

letters

FORTRAN: "for the birds"

Regarding the debate on FORTRAN (May 1984, page 66), a word from the inventors of the language seems very pertinent. John Backus, who led the original FORTRAN development group, spoke to an ACM SIGPLAN conference, 1-3 June 1978. When asked to comment on current trends in FORTRAN extensions, he replied,

I'm not in favor of any of them. I think conventional languages are for the birds. They're really low-level languages. They're just extensions of the von Neumann computer, and they keep our noses pressed in the dirt of dealing with individual words and computing addresses, and doing all kinds of silly things like that, things that we've picked up from programming for computers; we've built them into programming languages; we've built them into FORTRAN; we've built them into PL/I; we've built them into almost every language. The only languages that broke free from that are LISP and APL, and in my opinion they haven't gone far enough.¹

Backus remembers, of course, things that many later programmers have never learned. The first FORTRAN product was run on the IBM 704. Many of the key assumptions in the design of the language were dictated by the limitations of that old hardware. That old iron has been long gone to either museums or trash heaps, but the mathematical garbage that it spawned (such as statements like $N = N + 1$) has come to be viewed with remarkable awe. The rationale most often used is that the garbage is precious simply by virtue of the fact that physicists have built up large piles of it.

Over past centuries, until very recently, physics and mathematics were always intimately allied. The relationship fell apart as physicists have had their "noses pressed in the dirt" of digital computing. It is curious that the community of physicists has generally shown little interest in the opportunities that are now widely available for doing physical problems with a computer language that is a direct,

consistent, logical extension of classical mathematics. Your May issue is a good example of this phenomenon. An APL machine for parallel processing was announced in 1983.² Why was it overlooked in the review of advances in computers for physics?

In that same issue, Kuck and Wolfe discount the significance of APL because it currently lacks "major" use. Really now, how far can you stray from the traditions of classical science? Every advance in physics lacked major use when it started out. Shouldn't it be given a good test before being summarily dismissed?

References

1. R. L. Wexelblat, ed., *History of Programming Languages*, New York, Academic Press, 1981.
2. J. Donovan, "APL plus parallel processing simplifies numerical analysis," *Electronics Products Magazine*, 24 October 1983.

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6/84

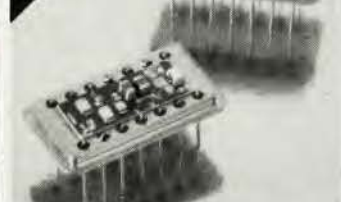
Incomplete phase separation

The news story in April (page 94) reporting on the well-deserved award of the Simon medal to D. O. Edwards stated that "In 1961 Edwards and John G. Daunt showed theoretically that even at absolute zero the dilute phase [of a phase-separated $\text{He}^3\text{-He}^4$ mixture] should contain 6% He^3 ." It might perhaps be more accurate to say that the possibility for such an incomplete phase separation at $T = 0 \text{ K}$ in $\text{He}^3\text{-He}^4$ mixtures was suggested in this paper on the basis of a semiphenomenological theory that contained at the time no known precise values for binding energies and latent heats.

I want to mention that a simultaneous and independent theoretical approach by E. G. D. Cohen and J. M. J. van Leeuwen¹ on the basis of an approximate treatment of a hard-sphere model of fermions and bosons led to the prediction of an incomplete phase separation of the fermions and bosons at $T = 0 \text{ K}$. I think it is fair to say that

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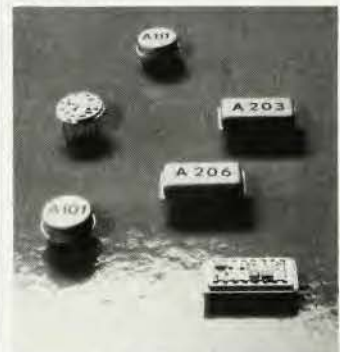
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both developments strengthened each other, in particular in the reinforcement that the theoretical model calculations provided in clarifying the question of whether an incomplete phase separation was even possible in principle.²

These remarks are made to complement those in *PHYSICS TODAY* and not to take anything away from the quality of the work for which Edwards was rightly honored.

