per second is much more than a factor 100—not much less, as the author thinks. (The Cyber 205 "supercomputer" of CDC even flips about ten million Ising spins each second,[3] which may be a more valid example for a comparison with the latest home computer model.) Also, the quality of the calculation may be compromised, contrary to Bak's hopes, if $32\times32\times32$ is the maximum system size for simulations of critical phenomena. If all reviewers for NSF were to share the feeling that big computers are too costly for this type of simple model, and that most of these computations should be done on a Commodore, physics in the US would not be helped. (A minor point: Bak's estimate 4.50 ± 0.02 for the critical temperature should be compared with[4] the 200-fold more accurate 4.5115 ± 0.0001, not with old results.)

Fortunately, I can live a reasonably happy life as a "parasite," to mention Bak's catchy picture, feeding off the resources of a CDC Cyber "dinosaur." A small machine makes me independent of bureaucracies but more dependent on my brain; some people around here doubt that this would be an advantage. However, since there might be more to physics than just the simulation of huge three-dimensional Ising models, a well-balanced diet of large, medium and small computers is what NSF and others should feed us with.

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THE AUTHOR COMMENTS: I am delighted about the extensive debate on computations in physics provoked by my article. Most of the letters express interesting and valid points of view, and I shall restrict myself to correcting a few misunderstandings.

The reason for hand-picking a calculation involving machine language and integer operations only was to permit a comparison of the potential capabilities of the computers on an equal footing, not that I particularly enjoy writing machine programs. It makes no sense to compare floating-point performance of a microcomputer using a BASIC interpreter and an integer-number processor with that of a mainframe computer using a compiled language and floating-point processor, as was done by David Anderson, Richard Branham, and Andrew DePristo and Stephen Elbert, when estimating a performance ratio of the order of 2000 to 10 000. Naseem Rahman and Roberto Ambrosetti arrive at roughly the same estimate when comparing the Digital Professional 350 with the IBM 3033N.

Today—as anticipated in the article—floating-point processors and compilers (see Jassby's letter) are available for several microcomputers, and we can compare the floating-point performance directly. As correctly pointed out by Donald Esterling, Anderson, and DePristo and Elbert, an IBM personal computer with an 8087 floating-point processor has about $1/10$–$1/30$ of the speed of a VAX, and about $1/100$–$1/300$ of the speed of a CDC 7600. Certainly, less expensive and more powerful microcomputers than the IBM PC (such as the Apple Mackintosh with the 68000 processor) are available today, making the gap even narrower. I agree with Jon Forrest that we are near the point where desktop computers will perform like a VAX.

Forrest also made the important observation that the main reason for high computing cost at computer centers is the large overhead; Anderson forgets this when comparing only prices of installed computer power.

DePristo and Elbert estimate (by comparing different calculations on different computers) that my Monte Carlo calculation would take 0.1–1 minute on a CDC 7600 at a cost of \$5 to 50¢. Actually, it would take two hours,[1] with 0.5×10^6 flips per second. Dietrich Stauffer argues that supercomputers may perform 10^7 steps per second. Again, if this performance is compared with that of the most sophisticated home computer, not more than a modest factor of 100 in speed is gained. Moreover, we are not restricted to a $32\times32\times32$ lattice in a modern microcomputer with about 100 K memory with room for more than a million spins.

In summary, I certainly disagree with Elbert and DePristo's statement that "it is unlikely that micros can be of much use." Already now, microcomputers are mushrooming at physics departments everywhere.

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Physics News in 1983

I, for one, applaud the publication of "Physics News in 1983" as a special section in January. As adviser of our SPS chapter, I have received a single copy of Physics News for the last several years and have been at a loss about a method to make it uniformly available to our department. When I have placed it in our student conference room, it disappears quickly, probably to a student or faculty member who "borrows" it "overnight." This way, all the faculty and SPS members have their own copies. With the number of potential borrowers significantly reduced, it should now be safer to leave our copy available.

F. W. PROSSER
The University of Kansas
1/84 *Lawrence, Kansas*

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I refer to John Croat's article on the new class of high-performance iron–rare-earth–boron permanent magnets (January, page S-20, "Physics News in 1983").

I agree with Croat that this is a major discovery in the world of permanent magnets and it will probably revolutionize the electric motor business. These magnets do not contain any cobalt, a metal considered to be critical and stragetic; instead they consist mostly of iron with some additions of light rare-earths and boron, which are abundant and less expensive materials.

