computation can be offered at different terminals simultaneously.

Schwarz's article also does not bring out the tremendous investment in preparation of CAI teaching programs, perhaps tens of man years for one course. This programming cost might equal the operational cost above, so it is apparent why CAI is as yet unattractive to administrators, and why it will not become economically feasible until many installations can share the same teaching programs and split the preparation cost. Unfortunately, many of the systems mentioned in the article on which the most work has been done are "orphans" or are now extinct. The publication of CAI programs must then be in such a form that they may be easily adapted to many different computer systems.

It seems a shame that the CMI project at the Naval Academy and New York Institute of Technology was not given more prominence. This seems to be a workable and much cheaper alternative to computer supervision of a lesson sequence than the CGI technique used at Florida State.

Finally, it is a pity that workers still have not standardized their nomenclature in this field. I would like to suggest that until national educational organizations adopt official definitions, the following nomenclature be used:

Computer Assisted Learning (CAL), the generic term recommended by the Irvine conference, would include all uses of the computer in education, including *Computational Programming by Students* (CPS) as well as instructors, *Computer Interactive Simulation* (CIS) and the other modes mentioned below.

Computer Supervised Instruction (CSI) would include all situations where the progress of a student through a sequence of lesson units is controlled by the computer. *Computer Administered Instruction* (CAI) would be restricted to situations where essentially all the instructional material is presented via the computer terminal devices, *Computer Guided Instruction* (CGI) where much of the instructional material is presented via other media, and a conversational program presented at a terminal guides the student to his next assignment. *Computer Managed Instruction* (CMI), as presently used, is an off-line, nonconversational program where the student's responses to key questions, perhaps on self-punch cards, are used to guide him to the next lesson unit. I certainly would not insist on any of these definitions, particularly the acronyms, some of which are esthetically repulsive. The important thing is to be sure that when somebody mentions CAI he is referring to the teaching mode, not the proprietary program or any old use of computers by a teacher.

DONALD L. SHIRER
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Book-review rebuttal

R. G. Sachs's review of my book "The CP Puzzle: Strange Decays of the Neutral Kaon" (Academic Press, London 1968) in your August issue adopts a very different tone from that of reviews that have appeared in other journals. A reviewer is perfectly entitled to express whatever opinion he holds of a book, but if he quotes statements as examples of "important errors of principle," the author is bound to reply. I do not accept the allegation of error for any of the statements cited by Sachs.

(1) On page 78 Sachs states that "Students should be especially cautious of accepting such statements as, '. . . if all interactions are C-invariant, the K_+^0 state could decay only to states with $C = +1$ . . .' (page 4)." As he cautiously refrains from disclosing what he considers to be dangerous about this statement, one can only guess at his suspicions and hope that he does not question the correctness of my assertion. The wording differs little from Gell-Mann and Pais's original statement: "According to the postulate of rigorous CC invariance, the quantum number C is conserved in the decay; the θ_1^0 must go into a state that is even under charge-conjugation . . ." (*Phys. Rev.* **97**, 1389, 1955). [θ_1^0 is what I call K_+^0].

(2) Including his correction published on page 17 of the September issue, Sachs continues with the surprising remark: "Semileptonic states that are not eigenstates of C have apparently been overlooked by the author in this statement." Semileptonic states were not overlooked in reaching the stated conclusion, which is *not* invalidated by the fact that states like $\pi^-e^+\nu$ may not be C-eigenstates; just as the fact that a state in which a π^+ and a π^- have equal and opposite momenta along the z-axis (which

