an inappropriately low level.

Reasonable men can, of course, differ as to what level of power consumption should be sought to optimize the quality of life in the US. My point here is simply that the level is in no sense preordained; rather, it is something we can, and should, control by public policy decisions involving pricing and taxing power production.

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Perhaps it is to justify the AEC's costly liquid-metal fast-breeder reactor project that the article by Culler and Harms contains a number of highly debatable assumptions. The rapid price increase for uranium with increased use, and the low projected capital cost of an unproven design as well as the low operating costs for processing and disposing of ever larger stocks of highly radioactive materials, are among these [A. L. Hammond, Science **176**, 391 (1972)]. Even with such assumptions, the ore needs and the projected generating capacity curves shown do not indicate a real need for LMFBR's until the year 2000, by which time many of the extrapolations made are almost bound to be out of date.

However, the most disturbing underlying assumption is the projected electrical energy generation curve. Do we really need five times as much energy per person in the year 2000? Should we not think of how to improve the quality of life by using energy more efficiently? As physicists we pride ourselves on asking fundamental questions. The whole thesis of Culler and Harms falls flat on its face if the answer to the US energy requirements is that its growth *rate* will shortly begin to decrease.

The physics establishment may well be successful in getting billions of dollars for the LMFBR; but if the resulting reactors cannot be sold to the utility industry on a cost basis, the whole physics community will suffer the backlash.

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THE AUTHORS COMMENT: Our projection of probable future electrical capacity was taken from AEC analyses as noted in the article. Similar estimates of growth in electrical energy have recently been made by such diverse sources as the Federal Power Commission,[1] the National Petroleum Council,[2] the Cornell-NSF Workshop,[3] and the Chase Manhattan Bank.[4] All of these estimates are based largely on extrapolations of past experience, and Rosenstock observes correctly that future consumption patterns may be altered significantly by economic and political policies.

However, the suggestion that we pick a maximum power level for the country and use pricing policy to prevent further growth seems presumptuous. It is far from clear that the price elasticity of domestically used electric power is such that a "small increase in the cost of power" would significantly affect consumer use of power for "gadgets." The "Quality of life" is an elusive concept, and it has not been demonstrated, to our knowledge, that an improvement would be obtained by shifting consumption from electricity to other goods. A more rational approach might be to reduce environmental impacts where possible and to price electricity at its full social cost (including an allowance for any remaining environmental impacts). All indications are that electrical demand would then continue to rise sharply for a number of years. Meeting such power demands is likely to require both increased emphasis on efficiency of use and the development of new energy sources such as breeders.

Studies are in progress on the factors influencing our national demand (or requirements, which is by no means the same thing) for electric power, including one here at ORNL under NSF support. Hopefully, such studies will provide the basis for public debate of these issues.

References

1. US Federal Power Commission, The 1970 National Power Survey, Dec. 1971, Part 1, page I-3-15.
2. National Petroleum Council Committee, *US Energy Outlook,* Chapter 1, pages 7, 10, 11.
3. Summary Report of the Cornell Workshop on Energy and the Environment, Print, 92nd Congress, 2nd Session, 1972.
4. The Chase Manhattan Bank, *Outlook for Energy in the United States to 1988,* June 1972.

F. L. CULLER, JR
W. O. HARMS
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Blackbody fluctuations

In their comments on my letter (August, page 9) concerning their article "The Concept of the Photon," (March, page 38), Marlan Scully and Murray Sargent have stated that I was incorrect in claiming that the Einstein equation for fluctuations in the blackbody spectrum could be derived by semiclassical means, without reference to particle properties or a quantum character of the radiation field. In view of published work that demonstrates what I intended to claim, I as-

continued on page 79

letters

continued from page 15

sume that their statement was made because I failed to mention that our experimental knowledge of what is termed "photon statistics" is represented by measurements of *photoelectron* counting statistics. As is shown, for example, in the review article by L. Mandel,[1] the latter *can* be derived with no other use of quantum theory than is required to show that the probability per second of photoelectron emission is proportional to the average square of the electric field. This is, of course, the photoelectric effect, which Scully and Sargent included among the phenomena *not* requiring the use of quantum electrodynamics for their explanation. I agree that one does not obtain the "particle" term from a calculation of the energy-density fluctuations of a classical field alone; the presence of the added stochastic element involved in electron emission is essential. But if one chooses to say that the counting of a photoelectron implies the counting of a photon, one is simply demanding the microscopic conservation of energy, which I discussed in my letter, and which Scully and Sargent regard as part of "a turbulent sea of philosophical argument" that is better avoided by concentrating on the comparison of theory with experiment. I agree with them, and it is the experimental comparison of photoelectron counting statistics with the predictions of semiclassical theory that shows that fluctuation in the blackbody spectrum is also not a phenomenon requiring a quantum-electrodynamical explanation.

Reference

1. L. Mandel, "Fluctuations of Light Beams," in *Progress in Optics*, ed. E. Wolf, Vol. II, North-Holland Publishing Company (Amsterdam) 1963.

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THE AUTHORS COMMENT: We appreciate Forrest Strome's letter clarifying his previous comments on fluctuations in blackbody radiation. We objected to the implication that the "intensity fluctuations in a light beam" can be completely understood with a classical field. In his present letter he pointed out that in practice photoelectrons are what we observe and the fluctuations associated with the photoelectron distribution have both wave and particle contributions, whether calculated by a semiclassical or a fully quantized theory. This point is well taken. [Note however that this is true even when the quantum efficiency (probability of photoemission) is unity (Scully and Lamb, Phys. Rev. **179**, 368, 1969).] In principle, however, we can measure the total (macroscopic) energy in each member of an ensemble of cavities prepared in thermal equilibrium. (A discussion along these lines will be given elsewhere by Chao and Scully.) We could then reduce our data to find the average energy, $\langle E \rangle \propto n_T$, and the fluctuations ΔE. Semiclassical theory for these measurements predicts $\Delta E = n_T^2$, while the fully quantized theory predicts $\Delta E = n_T^2 + n_T$. It is this difference that we regard as providing another vote for a fully quantized over a semiclassical radiation theory.

MARLAN O. SCULLY
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University of Arizona
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Correction

October, page 64—The most recent edition of *Graduate Programs in Physics and Astronomy and Related Fields* is that of October 1971. It is available for $7.50 prepaid, $8.50 otherwise, as described in the October 1972 issue. Order from AIP, 335 East 45th St., New York, N.Y. 10017. □