THE BIOLOGICAL PHYSICIST

The Newsletter of the Division of Biological Physics of the American Physical Society

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In this Issue-

A LETTER FROM THE DBP CHAIR

Dear DBP Members:

In my letter in the June issue of *The Biological Physicist*, I noted that "Biological physics is one of the fastest growing and most vigorous areas of research in the APS." **Well, this is your chance to prove it!**

The time has come to begin thinking about the 2002 March Meeting. There are three different types of session: contributed sessions, invited symposia, and focus sessions, a mix of the first two.

The topics for the focus sessions were selected during the summer from your input (see **page 6** of this issue for a list of sessions). In order to make these sessions successful, we need you to submit contributed talks to round out these sessions.

Equally important are the contributed sessions. **The total number of your submissions to these sessions determines the number of invited symposia we are allotted in the 2003 March Meeting!** Thus, your contributions help fuel the growth of the Meeting and our field.

So keep in mind the abstract submission deadline of **5pm EST on December 7, 2001.**

Sincerely,
Mark L. Spano
Chair, Division of Biological Physics

THIS ISSUE OF
THE BIOLOGICAL PHYISICIST
IS DEDICATED TO THE MEMORY OF THOSE WHO LOST THEIR LIVES IN THE DISASTER OF SEPTEMBER 11, 2001.

Disciplined Minds: An Interview with Jeff Schmidt

by S. Bahar

Last year, Dr. Jeff Schmidt, a staff writer at Physics Today*, published a book that looks critically at education and employment in physics and other fields, focusing specifically on the treatment of graduate students, postdocs and untenured junior faculty.* Disciplined Minds *(Rowman and Littlefield, 2000) is startling in its description of the destruction of creativity within the academic workplace. The book has received much attention in the months since publication, but the reaction of Schmidt's employer was not positive. Shortly after publication of the book, Schmidt was fired from his 19-year job at* Physics Today*. Managers cited the book's provocative opening lines, in which Schmidt dramatized the way he worked on his book about work: "This book is stolen. Written in part on stolen time, that is. I felt I had no choice but to do it that way. Like millions of others who work for a living, I was giving most of my prime time to my employer…. My job, like most professional jobs, was not intellectually challenging and allowed only the most constrained creativity…. The thought of just accepting my situation seemed insane. So I began spending some office time on my own work, dumped my TV to reappropriate some of my time at home, and wrote this book."*

To date, 750 writers and scientists, including 500 APS members, have written to Physics Today *protesting Jeff Schmidt's dismissal. Some of their comments, as well as additional information about* Disciplined Minds*, can be read online at* http://disciplined-minds.com.

The editor of The Biological Physicist *talks with Jeff Schmidt about his controversial work.*

Sonya Bahar: *If you were to describe the book briefly to someone who had not yet read it, what would you say?*
Jeff Schmidt: It's about the politics of work and the battle one must fight to be an independent thinker. It focuses on the predicament of scientists and other salaried professionals.

The book shows that the paramount concern of supervisors is the political aspects of the work, over which they want exclusive control. Professional work involves decision-making in which someone's point of view, power or wealth is at stake, and so the work is an inherently political activity. Once you admit that, you can explain why there is so much job dissatisfaction and burnout. The disillusionment comes when employers succeed in dictating the political orientation of the work -- dictating who you are in your work and therefore who you are in society, because your work is your biggest project, your biggest interaction with society.

Recognizing the political nature of work also allows you to understand why professional training is so abusive. I'm talking about graduate school, which is a repressive intellectual bootcamp because it attempts to break individuals in to playing a politically subordinate role, to ready them for employment.

So the workplace is a battleground for your very identity, as is graduate school. The issue is: Are you going to pursue your own vision and stand for something, or are you going to be politically subordinate? The book shows how to do the former.

SB: *Describe the genesis of the book -- what led you to write it?*
JS: At the University of California, Irvine, it seemed like the best of my fellow physics graduate students were dropping out or being kicked out. The system seemed to favor self-centered, narrowly focused students. The others were at a disadvantage not only because their attention was divided, but also because their concerns about big-picture issues such as justice and the social role of the field caused them to stop, think and question. Their hesitation and contemplation slowed them down, tempered their enthusiasm and drew attention to their deviant priorities. That put them at a disadvantage relative to their unquestioning, gung-ho classmates.

