CULTIVATING THE PHYSICS MAJOR

It was with great interest that I read Sheila Tobias' guest commentary in the February 2000 issue of AJP, especially the last section of the commentary on ''Cultivating the Physics Major.''

In the May 1997 issue of *Physics Today*, Robert Ehrlich proposed reasons for the low enrollment in the physics major. As a result of that commentary, a marketing colleague of mine and I designed a study to address the low enrollment question in a more ''scientific'' way. We surveyed over 500 students from a variety of majors, years, preparation levels, demographics, etc., to try and study the problem from the student perspective. It seemed to us that what no one had done was to ask students, lots of students, why physics hadn't been their choice of major. Based on our findings[1] we argue that efforts to recruit and retain physics majors at the undergraduate level must be systemic and holistic. While many physicists in academia focus efforts on improving instruction as the means to retain students, we argue that recruitment and retention efforts must employ several, complementary strategies to ensure success.

What Tobias calls for in her commentary is exactly what our study led us to conclude as well.[2] It seems that it would be wise for the AIP, the APS, and the AAPT to begin working collaboratively on the design of programs that can provide support to a few visionary physics departments across the country that are in dire need of programmatic change. These departments could serve as the guinea pigs for testing such systemic and holistic change. Each of these organizations has programs in place to help physics departments maintain or revitalize their physics programs in one way or another, e.g., the AAPT Revitalizing Undergraduate Physics Conferences, the AIP statistics resources on education and employment, and the APS Committee on Education's Teacher–Scientist Alliance Institutes. What departments may need, however, is a program that utilizes many of these resources to address the recruitment and retention problem in a comprehensive way specific to the needs of that particular department. What we seem to have now is a variety of programs from which a department may choose, but no systematic way to make these choices and measure the changes that a particular choice has made. This is where the professional societies could provide great help to the undergraduate physics education community. As my marketing colleague and I mention in our manuscript, faculty know what some of the problems in their departments are, but in many instances have not studied the problems in a way that would allow the construction of a testable solution.

As suggested by Tobias, let's design such an experiment (solution) and carry it out before another centennial has passed.

[1]M. K. Falbo-Kenkel and M. Shank, ''The Image of Physics: Do the Stereotypes Hold True?'' presentation given at the joint meeting of the Ohio Section of the American Physical Society and the Indiana and Southern Ohio Sections of the American Association of Physics Teachers, Ball State University, Muncie, IN, May 1998.

[2]M. Falbo and M. Shank, ''Managing Enrollment in the Physics Major,'' NKU preprint 2-98. For copies of the paper, access to the database or survey instrument, email falbo@nku.edu.

Maria Falbo
Department of Physics and Geology
Natural Science Center 438
Northern Kentucky University
Nunn Drive
Highland Heights, Kentucky 41099-1900
16 February 2000

THE THERMODYNAMIC CUBE

The paper by Stephen F. Pate, ''The thermodynamic cube: A mnemonic and learning device for students of classical thermodynamics'' [Am. J. Phys. **67** (12), 1111–1113 (1999)], refers in footnote 3 to the fact that other polyhedra could be used to represent thermodynamic relations, citing as an example a truncated octahedron. No reference was made to the paper by Ronald F. Fox, ''The thermodynamic Cuboctahedron'' [J. Chem. Educ. **53**, 441–442 (July 1976)], which gave a detailed description for such a solid.

P. E. Liley
Mechanical Engineering Department
Purdue University
1288 Mechanical Engineering Building
West Lafayette, Indiana 47907-1288
12 January 2000

WHAT IS SCIENCE?

I read with some interest the AAPT statement on the definition of science in a recent issue of the *American Journal of Physics*.[1] The statement incorporates a key point of the definition of science given by the philosopher Sir Karl Popper, who stated something along the lines that the business of science is to disprove theories, not to prove them. I think the published statements are fine as far as they go but we should be aware that there are some crucial underlying questions which are not considered in that definition.