References

1. E. G. D. Cohen and J. M. J. van Leeuwen, *Physica* **26**, 1171 (1960), **27**, 1157 (1967). C. N. Yang, VII Winterschool of Theoretical Physics in Kapacz, Wroclaw (1970). J. Lopuszanski and Z. Galasiewicz, eds.
2. D. O. Edwards, "Quantum Fluids," D. F. Brewer, ed., North-Holland Publishing Company, Amsterdam (1966), page 226.

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5/84

Earthies, airies: round two

As a devoted practitioner of "airy" science, I want to congratulate Helier J. Robinson for his fine article (March, page 24). Although I agree with most of his conclusions, there is one point I would like to dispute, at least partially. It concerns Robinson's statement that "Theoretical science is invented, and this requires creativity, for which there is no known method." I would suggest that an airy does have a *modus operandi* that has four identifiable stages: identification, digestion, recognition, formulation and solution.

The first stage is the "identification" of the problem. Problems, especially important ones, are not as a rule easily identifiable. For example, many airies who were Einstein's contemporaries at the turn of the century did not recognize the inadequacy of Newtonian mechanics. Even after the negative results of the Michelson-Morley experiment, some airies did not recognize the problem. But Einstein did, reputedly before the Michelson-Morley experiment.

After identifying the problem, there follows a period of "digestion." During this period, which may be of the order of minutes or decades, an airy mulls over (digests) the problem. The problem is similar to a puzzle with missing pieces and pieces that don't fit. When the problem fades from the airy's consciousness it recedes to the unconscious mind with the latter playing an important role in solving the puzzle. This sentiment has been expressed by Nobel laureate William Lipscomb who has been quoted as saying: "The un-

conscious mind pieces together random impressions into a continuous story. If I really want to work on a problem, I do a good deal of work at night—because then I worry about it as I go to sleep." This digestive period can be very frustrating because many times an airy may think he has solved the puzzle only to be rebuked. In this regard I am reminded of a remark by Robert S. Mulliken: "The man who woos nature for her secrets must develop enormous tolerance in seeking for ideas which may please nature, and enormous patience, self-restraint, and humility when his ideas over and over again are rejected by nature before he arrives at one to please her."

Most of the time, but not always, a solution to the problem begins with the "recognition" that the problem is "similar" to (or can be reduced to) another problem whose solution is known or that the problem is "amenable" to a known mathematical method or physical model. The key phrase here is "recognition of similarity or amenability." For example, Pierre-Gilles de Gennes of the Collège de France in 1972 "recognized" that the n -vector model in the limit $n \rightarrow 0$ and in zero magnetic field was "similar" to a self-avoiding random walk on a lattice. Because such a walk is an excellent model of a polymeric chain, this "recognition" made it possible for powerful scaling and renormalization group methods (the formulation and solution stage) to be brought to bear on the problem of polymer chain dimensions. This "recognition of similarity" between two branches of science has revolutionized the field of polymer physics in the past decade.

Two examples of "recognition of amenability" come from the field of critical phenomena. Kenneth G. Wilson, stimulated by interactions with Cornell colleagues Benjamin Widom and Michael Fisher, "recognized" that critical point phenomena were "amenable" to field-theoretic methods. Wilson's familiarity with field theory and renormalization ideas in particle physics and quantum electrodynamics provided the right knowledge base for him to develop a descriptive theory of critical phenomena in 1971. Wilson and Fisher also developed the ϵ expansion in 1972 (*PHYSICS TODAY*, March 1972, page 17). Again, this was an example of "recognition of amenability." Critical exponents can be calculated exactly in 4 dimensions by mean field (Landau) theory. In 2 or 3 dimensions the mean field theory fails, but every good airy recognizes that if a problem can be solved exactly, its range of applicability can usually be extended by perturbative methods. Wilson's and Fisher's key to solving this problem was to treat dimensionality as a continuous vari-

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