

the set of possibilities available in principle in the Hilbert space. Each time the system of interest (or the memory of an apparatus, computer or nervous system) is forced into a superposition that violates environment-induced superselection rules, it will decohere on a time scale that is nearly instantaneous when the options are macroscopically distinguishable. This onset of decoherence is the apparent "collapse of the wavepacket." Thereafter each of the alternatives becomes a "matter of fact" to the observer who has recorded it: It will evolve on its own, with negligible chances of interference with the other alternatives, but with the correlation of the records with all the relevant states of the measured observables intact.

In spite of the Everett-like framework of this discussion, the picture that emerges in the end—when described from the point of view of an observer—is very much in accord with the views of Bohr.¹² A macroscopic observer will have recording and measuring devices that will behave classically. Any quantum measurement will lead to an almost instantaneous reduction of the wavepacket, so that the resulting mixture can safely be regarded as corresponding to just one unknown measurement outcome. According to the existential interpretation, what is perceived is not a "complete wavefunction of the universe" but a few characteristics of its specific branch consistent with all of the records the state of the observer happens to include. The freedom to partition the global state vector into nearly arbitrary sets of branches (present in the original work of Everett) has been constrained by the requirement that the effectively classical states should be able to persist on dynamical time scales, that is, for much longer than the decoherence time. The global wavefunction of the universe—save for the bundle of branches consistent with the identity of the observer, including in particular his or her records—is completely inaccessible. Such an observer will remember events, perceive specific "matters of fact" and agree about them with other observers.

A more extensive presentation of the issues, stimulated in part by the correspondence I have received in the wake of my PHYSICS TODAY article, can be found elsewhere.³ Reference 13 lists some of the recent papers relevant to this subject.

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3/93

How Bubbles Blow Up (Other Things, That Is)

M. M. Chaudhri (July 1992, page 15) cited the bubble-enhanced detonation of explosive crystals as evidence of very rapid and highly efficient heat transfer. Since the 1960s, however, another possible mechanism has been known to those who study cavitation-

induced corrosion of ship propellers and the like.¹ When microbubbles in the vicinity of a surface collapse, they often "cave in" asymmetrically and form supersonic jets toward or away from the surface. The tremendous pressures induced by these jets seem a more likely mechanism of detonation than collapse heat.

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8/92

CHAUDHRI REPLIES: First, I should like to correct Chris Matzner: The jet from a collapsing bubble is not always supersonic; the jet velocity very much depends on the primary shock. In one of the papers I cited in my previous letter, Frank Philip Bowden and I showed that a jet with a velocity of 120 m/sec and a localized shock of approximately 1 kilobar were associated with the collapsing bubble that caused the explosion we photographed.¹ We showed that the localized shock was too weak to initiate the explosion. Later John E. Field and I showed that the impact on an explosive single crystal of silver azide (a sensitive primary explosive) of jets of velocities of up to 450 m/sec was unable to initiate an explosion.² Having eliminated these two causes and having made further experiments with gases of different gammas (ratios of the specific heats of the gases), we concluded that the heat from the collapsing bubble was the main cause of the explosion. Furthermore, this conclusion was supported by calculations of the heat available in the bubble and of the amount transferred to the adjacent crystal surface in the time available.

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2/93

Different Angles on Errors in Textbooks

Jay M. Pasachoff suggests in his letter (July 1992, page 91) that other scientists follow his example and become involved in writing pre-college textbooks that are more correct than most present texts and urges that school

boards be pressured into making accuracy of texts a requirement for adoption. Although theoretically those would appear to be steps in the right direction, there are pitfalls and problems.

In more than 20 years of speaking at teachers' conventions and writing a column in *The Physics Teacher* discussing errors in textbooks, I have often been surprised by the large number of college teachers and scientists listed as authors or consultants in books that have many errors. Having scientists' names on the books seems to relieve the editors of the need to make real efforts to remove erroneous material, since they can claim apparent approval by the experts. Also, textbook selection committees, which usually have no way to evaluate accuracy, feel assured that the books are accurate when they see familiar names and impressive credentials listed. However, it is not safe to assume accuracy based on the involvement of experts, for a number of reasons.

