GINZBURG'S 'IMPORTANT AND INTERESTING PROBLEMS' APPLAUDED AND AMENDED

Vitaly Ginzburg concluded his well-written column (May 1990, page 9) with a comment that every physicist ought to know *something* about *all* the subjects mentioned in his comprehensive list of "especially important and interesting problems" of physics. Not only do I heartily concur with that comment, but I am sorry he did not emphasize it more strongly. I was pleased to note that the list includes grasers—an excellent example of the physicist's need for breadth.

The development of grasers, although sought for three decades, has been retarded relative to other fields that opened at about the same time, to some extent because few physicists can think of things one might do with a graser(!), but largely because it has been generally assumed that advances to shorter wavelengths must be a uniform progression and, further, that if x-ray lasers cannot advance into the kilovolt range, then there is little hope of stimulating gamma radiation. This is an unfortunate misconception, because the x-ray and gamma-ray laser fields involve almost entirely different phenomena: the one, largely atomic and plasma physics; the other, not only atomic physics but solid-state, chemical and nuclear science as well.[1]

I believe that this misconception, as well as the inability to think of eventual applications, has its roots in the overspecialization that characterizes the present generation of physicists, who are either unable or reluctant to cope with—or, if with a support agency, to sponsor—so highly interdisciplinary a problem. Still, in the past the most significant advances in science and technology have usually arisen in borderline areas common to two or more distinct disciplines.

I recall the advice given me by Albert Hull shortly after I joined the General Electric Research Laboratory in the early 1940s, to the effect that a physicist should change his field every five years or so. At that time, the laboratory actually encouraged this as a stimulus to cross-fertilization; it was felt that a newcomer to a field would not know why "it can't be done!" But today, overspecialization is encouraged, not merely by the enormous volume of scientific knowledge, but even more by the way science is supported in this country, with the emphasis on support of individual specialists rather than of institutions, and by the overcompartmentalized organization of the latter. These make it increasingly difficult to exchange ideas with workers in other fields, to change one's own field or even to broaden one's professional scope.

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We take exception to Vitaly L. Ginzburg's choices of "important and interesting" topics within astrophysics. All eight topics he lists—cosmology, neutron stars and so on—are in high-energy astrophysics. While we respect the work of our high-energy colleagues, we emphasize to the young physicists for whom Ginzburg's list is intended that other profound problems exist. We have devoted our careers to one of these—the formation of stars. Man's fascination with stars is, after all, the *raison d'être* of astronomy, and the question of their origin is a central one, as yet unsolved. Given the diverse observational and theoretical tools that can be brought to bear on this problem, it deserves the attention of talented young researchers. By focusing exclusively on the high-energy end of astrophysics, Ginzburg is surely too narrow in his vision.

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One can argue whether Vitaly Ginzburg's list is complete, and that in fact it would be more appropriate to compile subjects that are *un*important. True science is often done in solitude and often in a field that is totally out of fashion.

Nonetheless, a few remarks concerning the list are necessary. It contains no reference to atomic and molecular physics, nor any topic that is related to it. (At the moment superlarge molecules, which Ginzburg does mention, are more in the domain of biology than of physics.) This field is very important for understanding chemistry, processes in molecular clouds, interaction of radiation and matter, and so on. It is also unavoidable in research on fundamental aspects of quantum and classical theory, which have essential relevance even to high-energy physics. And yet Ginzburg has included, for example, surface physics, a very important subject and a huge area of research. It is listed under "Macrophysics": Is this because only the kinetics problems are meant, or because atoms and molecules are so huge that they are considered "macroobjects"? If the latter, then the author has little knowledge of the richness of atomic and molecular physics. Surface physics is such a huge field that anybody could have put it on the list without hesitation. It would be interesting to know Ginzburg's reasons for putting it on the list: What in this field is so important, and why?

Take another topic: weak and electromagnetic interactions. Does Ginzburg know that one of the basic arguments that led to the formulation of weak interactions really has no basis in quantum theory? It is known from the study of atom–atom collisions that with increasing interaction the lifetime of resonances does not necessarily decrease, but in fact increases. This is not to say that the today's theory of unification is wrong, but one gets the impression that there is no doubt that it is right.

In short, nature holds surprises, but

they will be revealed, most probably, from totally unpredictable sources.

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Vitaly L. Ginzburg raises a question that many a scientist must at some time in his career have asked himself—namely, whether he should broaden the base of his knowledge or deepen his knowledge in some particular field. Which of these choices one opts for, whenever one does so consciously, may depend upon a number of factors: the circumstances of one's work, the prevailing opinion among the celebrities in one's field and, above all, one's particular scientific ethos. Academicians and Nobel laureates apart, the question that really concerns the scientist is whether he would become more effective (in some well-defined sense, such as being more creative) by broadening or by deepening his knowledge. While Ginzburg expresses himself unequivocally in favor of breadth, even for "pure pragmatists" (as he calls them), it is not so clear whether this is the right choice for an applied physicist working, say, in industry. Even the academic physicist cultivating breadth may find that for lack of depth he is not as productive in his research as his colleague who cultivates depth. Indeed, no one seems to have ascertained whether breadth and depth are wholly counteracting qualities or whether they have some measure of coherence, the one assisting in the development of the other.