There's about a 50 percent dropout or kickout rate for students entering PhD programs in all fields. I found that this weeding-out is not politically neutral. To put it bluntly, the programs favor ass-kissers -- students with a politically subordinate attitude -- those who will be the best servants of the status quo.

I realized that employers, too, favored people who kept their concerns about the big picture nicely under control, always in a position of secondary importance relative to the assigned work at hand. So I saw education and employment as a self-consistent, but deeply flawed, system. I wrote *Disciplined Minds* to expose the problem more completely and thereby force change.

SB: *How would you describe the reaction to the book -- from reviewers, students, faculty?*

JS: Happily, they are taking the book seriously. Reviewers appear to understand the book's key concepts, such as ideological discipline and assignable curiosity. The reviews have all been positive, but that's less significant than the simple fact that the book is getting reviewed.

I've received some very enthusiastic letters from graduate students, thanking me for writing the book, and saying, "Thank you for validating my experience and letting me know I'm not crazy."

A few faculty in physics departments and STS (science, technology and society) programs are using the book in their courses on ethics and the social responsibility of scientists. And some education, sociology and even business faculty are using the book in their classes.

It's funny that the big chain bookstores have put the book on their "Business Life" shelves, sandwiched between dress-for-success, how-to-conform type books with the opposite point of view. The first time I saw that, I jumped back, fearing some sort of matter/antimatter annihilation.

SB: *What did you anticipate* Physics Today*'s reaction would be to starting your introduction with the statement, "This book is stolen"?*
JS: Because the introduction doesn't name *Physics Today* or any other employer, I thought they would read it the same way everyone else would read it -- as an announcement of an attitude, a point of view about life in hierarchical organizations. And I'm sure they did read it that way. But they didn't like the attitude, which they called "inflammatory." And they were looking for an excuse to get rid of a workplace activist. It didn't matter that my supervisors and members of the physics community praised my work for the magazine and that I was two months ahead of schedule in meeting the annual work quota that the magazine set for me.

As I mentioned, *Disciplined Minds* argues that management's paramount concern is the political content of the work. What I learned by writing the book is that they also judge your spare-time work by its political content.

SB: *You talk about "the political aspects of the work." Could you define political in this context?*
JS: Political means affecting the distribution of power in society. The product of professional labor, for example, is political. It takes sides. The journalist's angle on a story, the accountant's bookkeeping decision, the lawyer's choice of contract language, the historian's depiction of events, the minister's sermon, the teacher's lesson, the welfare worker's determination, even the speech writer's joke -- professional work tilts one way or the other, and the way it tilts is never an accident. The work of salaried professionals is politically sensitive, because it involves decision-making in which their employers' interests are at stake.

A scientist's research, for example, can go in any of a vast number of scientifically interesting directions at every juncture. Which of these directions does the scientist deem the most interesting? Is it a direction that holds promise for the company business or for attracting the interest of a funding agency? Or is it some other direction? *Disciplined Minds* quotes the boss of a major corporate research facility that employs more than 500 PhD scientists and engineers: "You can't select problems for true scientists, much less tell them how to attack the problems. But you can make sure that they are fully informed of the needs of the company businesses that pay the bill." The scientific professionals are also fully aware that the company periodically scrutinizes the product of their labor, to decide which scientists to keep and which ones to dump.

SB: *In your chapter "Now or Never," you advocate resistance to what you describe as a "soul-battering system" -- a personal resistance with a strong political dimension. Do you have any suggestions for institutional reform?*
JS: I suggest workplace democracy at the lowest level, with each workplace innovating its own mechanisms of democracy. Workers who believe that democratic decision-making is inefficient would be free to elect someone to boss them. However, they'd have the power to unelect the boss at any time. The staff meeting would always be the highest authority in the workplace.

At *Physics Today*, I asked my colleagues, "Who is going to make the best decision when an important question arises -- the staff, with two centuries of collective experience in science journalism, or the appointed boss, with his five years of experience?" They looked over their shoulders and said I had a good point. In my case, for example, the staff was proud to have a coworker who had written a book, and certainly wouldn't have fired me for it. But it wasn't a democratic workplace.

SB: *What do you think of the tenure system?*
JS: It would be elitist to say that faculty are the only people within the physics community who should have academic freedom. I think everyone should have the protection of tenure. I certainly could have used it.