However, for the completeness of the story I would like to mention the appropriate references that were left out in Croat's article. The very first data on the magnetic hardening of iron–rare-earth–metalloid alloys were announced at the Intermag meeting in Philadelphia on 21 April 1983, where coercivities higher than 23 kOe and energy products of around 10 MGOe were reported in Fe(Co)PrB(Si) samples.[1] The first official paper on these materials was published[2] on 15 October 1983, and it showed an energy product of 13 MGOe in a melt-spun sample of $Fe_{76}Pr_{16}(BSi)_8$. The same paper included transmission electron microscope data and x-ray diffraction studies that showed the presence of a new anisotropic phase with the composition $Fe_{21}R_3B$, which has a tetragonal structure[3] similar to that reported by Croat.

Our data, as well as those of Croat, were obtained on melt-spun samples. The only difference was that our samples had to be annealed at around 700 °C to obtain magnetic hardening, while those of Croat showed magnetic hardening in the as-quenched state after being slowly quenched.

Very recently, the Sumitomo Co. of Japan announced the production of the first commercial magnet made out of these materials with a record of "energy product" of 36 MGOe. Sagawa and collaborators[4] have attributed the hard magnetic properties of these magnets to the presence of a tetragonal phase that appears to be the same phase

observed in the melt-spun samples. However, the new magnets were made by sintering instead of melt-spinning. This is possible because the $Fe_{21}R_3B$ phase is not a metastable phase and therefore can be produced in as-cast samples where conventional powder techniques are used.

Similar studies are also made by Norm Koon at the Naval Research Laboratories and by Joe Becker at General Electric.

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GEORGE HADJIPANAYIS
Kansas State University
Manhattan, Kansas

1/84

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I would like to express my enthusiastic appreciation of your inclusion of the Physics News of 1983 in January. I hope this becomes an annual event.

T. R. SANDIN
North Carolina Agricultural and Technical State University
Greensboro, North Carolina

1/84

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I would like to congratulate you on publishing "Physics News in 1983" in January. While I am happy with the intention of this report, I am quite dissappointed with the Astrophysics section, for two reasons: factual inaccuracies and the breadth and quality of coverage.

First the factual inaccuracies:

▶ Pulsars: The Fastest and Farthest (page S-7)

The author states that "Correcting for interstellar dispersion the period was found to be 1.5578 ms."

Interstellar dispersion results in a dispersive delay of the pulse train and does not change the pulse-period.

The author states that the minimum age of 10^8 y is completely out of line with the measured space velocity and emission strength.

There is very little correlation between luminosity of observed pulsars and their electromagnetic age.

"The discovery of the optical counterpart confirms . . ."

To anyone following this object it is well known that there has been no independent confirmation from other groups or a substantiation of the first report by the Australian group which claimed the detection of optical pulses in the first place.

"A second rapid pulsar, called PSR 1953 + 290. . . . Its position has been determined with the very large array (VLA) radio telescope to be near the gamma-ray source G065 + 1."

Because the gamma-ray source position is known to only 1 degree accuracy, I fail to see why the arcsecond accuracy of the VLA was needed to confirm the gamma-ray pulsar coincidence. As a matter of fact, the Cornell/Palermo group searched systematically the gamma-ray error boxes with the Arecibo telescope. The VLA position was needed later only for high-accuracy pulse-timing observations.

References 7 and 8 are reversed.

▶ IRAS (page S-8)

". . . first satellite to "discover" a comet, 19830, now officially named IRAS-Alcock."

The official name is IRAS-Alcock-Araki.

Now to the coverage and the nature of coverage. Much of observational astronomy, at least these days, requires the effort of a large number of people at different wavelengths. Rare indeed are the instances when one person or a group can take complete credit for the entire discovery. This being the situation I was quite amused and annoyed to find that about 25% of the articles were one-sided either by not mentioning the work done by other people or not mentioning the controversy surrounding the interpretation of the observations. It is quite easy to see which articles belong to this category by studying the reference list. Also I did not see any reason why you thought that the Space Telescope deserved a column, for until it is launched it is not really news. The only news about the Space Telescope is the delay and the additional money that has to be spent to complete it. No mention of that was made. A similar statement can be made about the latter half of the article "Planetary Science Looks to the Future."