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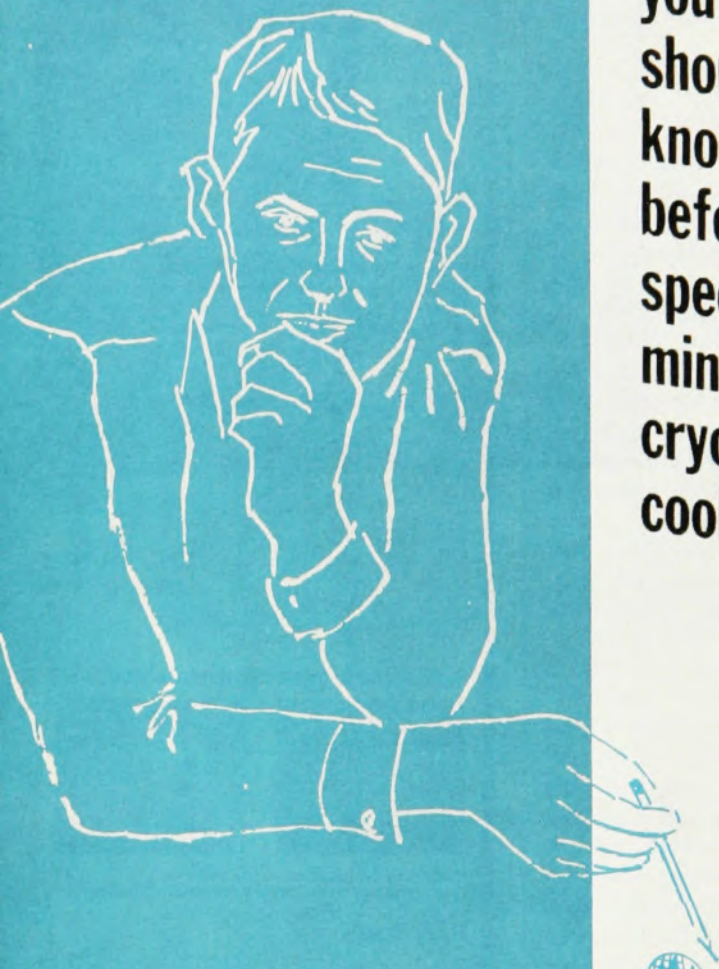

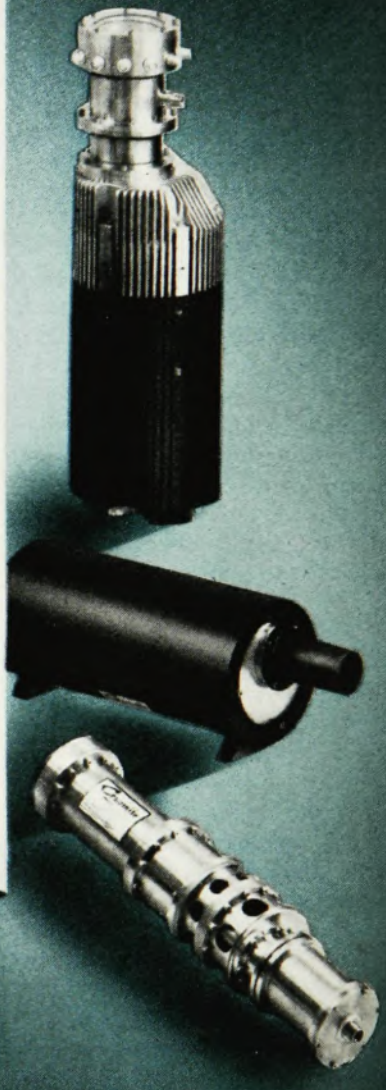

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is a possible configuration arising from tho decay of K^0 at rest) is not an eigenstate of angular momentum, does not negate the statement that angular-momentum conservation only permits final states with $J = 0$.

(3) The review continues: "Again, 'A neutral 2π state with eigenstate of C with eigenvalue $C = (-1)^l$ . . .' (page 5). An l-odd state of two neutral pions does not satisfy this condition"; although, as Sachs recognizes in his subsequently published correction, such a state is forbidden by Bose statistics! An open-minded attitude is certainly desirable in science, and perhaps I should have taken care to say "Any neutral 2π state so far known or believed to occur in Nature" instead of "A neutral 2π state" in the sentence quoted above. However, as Bohr once said, "Truth and simplicity are complementary," and in the interest of simplicity, I followed the convention of adding qualifications only when making assumptions other than those implicit in the usual theory.

(4) Sachs complains that, on page 45, I state that " . . . the phase of r has no absolute significance . . .' but "the following appears on page 47: 'In Appendix D we show that r is essentially real . . . .". Had the quotation from page 45 been completed, it would have read " . . . the phase of r has no absolute significance since it depends on the *choice* of relative phase of K^0 and $\bar{K}^0$ states." While the sentence from page 47, when quoted more fully, continues: "In Appendix D we show that r is also essentially real . . . with the Wu-Yang phase-convention." Anyone who reads page 46 between pages 45 and 47 will also find an explanation of the Wu-Yang convention, relative to which the phase of r is determined .