Graduate school is an intensive and protracted period of scrutiny during which the individual is pressured to conform under threat of expulsion. The tenuring process is another years-long process of scrutiny. Those who remain after the two long rounds of weeding and transformation are so intellectually and politically timid that they don't need tenure. Thus the people who need the protection of tenure don't have it, and those who have it don't need it, because they have nothing provocative to say.

SB: *But don't some people survive that process with their values intact? I personally know a number of tenured faculty who retain both great creativity and integrity.*

JS: Of course. In fact, the book has a chapter titled, "How to Survive Professional Training with Your Values Intact." Resistance is difficult, but it is possible -- and necessary. It need not be as rare as it is. Certainly, resisting the system carries some risk, but not resisting is a far deadlier course for your individual identity.

SB: *How would you compare the treatment of graduate students and postdocs to the treatment of untenured junior faculty?*
JS: In all three cases, supervisors can demand cult-like dedication, because more than money is at stake. The employees labor under the threat of having their career tickets canceled.

Junior faculty often have fewer illusions about what's going on. They may be able to figure out who's going to vote for them and who's going to vote against them at the tenure-decision meeting. There's less pretense that there is no politics involved. Graduate students are generally less aware that their attitudes and values are being scrutinized, less aware that the attitudinal assessment plays a role in deciding if they will be deemed to have passed the PhD qualification examination.

SB: *Would you have any advice for a faculty member who truly believes that a student is not qualified, on scientific grounds, to pursue a doctoral dissertation?*
JS: Such students are usually not very thrilled with the work and leave on their own. Those who are excited about the subject matter and motivated to stay should be offered a program of remedial instruction and the time to do it -- just as junior faculty are sometimes granted extra time before the tenure decision.

SB: *Is the ACLU involved in your case?*
JS: The Bill of Rights doesn't protect employees in private workplaces, and so the ACLU tends to stay away from employment cases. However, they find *Physics Today*'s actions particularly troubling, and so they are thinking about the case. But legal action is not likely to do anything. I'll get justice only if enough members of the science community announce that they are concerned about it.

SB: *You specifically focus on the physics community in your book. Do you see any major differences between physics and, say, the biological sciences, in this regard?*
JS: No. There are plenty of differences, but they aren't major. It's the similarities that are major -- similarities between all the professions, from art to law to zoology. The politics of professional training are the same, and the politics of the work itself are the same. If you let me delete one percent of the words from a transcript of people "talking shop" at a cocktail party, I can make it impossible for you to figure out what field they are in. People who go into physics hoping to escape politics are disillusioned when they find that the field is no less political than any other field. Social scientists have focused so much on the differences between the professions that they have missed the important, fundamental similarities. (If it takes a physicist to identify those, then we have a paradox!)

But it's always fun to ignore the big picture so that the differences look big. If we do that, then the cultural difference between biologists and physicists in science is like the difference between pediatricians and surgeons in medicine, or the difference between bomber pilots and fighter pilots in the Air Force. The subculture of biology is less arrogant, more open to questions, more respectful of differences, more attractive to women. There are also differences within subfields of physics itself. In the book, a physics graduate student describes the almost laughable arrogance of the high-energy-physics group at his university. There are lots of exceptions to these generalizations, and so they are more entertaining than useful.

SB: *You talk of democratically controlled workplaces, but how do you propose to deal with the hierarchy inherent in educational systems? Given that there is some "canon" of science that does need to be learned -- how to solve the Schrödinger equation, how to run a DNA sequencing gel -- what do you propose as an alternative to a hierarchical educational system?*
JS: Thirteen-year-olds teach their teachers about computers, proving in the process that there is no social hierarchy inherent in education. However, to people who assume naively that there are separate systems of education and employment, education appears to be inherently hierarchical. Our society features a single, thoroughly integrated system of education and employment. The education component is hierarchical and competitive because it is a sorting machine for employers, a gate-keeper for the corporations and academic institutions.

Learning doesn't require credentialing, ranking, grading, high-stakes testing, groveling for letters of recommendation and so on. Good teachers don't need -- or want -- the power to crush their students socially.

