LETTERS

How Should Physicists, Biologists Work Together? The 'Harness the Hubris' Debate Continues

Adrian Parsegian makes interesting points in his article "Harness the Hubris: Useful Things Physicists Could Do in Biology" (July, page 23), as does Robert Austin in his response. But both of them fail to make clear what they believe has been lost (or not realized) because academic physicists collectively don't address more biological problems.

More biology can be added to physics from the bottom up as well as from the top down. Teachers of introductory physics can simply incorporate materials to start students thinking about the physical problems of biology. A case can be made that many topics in introductory physics are taught better using biological examples. In my first lecture in Physics I (mechanics), I ask students how they got into the room: "Given limbs with stiff bones, flexible joints and muscles that only contract, how did you do it?" Students must know an amazing amount of physics to keep their bodies upright and propel them through doors and up stairs to arrive at their seats. Students are also skilled at managing their energy and body temperature. It helps them to know that, averaged over a day, human bodies are roughly 100-watt machines, while hearts are roughly 10-watt pumps. Acoustics, fluids and vision also provide wonderful examples for teaching.

There are useful biological examples at higher levels (too many for listing here), but more needs to be done in molecular biophysics, as Parsegian points out.

Perhaps physicists' contributions to biology have not been lost but only delayed—until we academic physicists respond better to the case for more biophysics.

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I enjoyed Adrian Parsegian's thoughtful article and was amused by Bob Austin's response. Missing from both their points of view, however, was the question of how to train students so they will be prepared to make contributions in the application of physics to biology.

Many of the relatively tractable problems, for which physics can usefully be applied to biology by individuals who do not have a full understanding of the diversity of biological systems, have been solved. In the foreseeable future, the big payoffs in biophysics will come increasingly from applying a physical understanding in the context of an equivalent understanding of biological diversity and specific biological phenomena.

In our graduate program in biophysics and computational biology here at Illinois (and presumably in similar programs around the country), we are seeking to train a new kind of scientist—a true biophysicist who will have the physicist's understanding of the underlying unities of physical law, the biologist's understanding of biological diversity and a practical and theoretical knowledge of the experimental and computational techniques needed to understand biological phenomena in quantitative and physical terms. As the late Gregorio Weber, an eminent biochemist and biophysicist here at Illinois, observed: If you get a physicist with n good ideas and a biologist with n good ideas thinking about a biophysical problem, you will get $2n$ good ideas applied to the problem; but if you get one individual trained in both disciplines thinking about the same problem, you will get $n!$ good ideas applied to the problem.

The task of training individuals with this array of capabilities may sound formidable, but it is essential. Austin's list of big problems is a good starting roster of problems that will not yield to any lesser array of capabilities than this.

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Robert Austin sees Adrian Parsegian's proposal that physicists explore collaboration with biologists as being too modest. He wants physicists to work on "big problems."

Those interested in following Austin's position may want to read about Max Delbrück, a theoretical physicist who turned to biology.[1] Inspired by Niels Bohr's idea of complementarity, Delbrück's work on viruses was recognized by his becoming a co-winner of the Nobel Prize in Physiology or Medicine in 1969. Interestingly, Delbrück, by his own admission, never achieved his goal of using complementarity to expose fundamental biological paradoxes. Nonetheless, I suspect that biologists would welcome many more of the likes of Max Delbrück, even if they fail in their epistemological quests.

Parsegian is right in his assertion that most physicists (or scientists for that matter) do not work on "big problems." As he suggests, this does not mean that physicists who do not work on big problems cannot make important contributions to biology. In my own field of biological oceanography, the combination of classical physics and biology is leading to rapid advancement in understanding processes that occur from micrometer to oceanwide scales. I can only hope more physical oceanographers will become interested in physical/biological coupling problems.

I chuckled when I read Austin's references to biologists and biochemists who "can't reason their way out of a paper bag" and physicists who are "scary smart." His attitude, which has been termed the arrogance of the physicists, has a long and infamous tradition.[2] No wonder biologists love to tell bad jokes about physicists. Although I appreciate the contribution of my physical oceanography colleagues, I like to treat them with a well-deserved playful irreverence. My own bad joke about physicists is that they can't be all that smart—they deal with only four fundamental forces and still can't make sense of nature.