SHRINIVAS KULKARNI
University of California
Berkeley, California

2/84

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I am writing about the note by Steven Shore entitled "Pulsars: the Fastest and the Farthest" in "Physics News in 1983" in January (page S-7). Several statements made in this note severely misrepresent our work on millisecond pulsars.

It is stated that "The first explanation, that the neutron star was in a binary system and had 'spun up' by accreting matter,[6] [M. A. Alpar *et al.*, Nature **300**, 728 (1982)] has been ruled out since the pulsar does not appear to be in a binary system." In our model a millisecond pulsar *has been* in a binary, accreting matter from its companion, *in the past*. Whether such a pulsar is still in a binary depends on the binary parameters, in particular on the mass of the companion. Possible ways in which the pulsar can end up single were actually mentioned in the first paper Shore refers to. Further work published by my coauthors details binary accretion scenarios at the end of which one has a single spun-up pulsar [M. A. Ruderman, J. Shaham, Nature **304**, 425 (1983)].

The next paragraph states that "Another explanation is that the pulsar's magnetic fields are unusually low[7] [V. Boriakoff *et al.*, IAU Circular 3806 (1983)]." Now, the point that this pulsar's magnetic field must be very low is *not* "another explanation." It is an integral part of our scenario, which explains the coexistence of a rapid rotation rate and a low magnetic field as a natural consequence of the pulsar's being spun up by accretion. The different point of view described in this paragraph addresses the *reason* for the connection of rapid rotation rate and low magnetic field. The low magnetic field is common in both models—indeed, it is required by conventional pulsar theory, given the observed large Ω and small $d\Omega/dt$ of this pulsar.

In the next paragraph regarding the discovery of a second binary pulsar, it is not at all "most unexpected that this neutron star is in a binary system." In the binary accretion scenario, many binary systems would remain undisrupted after the accretion episode is over. In fact, in reference 6, it was shown that the previously known binary pulsars can be incorporated in the binary accretion scenario.

Further, the largeness of the *current* semimajor axis of the binary containing PSR 1953 + 290 is no argument for ruling out accretion (mass transfer) in a past era; the separation increases as the binary evolves and the mass accretion phase is completed [D. J. Helfand *et al.*, Nature **304**, 423 (1983); B. Pacynski, Nature **304**, 421 (1983); P. C. Joss & S. A. Rappoport, Nature **304**, 419 (1983)].

Finally, a minor point—my name is spelled "Alpar," not "Alphar."

ALI ALPAR
University of Illinois
Urbana, Illinois

3/84

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I read with interest "The physics of judging a fly ball" by Peter Brancazio, which appeared under Acoustics in Physics News in 1983 (January, page S-5). The final two paragraphs of the article suggest that ". . . signals trig-

gered by the motion of the head from sensors in the inner ear . . ." resulting from the ". . . sudden and rapid motion of the fielder's head as he looks upward to follow the flight of the ball off the bat may provide the sensory information that directs the player's body toward the eventual landing point." Hence, "we may be judging fly balls by ear."

I suggest that the ear is indeed an important element in the judgment of a fly ball, but for a much more simple reason than that offered by Brancazio (ironically omitted from the acoustics section). Outfielders learn to judge the trajectory of a fly ball by both visual and acoustical information. The sound made on impact is easily recognizable by all experienced players, and readily translated into an estimate of force. Brancazio notes the ability of one-eyed outfielders to learn to judge fly balls. It would be interesting to determine how readily deaf outfielders are able to compensate.

CLAIR J. CHEER
1/84 University of Rhode Island, Kingston

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As a physicist who is spending substantial time in other areas, I wish to express my appreciation for the lucid coverage of recent developments given in the special insert "Physics News in 1983" in January.

As you know, reading original research reports can be fascinating but often requires a certain amount of research by the physicist outside of the special field involved to ascertain the motivation and sequence of decisions which have led to the particular project described.

The broader view provided by the articles in the AIP special insert avoids this problem of orientation and has been very gratifying to me.

DAVID L. HILL
1/84 Southport, Connecticut

Secondary science teaching

Would you or one of your readers identify just one college or university that requires more than one one-term, three-credit course in methods for prospective secondary-school science teachers? In 40 years of examining college catalogs I have failed to find such a school.