The scientists, as authors or consultants, have little control over what is ultimately published. The editors have the last word and often cannot be persuaded to use the scientist's version of material they consider controversial; they are reluctant to change books that have sold in the past. Or the scientists are consulted only on a few details rather than having a general advisory role for the total publication.

Perhaps because of the perceived audience, many scientist-authors don't give their writing adequate thought. They don't seem to realize that special care is required to write for an audience with little background and that it is generally not easier than writing for fellow professionals.

Having been a consultant on a number of books for a number of publishers, I have learned to appreciate the difficulties that publishers have in finding authors, proofreaders and consultants who are knowledgeable, careful and willing to spend the necessary time on their tasks.

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7/92

The letter by Jay M. Pasachoff prompts me to write about a common error in texts on modern physics. The problem concerns the interpretation of the famous experiment by Clinton Davisson and Lester Germer. Just as Lawrence Bragg had measured x-ray wavelengths with crystal gratings, Davisson and Germer were attempt-

ing to use crystals to measure the wavelength of the electron—a new idea due to Louis de Broglie but foreshadowed by Niels Bohr's atomic theory. However, as Arnold Sommerfeld later noted, the theory underlying the experiment "at that time was still in quite an unsatisfactory state."¹ Davisson and Germer assumed considerable penetration of the electron into the crystal and so used the Bragg law to describe their results. In fact the electron-lattice interaction at the energies of the experiment is so great that it is the two-dimensional surface net that determines the angular disposition of the spots in the diffraction pattern.² As well as being correct, an approach based on this fact leads to a simpler analysis of the Davisson-Germer results than the original. It appears that most authors of modern physics texts have returned to the original article by Davisson and Germer for both experiment and interpretation. Of six modern physics texts on my bookshelf, only that by Robert L. Sproull and W. Andrew Phillips³ has a correct discussion of the Davisson-Germer results.

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8/92

High School Teachers Need College Contact

I was delighted to read Peter Lindenfeld's Opinion column "The Lonely Physics Teacher" (July 1992, page 63). Lindenfeld addresses a problem of which I have been aware for some time and makes his points clearly and strongly.

As a high school physics teacher who immigrated to the US from the European system, I have been frustrated by the lack of information flow between university physics teachers and high school physics teachers. Whenever I have asked a university teacher what background would be desirable for an incoming student, the answer has invariably been, "Nothing; we start at the beginning our own way."

At meetings and conferences concerning the Advanced Placement Physics curriculum there is an expect-

tation that high school students can absorb in two years all the physics they need to know to progress to sophomore standing when they enter college. I am trying to provide enough training for my top ten college-bound seniors to bridge the gap between eighth grade and college freshman physics in 40 weeks of their already crammed-full senior year. To set an achievable goal for that year, with sufficiently challenging material, I prepare them for the College Board's AP "B" paper in those 40 weeks—a course increasingly adopted by many of my peers at other schools. Actually I have only 35 weeks, since the AP exams are in the middle of May. It creates tremendous pressure for the teacher and the students.

I am in the fortunate position of being in close contact with faculty members at some small local colleges and some reasonably close larger universities, with whom I have had useful and supportive interactions. However, the college-level physics education establishment is not as supportive of high school teachers as the chemistry education establishment has been for many years.

My needs from the wider physics community are several, but the most urgent are:

▷ A more realistic curriculum for a one-year course for college-bound students that will be recognized as appropriate by the larger universities.

▷ More standardization of text material across the nation, with consequent standardization of the colleges' expectations of what the incoming freshman has studied in high school. Too many textbooks range from introductory to sophisticated in one course and then expect the student to carry and care for a 500-page book each day.

▷ Closer identification between college teachers of physics and high school teachers of physics, or in Lindenfeld's words, a less "remote, obscure [and] patronizing" stance on the part of the college teachers. The prevailing stance is particularly evident in the many journal articles that advocate classroom demonstrations that are ideal for the teacher with a full workshop in a garage or prep room and with the technical training to do a neat soldering job or skilled woodworking.

As one of the many women teaching physics, with limited technical training, without a fully equipped shop at my disposal, with a limited budget to purchase new demonstration equipment to meet ever changing course requirements and with family commitments outside of school hours, I look forward to more attempts like