually beginning to fear science results, especially in our increasingly complex technological society. Therefore, all scientists, including particle physicists, should strive to explain to nonscientists why and how their work is relevant to the general public's basic understanding of the world—as, for example, Stephen Gould has done so adeptly in the field of biology.[3] Our internal fights for slices of the grant pie look rather petty in this context.

References

1. A. J. Leggett, Found. Phys. **22**, 221 (1992).
2. G. E. Brown, Am. J. Phys. **60**, 779 (1992).
3. See, for example, S. J. Gould, *Urchin in the Storm*, Norton, New York (1988).

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[Editor's note: See "Opinion," on page 57, for Robert Cahn's reply to Pablo Jensen's essay in the July issue.]

Charge Inhomogeneity Plays a Key Role in Physics of Cuprates

In the June issue of PHYSICS TODAY (page 19), Barbara Goss Levi presents the charge and spin "stripes" in high-critical-temperature superconductors, posing the question as to whether they are a universal feature of these systems. I would like to add a few comments to the theoretical side of the story and to recall how charge inhomogeneity was introduced as a relevant aspect of the physics of the cuprates.

In 1990–91, it became clear that most models with strong local repulsion that had been introduced to represent the CuO_2 planes show phase separation in hole-poor (antiferromagnetic) and hole-rich (metallic) regions for physically relevant values of the parameters. In these models, Cooper pair formation occurs before and near phase separation and points to a connection between phase separation and superconductivity.[1] Thus, charge inhomogeneity appears as a simple mechanism to reconcile the strong repulsion, which leads to antiferromagnetism at very low doping, with the attraction that is necessary for superconductivity at higher doping.

In the presence of long-range forces, however, it would take too much electrostatic energy to segregate charges on a macroscopic scale, as required by phase separation. Charge fluctuations may still be locally and dynamically present in what is called frustrated phase separation.[2] Due to the interplay between the tendency toward phase separation and the presence of coulombic forces, a state with a spatial modulation of charge, not related to the period of the lattice, is also possible.[3] This charge density wave gives rise to charge-driven stripes that, because of the natural tendency toward antiferromagnetism of the materials at low doping, also imply spin modulation, thereby extending the antiferromagnetic fluctuations to regions of doping far away from the onset of the antiferromagnetic order. According to this scenario,[3] the onset of the stripe phase is a specific outcome of the occurrence of a quantum charge instability governed by a quantum critical point near optimum doping. In this way, one can explain why, around optimum doping, one gets both a deviation from Fermi liquid behavior and the occurrence of the maximum critical temperature: In fact, spin and charge fluctuations near a quantum critical point mediate an effective potential, which is sufficiently singular to disrupt the Fermi liquid behavior in the normal phase and to produce a strong d-wave pairing mechanism for the superconducting phase.

References

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3. C. Castellani, C. Di Castro, M. Grilli, Phys. Rev. Lett. **75**, 4650 (1995); Physica C **282–87**, 260 (1997).

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NVFRAMs Story Spurs Volatile Debate About Physics, Applications

We enjoyed the cover story on nonvolatile ferroelectric memories by Orlando Auciello, James Scott and Ramamoorthy Ramesh (PHYSICS TODAY, July, page 22). The article gives a nice flavor of the challenges that have been overcome in recent years to make ferroelectric memories commercially viable. It is exciting to see successful devices after decades of hard work, disappointment and innovation. There is a key question that remained for us after reading the article: Is the memory technology currently on the market as implied in the text and in the illustrations of smart cards?

We also would like to clarify one technical point made in the text. The discovery of ferroelectricity in films "less than 0.9 nm" thick (the films are actually slightly thicker) was reported recently by our group.[1] Significantly, it was achieved in polymers of vinylidene fluoride and trifluoroethylene, a ferroelectric material with its own storied history.

Reference

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We have a high regard for the work of Orlando Auciello, James Scott and Ramamoorthy Ramesh, but we feel it is necessary to point out five basic physics errors and also some omissions in their article.

First, the statement that "Ferroelectric crystals are characterized by having polarization vectors that can be oriented in two diametrically opposite directions . . ." (page 22) is somewhat misleading and restrictive. It is true that ferroelectric *crystals* can have two or more polarization states in which the polar vectors lie in equivalent (enantiomorphic) crystallographic directions, and also that ferroelectric *memories*, which typically use polycrystalline ferroelectric films, make use of two net polarization states. However, a memory cell can utilize several polarization states from the ferroelectric crystallites composing the film, and those states are not necessarily diametrically opposed.

Second, the caption for figure 1a (page 23) is incorrect. It states that the structure shown is "cubic perovskite $Pb(Zr_xTi_{1-x})O_3$," whereas what is actually displayed in the fig-