Confused by such matters, more than three decades ago I lost no time in posing this problem to the distinguished astrophysicist S. Chandrasekhar when he visited the Indian Institute of Science, Bangalore, and met the faculty for an informal talk. His reply ran something like this: "Every scientist should ask himself how wide he can spread out without becoming ineffective. He should know his limits. What is possible for Fermi, for instance, is not possible for me."

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Vitaly L. Ginzburg raised in his Reference Frame column a topic that is important to all physicists. The problem of keeping oneself informed of new developments in physics is more difficult for physicists at small universities in developing countries.

I belong to a small Indian provincial university whose physics faculty consists of 20 teachers spread over four broad branches of physics. There is very little interaction among ourselves or with the outside world. With a view to alleviating the situation, a few years ago I organized weekly seminars whose main object was to review articles in PHYSICS TODAY, *Scientific American*, *Contemporary Physics* and *Reports on Progress in Physics*. Most of my colleagues felt that it was a waste of time. Still, five to six teachers and a few PhD students participated. But all the teachers except three talked only of their own research. The seminars were discontinued after six months.

True to the Indian tradition that each man should seek his own salvation, I have tried to follow on my own the new developments in most of the branches of physics. I read regularly all the physics news in *Nature* and *Science*; all the articles and book reviews in PHYSICS TODAY, *Scientific American* and *Contemporary Physics*; and the abstracts of all the articles in *Reviews of Modern Physics* and *Reports on Progress in Physics*. On the average this takes about one hour per day. When I read the list of 24 "especially important and interesting problems" given by Ginzburg, I felt happy that I have some basic knowledge about most of the fields. Even with my best efforts I am not able to understand strings, phenomena in vacuum, quantum gravity and GUTs.

At the material level, I benefit from my reading by getting new ideas for research. For instance, while reading an article on ferroelectrics, I began to wonder whether the ferroelectric state of the medium has any effect on the energy loss of electrons. We investigated this experimentally and found that there is some effect.[1] Subsequently, Ginzburg and A. A. Sobyanin investigated the theoretical aspects of this effect.[2] Further, when the whole physics community went hot with cold fusion, we tried, in our own modest way, to see whether in partially deuterated ammonium bromide there can be p–d fusion reactions in the freely rotating ammonium ion; we even tried to ionize the hydrogen atoms through beta irradiation. Though we did not observe any fusion, we went through the excitement of the time and enjoyed doing "crazy" physics.

Physics is music of reason. Music can be enjoyed only if one is introduced to it and cultivates a taste for it. Similarly, one can enjoy physics of the kind produced by solo players like Einstein and Dirac and of the kind produced by the orchestras conducted by Carlo Rubbia and Leon Lederman only if one is first made to see physics with a broad perspective and then provided with the opportunities for sustaining such an interest. All our graduate students should be made to take one compulsory general course that reviews the whole of physics at a higher level, emphasizing the problems of current interest in terms of principles of physics, with minimum dependence on mathematics, so that the students get the necessary background to follow new developments.

Lastly, may I add one topic to Ginzburg's list? That is, precise measurements and extremum values of physical quantities—for instance, precise measurement of the g values of the electron and of the muon; the upper limit for the proton–electron charge difference; the maximum and the minimum pressure, temperature, magnetic field and so on attained in the laboratory and existing in nature, and the methods of measuring them. Knowledge of this topic would make all physicists appreciate the versatility of experimental techniques.

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GINZBURG REPLIES: In my Reference Frame column I "strongly emphasized the conditional and subjective character of any list of 'especially important and interesting problems' and the impossibility of studying only those problems," and I added, "Still, one cannot embrace everything, and for educational purposes . . . it is absolutely imperative to have a limited range of questions." There are hundreds of important and interesting problems not included in my list. Everybody could omit some problems from the list and add others—for instance, the star formation problem or some questions from atomic or molecular physics. Of course, the corresponding lecture or text must contain arguments for including this or that problem, and I have done this for the case of surface physics in particular.

I use this opportunity to explain that my problem 10, "Strongly nonlinear phenomena," includes chaos and turbulence.

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'Nonadiabatic' Solar-ν Solution's Significance

I enjoyed the lively update on solar neutrinos by Bertram Schwarzschild (October, page 17) except for one blemish. He did an excellent job of conveying the excitement of the latest experimental results and the implications for physics beyond the standard model, but he gave the impression that the significance of the "nonadiabatic" Mikheyev–Smirnov–Wolfenstein solution was only realized in the recent paper of John Bahcall and Hans Bethe. In fact it had been realized long before.