References

1. E. P. Fischer, C. Lipson, *Thinking about Science: Max Delbrück and the Origin of Molecular Biology*, Norton, New York (1988).
2. E. Mayr, *The Growth of Biological Thought: Diversity, Evolution, and Inheritance*, Harvard U. P., Cambridge, Mass. (1982).

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Parsegian replies: The big-question versus small-question debate creates its own bad jokes. Thank goodness these three respondents perceive that I was adding big items to the could-do list and not subtracting anything from physicists' possibilities in biology.

What is lost if physics doesn't work with biology? Ask what you would lose if you knew no physics. (I'd worry I'd lose my balance riding my bike.)

Max Delbrück's work shows nicely the evolution to detail necessary to do the biology job right. In biology, sooner or later, details matter. Few students are lucky enough to learn from the likes of Jay Huebner or Eric Jakobsson. Postgraduate accessibility to good courses is worth encouraging. There's not an ideal formal curriculum. Continued lifetime learning is essential when major fields can change many times during one person's lifetime.

By the way, regrettably there was an error in my article: I meant to acknowledge conversations with, and advice and counsel from, Ralph Nossal.

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Austin replies: Seeing my comments in print in Physics Today, I was a bit embarrassed by the sophomoric tone of my writing, but I still stand by my opinion.

Biological physics certainly has pedagogical value for physicists. Like Jay Huebner, I try to incorporate as much biophysics as I can when I teach the appropriate sections of introductory physics for (in my case) life science students, and whenever possible I squeeze in biological applications when teaching the engineering sections.

It is laudable that Eric Jakobsson is trying to educate a new Renaissance scientist schooled in both physics and biology. But I must confess that I lean toward emphasizing the physics in graduate school and letting the biology develop with time, in that the amount of material to learn is daunting.

For me, Hans Dam probably strikes closest to the target: Many of us go into biological physics aiming for the big questions, and along the way we find that our training and (if we're experimentalists) our nonfear of ripping equipment apart and doing it right often leads to completely unexpected and wonderful discoveries. I like the example quoted about Max Delbrück: Had he followed Adrian Parsegian's advice, he would have buried himself in polysaccarides and never have started the genetics revolution.

But that was biology. I hope that there really are quite amazing physics lessons to be learned from biology, deep and profound. In making my comments, I simply wanted to be sure that we all don't rush off and start studying polysaccarides or busy ourselves teaching service courses as though we're some sort of trade school personnel.

I've just finished teaching a class on gravity; it's damn beautiful stuff and surely something that any educated person should appreciate. Physics is about origins and fundamentals, and—with luck—we biological physi-

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cists will have much to say in biology and physics too.

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BNL Official Explains Sources and Handling of Tritium Leaks

As interim director of Brookhaven National Laboratory, I appreciate Irwin Goodwin's continuing coverage of BNL. His comprehensive and balanced articles have enabled the greater physics community to stay up-to-date on the issues involving the lab.

However, I must point out and correct three misperceptions contained in his October story, "Peña Vows to Speed Up Lab Reforms In Wake of Political Sharpshooting" (page 86).

First, the story includes a statement that "lab officials still don't know the source of elevated levels of tritium that were detected in groundwater." That is not true. After months of exhaustive analysis, we can say with near-100% certainty that the tritiated water is slowly leaking from the 68 000-gallon pool of spent fuel in the basement of the High Flux Beam Reactor (HFBR) building.

Second, I am perplexed by Goodwin's characterization of the sequence of events—specifically his claim that when the tritium leak was found in December 1996, "it was weeks before the leak was revealed to local authorities." Although the erroneous belief that we withheld information has plagued us since last January, I believe we acted in a manner that allowed us to verify the unexpected, and apparently contradictory, results before releasing them to other parties. There was no intent on BNL's part to keep information from the authorities then, and there is none now.

