hope that the availability of the packets will encourage teachers to serve as local resource people in community discussions of environmental problems and thus increase the interaction between scientists and the community at large.

The project has the cooperation of most of the US college science organizations and the Scientists Institute for Public Information. These organizations will be outlets for publicity and provide the expertise for careful review of packet materials.

We invite you to inquire further of the undersigned either for information or to be included on the project mailing list.

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Weak-field dilemma

I applaud the statement by Forrest Strome (August, page 9): "The time required for the work to be done by interaction of the field with the oscillating atomic dipole moment can be arbitrarily large for weak fields." That time, of course, is $h/2\mu F$, where μ is the transition dipole moment and F is the appropriate amplitude (E if electric dipole allowed, H if magnetic) of the radiation field. This fact does *not* suggest to me that we should abandon the semiclassical picture however. Why not assume that the observed excitation occurs as a two-step process? (1) Exposure to a feeble light source for a time τ excites *every* system in the ensemble to an identical superposition state, $\psi = C_1\psi_1 + C_2\psi_2$, with $C_2/C_1 \sim \tau 2\mu F/h$, followed by (2) exchange of energy among the systems of the ensemble with time-constant T_2 (transverse relaxation time). Following the second step, the properties of the ensemble are adequately described by the assumption that every system is now in a *stationary* state, a fraction $(\tau 2\mu F/h)^2$ in state ψ_2 and the remainder in ψ_1. This would make the effect excitation time $T_2 \ll h/2\mu F$, in accord with experiment.

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THE AUTHOR COMMENTS: The experimental fact that photoelectrons can be observed to be emitted without delay by the action of arbitrarily weak fields, in spite of the fact that the time required for the absorption of one quantum of energy by classical-field–atomic-dipole interaction is then arbitrarily long, does not suggest to me that the semiclassical picture be abandoned either. One point that I was trying to make was that the lure of the fuzzy-ball picture of the photon is connected with a desire on the part of many people to conserve energy in a single microscopic interaction between the field and the atom. However, the fuzzy ball is not a part of *either* quantum electrodynamics *or* the neoclassical approach of Jaynes and others. The distinguishing feature of semiclassical theory is not that it eliminates photons, in the fuzzy-ball sense, but that it does not deal with the radiation field as a quantum-mechanical system. The quanta of QED are not fuzzy balls, for, although we call them photons, suggesting a particle nature with a localization in a small volume, there is no more in the normal modes of QED than in classical fields to "explain" how an emitted photoelectron acquires its energy. Neither theory deals with the conceptual difficulty that arises when the energy density in the field is so small that the time required for the atom to "gather in" one quantum of energy at the speed of light exceeds the experimentally observed upper bound on an induction time for photoelectric emission. Macomber's suggestion of a transverse relaxation process attempts to be a little more specific about the gathering-in process, but it does not deal with this conceptual difficulty. Whatever attitude one chooses to take about microscopic energy conservation, it should be emphasized that arguments about the neoclassical theory, which were very much in evidence at the recent quantum-optics conference in Rochester, have nothing to do with that question, but revolve around the ability of the neoclassical approach to make calculations that are in good agreement with experiment.

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Neo-quantum theory

When E. T. Jaynes challenges quantum electrodynamics (October, page 17), he inevitably incurs the disapproval of Hilbert-space worshippers. And yet he is in good company, because Albert Einstein himself challenged all of quantum theory (in spite of the role that Einstein had in founding quantum theory).

Compare Einstein's well known remark that "I cannot believe that God plays dice with the universe," and now Jaynes's remark that "Physics goes forward on the shoulders of doubters, not

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believers, and I doubt that quantum electrodynamics is necessary."

According to the quantum theory, everything is quantum mechanical; and if not, then the theory must be scrapped, and replaced by a new one (perhaps a neoquantum theory?).

One thing that any theory must explain, however, is the phenomenon of superradiance—greatly enhanced *spontaneous* emission due to coherence in spontaneous radiation processes (R. H. Dicke, Phys. Rev. **93,** 99, 1954).

According to quantum theory, the basic Hilbert-space superposition principle explains superradiance neatly, as an interference effect (constructive interference, as opposed to destructive interference, which can cause extremely low spontaneous-emission rates, also observed experimentally).

However, a semiclassical theory that dispenses with such effects for the electromagnetic field would not appear to be able to explain such extremes in the observed spontaneous-emission rates, and hence one might think that Jaynes would have to do some acrobatic thinking to explain how the semiclassical radiation theory can account for some spontaneous-emission phenomena that have indeed been observed, but for which, so far, only quantum mechanics seems to supply an answer.

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Self-pacing: more caution

We wish to comment on the letter by T. R. Sandin, Julius Taylor and O. B. Okon (October, page 15). The authors, in noting the growing popularity of self-paced courses, offer a cautionary note to those conducting or planning forms of self-paced courses in which disadvantaged students will be enrolled. They point out, "for example, the extreme version of the self-paced course in which the student is given some sort of study guide and then left mostly on his own to achieve the guide's objectives will produce disastrous results for most disadvantaged students." Of course, experience has shown that the standard lecture course will also be disastrous for most of these students. Self-paced courses, *when properly designed and correctly implemented,* have demonstrated again and again their superior ability to generate activity and interest in the subject being studied.

Keller method or PSI courses are only one possibility among many for implementing self-paced instruction. They have the advantage, however, that they have been carefully conceived to modify student behavior by using principles that have evolved from years of work on the development of the reinforcement theory of learning. The Keller method is not an "extreme version" of self-pacing; in fact, it incorporates the desirable features of a self-paced course as cited by Sandin *et al.*

We offer a caution in addition to that of Sandin *et al:* Either use the procedures as put forth by Keller or his disciples[1,2,3] with no modifications, or put in the time and effort required to learn enough reinforcement theory so that you can defend the modifications you are making with reference to reinforcement theory.

The Keller method of instruction is a delicate apparatus. If you don't understand the principles of its operation, the probability that tinkering with it will improve its operation is very small.

References

1. Fred S. Keller, "Good-bye teacher ...," Journal of Applied Behavior Analysis **1,** 79 (1968).
2. Ben A. Green, Jr, "Physics teaching by the Keller Plan at MIT," Am. J. of Phys. **39,** 764 (July 1971).
3. *Proceedings of the Keller Method Workshop Conference,* (A. J. Dessler, ed) Rice University, Houston, Texas (1972).

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The Gibbs in use

In his letter (October, page 67) Hans Cassell proposes the "Gibbs" as the cgs unit of surface tension. His proposal prompts me to point out that the Gibbs is already in use as a unit of entropy (1 Gibbs = 1 cal deg^{-1}), principally by W. F. Giauque and his coworkers at Berkeley [see, for example, J. Chem. Phys. **42,** 3 (1965)]. It appears that there is no lack of desire to memorialize Gibbs.

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Physicists on coins?

The local chapter of SPS is considering a project of collecting coins that have pictures of physicists or physics equipment on them. We would appreciate any information that anybody has on lists of such coins and we would like to correspond with anybody who has been collecting them. Perhaps we will find that there are not that many coins that