Frankly, I believe developing and testing learning theories is a legitimate area of study. It seems the more one distances himself from the secondary-school classroom and the less that individual knows about psychology, the more expert that person becomes in classroom management.

I believe science teachers face devastating problems on both the secondary and university levels. The reign of the occult and drugs and the decline of traditional Western religious values with a belief in truth as a reality are removing the younger generation from the world as the older generation views it. The tremendous effort at remediation in even the "best" institutions may well be futile.

MARTIN D. STEWART
2/84 Norwalk, Connecticut

Consultant-fee problem

I have just received an invitation to apply for a summer position at an NSF-sponsored workshop at the California Institute of Technology that will assist in adopting the television series "The Mechanical Universe" for use in high schools.

At first I was honored, enthusiastic and happy that this summer my employment could be related to physics. Finally, someone has heeded the call to employ high-school science teachers year-round to stem the tide of teachers leaving for industry!

But alas, my enthusiasm quickly faded when I learned that the honorarium for the expert consultants is $125 per week. I am left wondering, and the readers of PHYSICS TODAY might answer me, how much would an industrial consultant charge?

ERNEST W. KUEHL JR
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Fusion criterion

Contrary to the claim, "MIT tokamak Alcator C exceeds Lawson criterion" (February, page 20), the MIT device achievement is a factor of 200 away from the *proper* Lawson criterion, provided one neglects the bremsstrahlung radiation losses. The proper Lawson criterion is strongly temperature-dependent and is given by (see J. R. McNally, *Nuclear Technology/Fusion* **2**, 9, 1982):

$$n\tau_E = \frac{24T}{\langle\sigma v\rangle_{DT}Q}\cdot\frac{1}{1-2P_B/P_N}$$

where, for Alcator C, $T = 12.5$ keV, $Q = 17\,600$ keV (neglecting neutron capture energy release in a blanket), and $\langle\sigma v\rangle = 7\times10^{-20}$ cm^3/sec. P_N is the total nuclear power release and P_B is the bremsstrahlung power loss, which exceeds P_N for $T<4$ keV!

The proper Lawson correlation of T versus $n\tau$ can be readily seen from the graph in the news story by drawing a horizontal line through the experimental point to the intersection with the appropriate $n\tau$ curves. Thus, PLT and PDX are about a factor of 100 away from the thermalized breakeven curve.

A more realistic parameter for the thermalized breakeven criterion is discussed in another paper of mine (Nucl. Sci. and Eng. **67**, 255, 1978). The effect of non-thermalized plasmas (that is, beam-driven) can be analyzed in terms of the four-factor fusion formula, $k = fn\epsilon p$; however, even here the presence of impurities worsens the approach to breakeven.

J. RAND MCNALLY JR
2/84 Oak Ridge, Tennessee

Following the MIT usage, we used the expression "Lawson criterion" in a narrow and perhaps misleading way—the lowest $n\tau$, at any temperature, that could yield breakeven in a thermalized D–T plasma. We agree that it is better to quote it as a function of temperature. In that sense, as we pointed out, Alcator C is still far from the Lawson criterion. —BMS

Size of manuscript paper

Whenever I send a manuscript to one of the journals published under the auspices of AIP, its receipt is acknowledged with the note: "In future please use paper no larger than 22×28 cm." Although these dimensions conform with the American standard size of $8\frac{1}{2}\times11$ inches, they are at variance with the standardized 21×29.7 cm^2 (A4) size that now is used throughout Europe (including Great Britain). I do not know if you receive any manuscripts from Europe where conscientious authors have cut off 1.7 cm to satisfy your wishes. If so, could you not remove the considerable burden of guilt of all those who fail to do so by relaxing the present requirement?

H. DE WAARD
University of Groningen
1/84 The Netherlands

COMMENT FROM APS: There is obviously a need to establish some standard for the size of paper used for submitted manuscripts. The American standard is used in this country for the very practical reason that office equipment, copiers, filing cabinets, folders and so on are geared to that standard. Paper sizes that do not conform require special handling, especially for copying or filing. However, provided authors restrict the dimensions of the typed area on each page, the use of European-standard paper does not cause too much of a problem.

Of rather more importance is the practice of submitting oversized figures. These are subject to serious damage either in the mails or in intra-office handling. Authors are strongly encouraged to submit original drawings or good quality glossy prints that do not exceed the normal paper sizes.

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