Here is what actually happened. On 17 October 1996, our environmental staff took samples for the first time from the two new groundwater monitoring wells that had recently been installed just south of the HFBR. The samples were sent to the BNL testing lab for routine analysis, and the results—received on 5 December—showed a tritium level that was unexpected but not extraordinary, given our knowledge of groundwater contamination at our site: 2520 picocuries per liter in one sample from one well. That result led our environmental staff to take a new set of samples on 11 December to validate the result obtained the previous week. When the results from the new samples became available on 8 January, they showed a surprisingly high level of 44 700 pCi/L in the same well. That discovery led to an immediate resampling the next day, 9 January, and expedited overnight testing verified the high concentration of tritium. The next business day, 13 January, we notified the Department of Energy, BNL's most immediate regulatory agency. Subsequently, we notified other regulators and public officials on 16 January, BNL employees on 17 January and the news media on 18 January.

To sum up, we believe that our actions reflected a careful verification of scientifically determined results, not a deliberate delay on BNL's part, before the appropriate regulators were notified. Throughout the groundwater testing and other environmental initiatives of the past year, we have shared monitoring data with regulators and the public as soon after verification as has been feasible.

Third, I would like to correct the incorrect impression left by Goodwin's statement that our recent facilities review—initiated voluntarily by BNL—"turned up another tritium leak under a second, smaller reactor that is used for medical research." Although the proximity of this much lower level of tritium contamination to the Brookhaven Medical Research Reactor may seem to suggest that the tritium comes directly from the reactor, we have determined that neither the BMRR nor any of its systems is directly responsible. The source of the contamination appears to have been historical practices involving a portable tank and/or sump, both of which received low-level radioactive waste from medical research years ago. Currently we are monitoring this contamination further.

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Lev Shubnikov: Physics Pioneer, Landau Ally, Secret-Police Victim

Physics Today has introduced a number of little-known or forgotten Russian physicists to Western readers in recent years (see, for example, the letters about Sergei Vavilov in your December 1995 and September 1996 issues), and I would like to add yet one more: Lev Shubnikov, a pioneer in the field of low-temperature physics who was arrested by the NKVD (secret police) during Stalin's "Great Terror" and whose fate has only recently been revealed.

This gifted experimentalist started in the mid-1920s with crystal physics, and that is why Abram Ioffe (the founder and long-time director of the Leningrad Physico-Technical Institute) recommended him to Leiden University's Wander Johannes de Haas, who was looking for an expert in growing crystals.[1] In the fall of 1926, Shubnikov started working in de Haas's department at the Kamerlingh Onnes Laboratory. There, on the basis of the advances he made in growing extremely perfect monocrystals of bismuth, he discovered a subtle phenomenon that later came to be known as the Shubnikov–de Haas effect. The result was published in 1930.[2]

Right afterward, circumstances forced Shubnikov to leave The Netherlands and return to the Soviet Union. He joined the new Ukrainian Physico-Technical Institute (UPhTI) in Kharkov, and after a frustrating period of waiting to get started, he succeeded in developing the Soviet Union's first cryogenic laboratory. His lab at UPhTI quickly gained a reputation as a world-class facility for conducting low-temperature experiments. His pioneering work on superconducting alloys was later acknowledged in the term given to the mixed state of type II superconductors: the Shubnikov phase.

Together with Olga Trapeznikova, his wife and colleague, Shubnikov was the first to detect the transition into a new, antiferromagnetic phase, and, with Boris Lazarev, to directly measure the nuclear paramagnetism of solid hydrogen. When Lev Landau, who had headed the theoretical division of UPhTI since 1932, developed the theory of the layer structure of the intermediate state of a superconductor, Shubnikov was the first to experimentally test it. In return, it was Shubnikov's pioneering experiments in low-temperature physics, as well as his many discussions with Landau, that aroused Landau's interest in this field, especially in second-order phase transitions.

Theirs was a close friendship that endured in difficult situations. When Landau vigorously defended pure science against the threats of ignorant administrators and proposed splitting the institute into divisions for pure and applied research, his ally from the experimentalists' side was Shubnikov. In December 1936, Landau