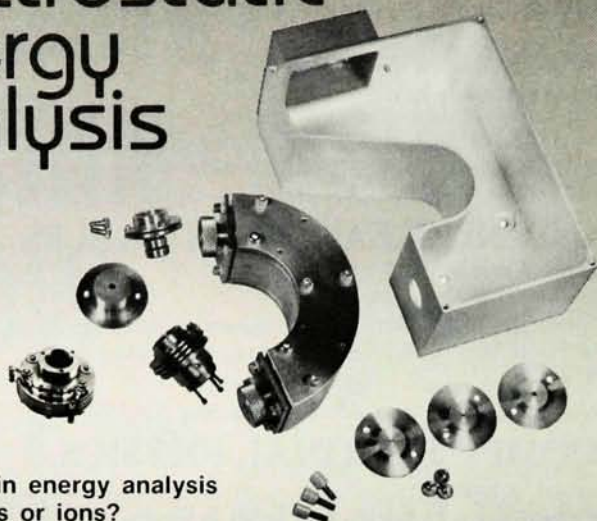


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letters

like to make an inference regarding the next steps:

► The following second step would take place in the USSR: Under an inch-tall headline "AMERICAN SCIENTISTS FOR PEACE," *Pravda* would applaud the action and announce that the Soviet people enthusiastically join the US scientists in their condemnation of American militarism. To express its solidarity and strengthen the united peace front, the Soviet Union would reinforce its nuclear defenses along its western borders and the authorities would increase their firmness in dealing with any hooligans who might attempt to sabotage this peace initiative.

► In the third step, a Charles Schwartz would try to exonerate the Soviets by pointing out that in the US undesirable things are happening as well—for example, the deficit rose again by \$2 billion—which naturally makes the USSR feel more insecure and justifies their build-up, which must be countered by fresh grass-root initiatives for arms reductions.

► In the fourth step, *Izvestia* would add full support, and so on.

There is nothing like logic and clear thinking.

V. KLEMES

National Hydrology Research Institute
7/85 Ottawa, Canada

Rotation spectra

In 1972 Tony Dorney and I published¹ a paper in which we drew attention to the near-degenerate sets of states (now called clusters) of spherical top molecules at high angular momentum, and we gave a semiclassical explanation of this behavior. Subsequently, thanks mainly to developments in laser techniques, these clusters have been observed in a number of molecules, and your July 1984 story "High-spin molecular rotation spectra are surprisingly simple" (page 17) is devoted to this subject. Unfortunately your article contains a number of historical inaccuracies, including:

► claims that the clusters were discovered in Los Alamos in the mid-1970s and that the standard theoretical approaches of the day offered no explanation

► a quotation from William Harter that there were fewer lines "than anyone had ever dreamed" in the mid-1970s

► a claim that the explanation "finally emerged from the Los Alamos computer diagonalization" in 1976

► a claim that "neither the clustering nor its orderly division by the separatrix had been anticipated"