The Biological Physicist ***welcomes comments about this article. Do you think Schmidt's criticisms of academia are valid? Do you think they go too far? How do his comments relate to your own experiences as professional scientists? Email your thoughts to bahar@neurodyn.umsl.edu, for possible publication in a special "Letters" section of the December 2001 issue of*** **The Biological Physicist.**

MEETING UPDATES

Conference postponed!!

The **First SIAM Conference on Life Sciences,** originally scheduled for September 24-26, 2001, at the Boston Park Plaza Hotel, Boston MA, was postponed due to the tragedy of September 11th. The meeting has been rescheduled for March 6-8, 2002, at the same location. For more details, visit the conference website: http://www.siam.org/meetings/ls01/.

APS MARCH MEETING DBP FOCUS SESSIONS

The abstract deadline for the 2002 March Meeting is approaching! The deadline for receipt of abstracts is 5:00pm EST December 7, 2001. Complete abstract submission rules and instructions can be found at: http://www.aps.org/meet/abstracts/meet-abstract.html. DBP Symposia are still being selected, but the Program Chair, Bob Austin, has provided a list of DBP Focus Sessions, invited speakers, and tentative talk titles. There are a number of slots for contributed talks in each Focus Session. For any questions, please check the APS website or contact Bob Austin at austin@princeton.edu.

10.9.1 Actin Cytoskeleton and Cell Locomotion, organized by **Jonathan Katz**. Invited speakers: **Julie Theriot**, "Force Generation by Actin Polymerization: Collective Phenomena"; **Alex Mogilner**, "Entropic Depolymerization – Contraction of the Cytoskeletal Gels and its Role in Locomotion". **Anders Carlsson**, Chair.

10.9.2 Dynamics of Cardiac Fibrillation, organized by **Harold M. Hastings**. Invited speakers: **Richard A. Gray**, "Ventricular Fibrillation and Restitution in Small and Large Mammalian Hearts: Experimental Results"; **Flavio H. Fenton**, "Ventricular Fibrillation and Restitution in Small and Large Mammalian Hearts: Simulation Results". **Harold M. Hastings**, Chair.

10.9.3 Dynamical Modeling: Molecular through Behavioral Studies, organized by **Peter A. Tass**. Invited speaker: Thomas Fieseler, "Synchronization Tomography: Modeling and Exploring Complex Brain Dynamics". **Frank Moss**, Chair.

10.9.4 Structure and Dynamics of Biomolecular Materials, organized by **J. Kent Blasie**. Invited speakers: **Cyrus Safinya**, "Synchrotron X-ray Scattering studies of DNA/Lipid Complexes"; **Greg Smith**, "Neutron Scattering Studies of Biomimetic Ultrathin Films". **J. Kent Blasie**, Chair.

10.9.5 Structural and Functional Properties of Protein Folding Intermediates, organized by **Wouter D. Hoff**. Invited speakers: **Wouter D. Hoff**, "Folding and Signaling Share the Same Pathway in a Photoreceptor Protein"; **Zheng-yu Peng**, "Structural Properties of a Molten Globule State". **Wouter D. Hoff**, Chair.

10.9.6 Biological Molecules in Solvent Free or Minimal Solvent Environments: Theory, organized by **Michael T. Bowers**. Invited speakers: **Robert B. Gerber**, "Vibrational Dynamics and New Force Fields"; **Todd J. Martinez**, "Photo-Induced Cis-Trans Isomerization". **David A. Dixon**, Chair.

10.9.7 The Dynamics of Evolution, organized by **Naeem Jan**. Invited speaker: **Ayse Erzan**, "The Statics and Kinetics of the Evolution of Sex". **Mehran Kardar**, Chair.

"Seek, My Young Friend, for other ambitions": Resistance to the idea of the Macromolecule

by S. Bahar

Today, thanks to decades of effort by molecular biophysicists, the picture of a protein or other biological macromolecule as a folded chain is familiar to all scientists. In the mid 19th century, however, many of the best chemists believed there was a limit to the size of molecules. Proteins, they felt, were colloidal aggregates of smaller molecules. One of the first challenges to this prevailing view came with the pioneering work of Kekulé, in the 1850s. Many students are familiar with the apocryphal tale of Kelulé's discovery of the structure of benzene in a dream: six devils dancing in a circle, holding hands. But the idea of the carbon-carbon bond went far beyond benzene. He speculated that carbon-carbon links might lead to quite long molecules in which each atom "is connected only with one or a few neighboring atoms, just as in a chain link is connected with link."