(1) A great deal of creativity goes into deciding exactly what to observe, what data to gather. Asking the right questions is a necessary part of science and is not addressed in the definition given. Surely a lifetime spent gathering data and inventing and disproving theories about umbrellas is not science but the definition as written would not necessarily rule that (or similar undertakings) out.[2]

(2) This creativity in approach is precisely why we need to address gender and ethnic diversity questions in physics. We need to start with as many different viewpoints and ideas as possible. This involves speculation, hunches, guesses, dreams, possibly hallucinations, none of which appears in any definitions of science that I know of. From there we go on to test, eliminate wrong ideas, etc., but the creative part is critical to the success of the project. This point is frequently overlooked in discussions about the nature of the scientific enterprise.

(3) Many scientists make *no* observations, collect no data. Einstein did no significant experimental work at all that I am aware of. Science requires a community of scientists; no one scientist does it all. Science is a social project. (This is, of course, not the same thing as saying scientific knowledge is no better than knowledge from other human endeavors, a claim made by some social scientists recently.) There are problems here, too. How does one get to be a part of this social group? Who is a member of this association? When the normal credentialing process of becoming a physicist is analyzed it is nearly indistinguishable from those required for joining many religious groups.

(4) Historically it is seldom the case that scientists abandon a theory based only on contrary evidence. Usually a better theory has to be found before an old theory is considered to be overturned. In the meantime the old theory is patched together with the knowledge that it isn't really working all that well. Wrong theories can also often be useful because they suggest new things to try, new approaches, new questions. We don't always discard wrong theories outright, often we use them as stepping stones to better theories. Occasionally we actually use wrong theories even though we know and understand better theories because the wrong one is simpler and works well enough for the project at hand.

(5) How much testing should be done on a theory? Are we ever ''finished'' testing a theory? If so, how do we know enough testing has been done? Or do we test for-

ever (in which case there are no theories that are truly accepted)? What scientists seem to do is test ideas in bundles. We assume *x, y,* and *z* are true in order to test theory *w* (we assume our knowledge of electricity and magnetism is sufficient to expect that voltmeters work well enough to measure properties of electrons in a collider, for example). We may eventually test *x* while assuming *y, z,* and *r* are true but for testing *w* we act as if *x* is true for the time being.

(6) Along the same lines, an important missing element in the definition is that it is perfectly rational and acceptable to ''believe'' or accept as provisionally true, the best or most useful theory available. If we understand the history of science we can't help but think that better theories will emerge. Does that mean we should abandon what we are using now, even before it is (eventually) superseded? I don't think so.

The statement ''I know science when I see it'' serves most working scientists well enough. The statement published in the August, 1999 issue of AJP does significantly better. When we try to create even more refined definitions of science we may wish to consult philosophers of science, from whom we can learn a lot (and who have already spent considerable time thinking about most of the problems stated above). I recommend to the readers of the AJP the collection of essays *Introductory Readings in the Philosophy of Science*, edited by Klemke, Hollinger, Rudge, and Kline, which formed the foundation in my own thinking for many of the ideas expressed above.

[1]''What is Science?,'' Am. J. Phys. **67**, 659 (1999).

[2]E. D. Klemke, R. Hollinger, D. W. Rudge, and A. D. Kline (eds.), *Introductory Readings in the Philosophy of Science* (Prometheus, Amherst, NY, 1998), 3rd edition, p. 99.

Kyle Forinash
Natural Science Division
Indiana University Southeast
4201 Grant Line Road
New Albany, Indiana 47150-6405
17 August 1999

JACOBI'S IDENTITY FOR POISSON BRACKETS

Concerning my recent note[1] in this journal, it is a matter of justice to draw attention to a previous note by Epstein[2] in which he also employs the theory of infinitesimal canonical transformations to give a concise proof of Jacobi's identity for Poisson brackets. The main difference between the two papers is that Epstein includes a direct derivation of my Eq. (2). Such a derivation, however, is unnecessary owing to the invariance of Poisson brackets under canonical transformations, as I argue in my note. This makes Epstein's proof somewhat longer than mine. I thank Professor Saul T. Epstein for a letter calling my attention to his paper.