The different types of MSW solution to the solar neutrino problem are contained in the original papers of S. P. Mikheyev and A. Yu. Smirnov, but the detailed properties of the nonadiabatic solution were first elucidated in 1986 by James M. Gelb and myself[1] and independently by Edward W. Kolb, Michael S. Turner and Terence P. Walker.[2] In particular, it was stressed at that time that the nonadiabatic solution could lead to a very small signal in the gallium experiment, whereas the adiabatic solution always gives a gallium signal close to the predictions of the standard solar model.

In 1988 the Kamiokande II collaboration reported its initial findings at the Munich conference, and Gelb and I immediately realized that the central value of R, the ratio of the observed to the expected signal, fell within the narrow band predicted by the nonadiabatic solution and within the broader band of the "large-angle solution," but definitely outside the range of the adiabatic solution. Unfortunately, the errors at the time were too large to allow one to draw any definite conclusions. We did observe that "were the error on the preliminary value 15% instead of 30%, then the adiabatic solution could be excluded at the 2-sigma level," and we pointed out that the gallium experiment could be used to distinguish between the nonadiabatic and large-angle solutions.[3]

I repeated these remarks in January 1990 at the Moriond workshop and was emboldened to argue that the gallium signal should be well below the standard-model predictions. Subsequent events have borne out this picture far better than I could have hoped for. The latest result on R from the Kamiokande II collaboration maintains the same central value as in the original Munich report, but the error is now reduced by a factor of two, and the preliminary SAGE result is, as the whole world knows, much smaller than expected. I, for one, most certainly hope that this trend continues in the future results of both SAGE and GALLEX.

The purely numerical analysis by Gelb and myself was put on a secure and relatively simple footing by Wick C. Haxton,[4] Stephen J. Parke,[5] and Arnon Dar, A. Mann, Y. Molina and D. Zaifman[6] using the Landau–Zener approximation. Exact analytical results were subsequently derived by Dirk Notzold[7] and P. Pizzochero.[8] This work, together with our original observation that the mass difference factor times the mixing angle is roughly 3×10^{-8} eV^2, leads to a simple modification factor for the spectrum of electron neutrinos arriving at Earth, namely $\exp(-9/E)$, where E is in MeV.

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10/90

Public Sees Physicists in the Wrong Light

I enjoyed the November special issue of PHYSICS TODAY on communicating physics to the public, as the topic has been on my mind. Recently, a ninth-grader participating in a summer program in our laboratory had this to say after his first day with us: "You guys are scientists, right? But you don't look like scientists!" I asked him what he thought a scientist should look like. He said something about television and movies, and it was clear to me that the creatures he had imagined weren't exactly human. Since then, I've paid attention to how physicists and other scientists are presented and misrepresented in the media.

Take molecular-beam epitaxy machines. MBE machines are among the most visually dramatic (that is, large and expensive) of our high-technology research tools, and photographs of them often appear in publicity pieces, news articles, annual reports and the like. Nine times out of ten, these photos feature a normal MBE scientist posed next to an expanse of stainless steel, bathed in blue, red and green light. The blue, red and green scientist looks otherworldly, bizarre and unnatural, and if I were a ninth-grader, I don't think I would aspire to be one. I know many MBE scientists, and none of them are blue, red or green. Moreover, only a few of them are otherworldly or bizarre.

Another typical photo of scientists at work that makes its way into the popular media shows multicolor lasers being meditated upon by one or more laser jocks. Fog and multiple exposures turn the invisible beams into brilliant swords of color. Somehow the laser jock is bathed in the same red, blue and green light that usually emanates from MBE machines. These images are only slightly more accurate than the MBE pictures. (Dye-laser jocks sometimes *are* red.) I'm sure that 90% of the population believes that laser beams actually glow as they propagate.

There is no reason to present ourselves and our everyday environments to the public this way. A commercial photographer who can't compose an interesting picture of an MBE machine without colored lights is more commercial than photographer. We should realize that since most commercial photographers have seen the same movies as the ninth-grader mentioned above, we may have to help them produce accurate images of us.

I urge readers of PHYSICS TODAY to consider carefully the images they present to the outside world. Make sure that scientists and engineers are depicted as men and women rather than as space aliens: Don't pose under colored lights!

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11/90

What Feminism Means for Physics

It is easy enough to be sympathetic to the two negative letters (December, page 93) reacting to Sidney Harris's cartoon in which a woman scientist says to her male colleague, "It's an excellent proof, but it lacks warmth and feeling." But it is also easy to be *un*sympathetic. Though the two letter writers complained about social prejudices that make science careers