(5) Sachs closes his list of fundamental errors with "Another significant error that caught my eye was: 'We shall also see that the *only CP*-noninvariant *effect* which can be predicted with any assurance . . . is an interference effect' (page 77). The charge-asymmetry in the semileptonic decay of the K_L^0 is not an interference effect." It is possible to argue even with the last assertion, but I shall restrict myself to defending my own statement. The possible charge-asymmetry in "semileptonic" decays of the long-lived kaon state is discussed on page 60 of my book, following the original discussion of Lee, Oehme and Yang. The theoretical prediction for this quantity, given by Eq. (3.66), is proportional to the factor $(1 - |r|)$. Before the charge-asymmetry was measured, all that was known about this factor was that it was small and there was nothing to show that it had a nonzero value. Therefore, while *CP*-nonconservation made its occurrence possible, there was no logical ground to anticipate a nonvanishing charge asymmetry, that is, a *CP* noninvariant *effect*. Therefore, my last-quoted statement was strictly correct; not also that the dots in the quotation represent the omitted words "at the present time," in other words, the time the book was written.

After reading Sachs's review, I could not help recalling Tweedledee's observation in "Through the Looking-Glass": "If it was so, it might be; and if it were so, it would be; but as it isn't, it ain't. That's logic." I should have greatly preferred a more careful and critical review.

P. K. KABIR
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SACHS REPLIES: Apparently I did not make it clear in the review that the matters discussed in Kabir's items 1 and 2 concern just one point. Kabir's statement (item 1) " . . . the K^0 state could decay only to states with $C = +1$ . . ." is clearly contradicted by his admission (item 2) that K_+^0 could decay to $\pi^- e^+ \nu$ since there is *no* $\pi^- e^+ \nu$ state that has $C = +1$ (it cannot be an eigenstate of C). After reading the material in his book to which Kabir refers in item 3, I agree that there is no error in it. In regard to Kabir's item 4, my review stated that the discussion of phases is misleading, not that it is incorrect. Kabir's remarks in item 5 do not appear to me to change the fact that his original statement is incorrect.

These two errors in Kabir's book may not appear to justify the emphasis that was given to the matter in the text of my review. That imbalance in emphasis arose, in part at least, because of cutting of the review by the editor. Discussion of what I consider to be a most fundamental error in a book that purports to be authoritative on *CP*-invariance was eliminated from the original review, without my knowledge. This error is the final

footnote of the book's conclusion. The footnote gives a totally incorrect interpretation of the physical meaning of *P*-invariance.

ROBERT G. SACHS
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Citing the literature

In his letter (PHYSICS TODAY, Oct. 1969, page 11) Peter Borcherds calls attention to the problem of referring to unpublished work and notes that such references waste the reader's time. I would add that this is but one instance of the difficulties imposed upon the reader by the method of citing references in use by the journals that he mentions (*The Physical Review, Physical Review Letters,* and *Journal of Applied Physics*).

A much preferable way of dealing with all references is to cite them by author and date of publication and to list them alphabetically at the end of the paper. Most scientists, reading a paper in a field with which they are familiar, will recognize many papers by authors and date and will not need to interrupt their reading to look up each reference. This method can also lead to an improved writing style, because it is natural to use the authors being cited as the subject of an active verb: for example, "Smith and Jones (1965) have measured . . . "

Another dividend is that it is easy, by looking at the alphabetized bibliography, to find out if any particular reference has been included or omitted in a paper. This method of citing references is, of course, widely used in US astronomical literature (for example, *Astrophysical J, Astronomical J*).

By adopting the policy of citing papers by author and date, scientific journals improve their readability. The problem noted by Borcherds is easily solved within this framework by references in the text of the three following forms: (Smith and Jones, to be published); (Smith and Jones, in preparation); and (Smith and Jones, private communication).

DAVID MORRISON
University of Hawaii

Has science overwhelmed society?

I should like to protest the superficial and self-serving optimism of your guest editorial in the December issue, entitled "Is Your Research Moral?" I