[1]N. A. Lemos, ''Short Proof of Jacobi's Identity for Poisson Brackets,'' Am. J. Phys. **68**, 88 (2000).

[2]S. T. Epstein, ''A Derivation of the Jacobi Identity in Classical Mechanics,'' Am. J. Phys. **36**, 759 (1968).

Nivaldo A. Lemos
Instituto de Física
Universidade Federal Fluminense
Av. Litorânea s/n, Boa Viagem
24210-340, Niterói, RJ, Brazil
11 April 2000

LITERATURE CITATION: GETTING WORSE BUT WHY?

Three cheers for the observations of J. D. Jackson expressed in his recent letter [''Egregious Lack of Citation of Literature,'' Am. J. Phys. **68**, 307 (2000)] regarding references and refereeing. At the end of the letter Jackson suggests that it is the responsibility of the authors to search out and cite previous published work even if it requires going to the library to find it. What strikes me is that it is becoming much less burdensome to determine what other articles may have been published on a subject and to obtain a copy of these articles. In the case of the former there are electronic databases which can save immense amounts of time when searching for articles on a specific topic. Then, when one has determined what articles are needed, it is increasingly likely that one will not have to trundle off to the library, but that the article can be found on the Internet. In the case of the *American Mathematical Monthly*, perhaps the sister journal to the *American Journal of Physics*, a hundred years worth of past issues are available to anyone in an institution subscribing to JSTOR. Hopefully something similar will be occurring in the future for AJP.

Despite all this I sense that Professor Jackson's view is that in regard to proper, even adequate referencing, things are getting worse rather than better and I would agree.

James L. Monroe
Penn State Beaver
100 University Drive
Monaca, Pennsylvania 15061-2799
21 April 2000

And a note from the editor-

The *American Journal of Physics* has only had an online edition since January 1999, but title, abstract, and author information since 1975 is available for searching at no charge. No subscription, personal or institutional, is required. Just go to the site of the online edition, *http://ojps.aip.org*, find the *American Journal of Physics*, and go to ''Search All Abstracts: 1975–Present.'' I envy the *Monthly's* century of past issues in electronic form, but I am not at all optimistic about the possibility of putting pre-1999 issues of my favorite physics journal online at any time in the near future nor about extending ''Search All Abstracts'' to the pre-1975 era. As for ''trundling off to the library,'' I find it hard to work up a great deal of sympathy for the author who occasionally has to go to that much difficulty. Is it really too much to ask of an author that he or she take an occasional trek to the library rather than sending off a manuscript with the implicit request that editors and referees do the trekking?

Robert H. Romer, *Editor*

FUSION INFORMATION ON THE WEB

R. F. Post's excellent resource letter[1] on inertially and magnetically confined fusion points out that the references listed are sparse in simple treatments. What is missing from the list are several World Wide Web pages that cover this gap and have pages that should be of interest to students. In particular, the (I)nternet (P)lasma (P)hysics (E)ducation (E)xperience (*http://ippex.pppl.gov/ippex/*) includes a virtual interactive magnetic fusion reactor and an area where students can access actual data, perform data analysis, and get feedback on their work via electronic mail. The Contemporary Physics Education Project (CPEP) has an online version of their wonderful fusion wall chart available at *http://fusedweb.pppl.gov*. Also included at this site are links to the laboratories throughout the world performing fusion research, a list of the most frequently asked questions (with answers), a fusion glossary, and an updated list of textbooks and graduate programs.

Enjoy!

[1]R. F. Post, ''Resource Letter IMCF-1: Inertially and magnetically confined fusion,'' Am. J. Phys. **68**, 105–114 (2000).

Andrew Post-Zwicker
Plasma Physics Laboratory
Princeton University
James Forrestal Campus
P.O. Box 451
Princeton, New Jersey 08543
Electronic mail: Azwicker@pppl.gov
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