Kekulé's idea of carbon-carbon bonds soon found its way into the realm of biological science through the work of his friend Eduard Pflüger. Working with albumin extracts, Pflüger pondered whether he might be working with "torn-off fragments of…vast molecules, which may well be as large as an entire creature". This view, of course, fit well with the vitalism of 19th century science that had replaced the "nature as clockwork" philosophy of the Enlightenment. Doubts crept in, however, when the great organic chemist Emil Fischer began to work on polypeptide synthesis. Since there could be such astonishing diversity even in small polypeptide chains, was there any need for molecules to form gigantic chains?

By the late 1800s, the thought of unraveling the chemistry of life seemed beyond reach. One scientist recalled decades later that "A very distinguished organic chemist, long since dead, said to me in the late [eighteen] eighties: 'The chemistry of the living? That is the chemistry of protoplasm; that is superchemistry; seek, my young friend, for other ambitions.' " Even Pflüger (1910) doubted that "in spite of the great exploits of Emil Fischer, the synthesis of protein will take up another century and the synthesis of living protein is hardly likely ever to succeed."

Several scientific trends fed the skepticism over the existence of "giant" (MW>5000) macromolecules. Early X-ray crystallographers were convinced that molecules could not be larger than the unit cell in their crystals, leading to the conclusion that large molecules were most unlikely to be found in nature. It was later shown that the unit cell can, in fact, be significantly smaller than a molecule, but at the turn of the 20th century the unit cell limit on molecular size was held dear by most crystallographers. Furthermore, colloid science, the study of molecular aggregates, was a "hot" field at the time, and provided an attractive alternative to long, curled-up molecular chains.

The macromolecule did have its champions, however. Years of experiments on polymers such as rubber convinced Hermann Staudinger that long-chain macromolecules did, in fact, exist in nature, held together by covalent bonds (*hauptvalenzen*) rather than secondary bonds (*nebenvalenzen*). But when he spoke on the subject at the Zurich Chemical

Society in the 1920s he was received with derision. A spectator recalled that most of the audience felt that "…it was impossible to accommodate his view in the unit cell established by X-ray analysis. All the great men present…tried in vain to convince Staudinger of the impossibility of his idea…. The stormy meeting ended with Staudinger shouting *'Hier stehe ich, ich kann nicht anders!'* [Here I stand, I cannot do otherwise!] in defiance of his critics." Despite his Martin Luther-like stand, Staudinger was curtly told by a colleague after the meeting that "molecules with more than forty carbon atoms should not exist." The same colleague later wrote him "leave the concept of large molecules well alone; organic molecules with a weight above 5000 do not exist. Purify your products, such as rubber, then they will crystallize and prove to be lower molecular substances."

But more accurate measurements of molecular weight gave the lie to this advice. Careful experiments by Svedberg using the ultracentrifuge, which he invented in 1924, allowed the precise measurements of proteins and nucleic acids with molecular weights far higher than 5000. These observations spurred crystallographers to revise their unit cell hypothesis, and support grew for the idea that large molecules did exist in nature. Svedberg, however, spun down a sidetrack when he noticed that most of the proteins he "weighed" had molecular weights that seemed to be multiples of the weight of albumin. He proposed this as a basic unit of molecular weight! Other researchers chimed in with a mathematical formula for different "protein classes", of weights n, 2n, 3n, and 6n, where n was the "unit weight" of albumin. Pauling and Niemann provided a voice of reason in the pages of *Science*, writing in 1939 that it was unlikely that Svedberg's rule would be "adhered to rigorously." They speculated that the phenomenon "stabilization of molecules of certain sizes…is to be given a biological rather than a chemical explanation – we believe that the existence of molecular weight classes of proteins is due to the retention of this protein property through the long process of the evolution of species."

As the idea of the existence of large molecules gained wide acceptance, biological scientists were quick to grasp its implications. The idea of chromosomes as large, single molecules, was immediately put forward. (Though, due to the initial difficulty of separating histones from nucleic acids, debate raged over whether the genetic material was nucleic acid or protein.) Unraveling how hereditary information was encoded in these macromolecules, was, of course, another matter….

All quotations in this article are from the first two chapters of The Path to the Double Helix: The Discovery of DNA, a fantastic – and highly recommended – book by Robert Olby (Dover, 1994).