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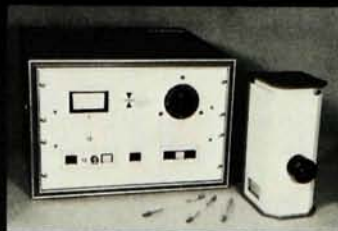
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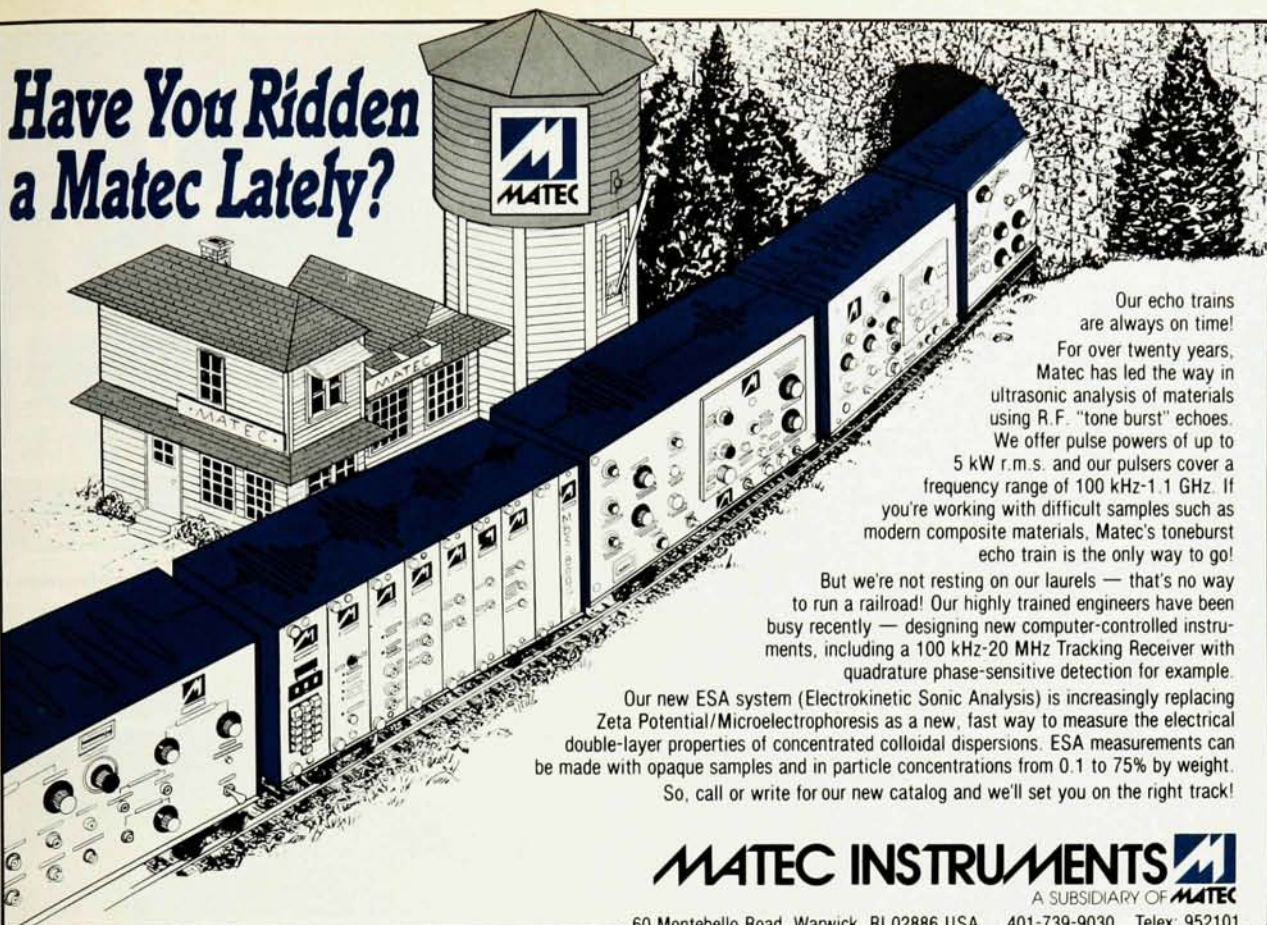
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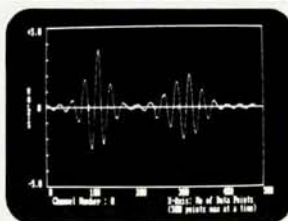
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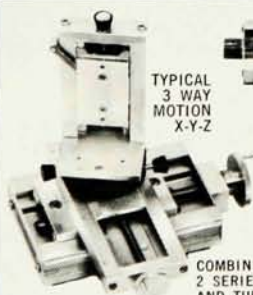
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letters

► a claim that Harter and Chris Patterson "devised the rotational energy (RE) surface."

In this four-page news story largely devoted to Harter and Patterson, there is a one-sentence mention of our work.

In a comment on a letter by Philip R. Bunker, Harter (January 1985, page 13) says, "References to the paper by Dorney and Watson are included in all our publications on the subject of clustering," but apparently this does not inhibit him from making false statements about what "anyone had ever dreamed" or what "had been anticipated" by the mid-1970s. Harter also claims that the one-sentence mention of our work "overstates" our contribution. Inasmuch as the topics listed above are treated in our paper and not acknowledged in the PHYSICS TODAY story, I disagree. I will not here discuss the other matters raised by Bunker's letter and the comments of Harter and Eric J. Heller, but I wonder why PHYSICS TODAY continues to publish Harter's claims. The opportunity should have been taken in January to correct some of the errors of the July 1984 article, rather than to allow Harter to claim that a one-sentence mention in a four-page story "overstates" the contribution of other workers.

Reference

1. J. K. G. Watson, A. J. Dorney, J. Mol. Spectrosc. **42**, 135 (1972).

J. K. G. WATSON
Herzberg Institute of Astrophysics
2/85 Ottawa, Canada

WILLIAM G. HARTER REPLIES: Bertram Schwarzschild's story implies that the semiclassical theory of angular-momentum tunneling mechanisms and resulting superfine energy-level structure was put forward¹ by Anthony J. Dorney and J. K. G. Watson's work. Because those details were not discussed by those authors the story does, therefore, overstate their contribution to that subject. However, in my reply to Philip R. Bunker (January 1985, page 13) I did not mean to downgrade Dorney and Watson's contribution to the problem of understanding fine (as opposed to superfine) structure through arguments based on classical mechanics. I apologize for this misunderstanding.

The cluster problem put forward by the Los Alamos theoreticians in 1976 focused heavily on the superfine structure. They noticed that only certain arrangements of species A_1T_1E , T_2T_1 , ET_2A_2 , $A_2T_2T_1A_1$ or T_1ET_2 were allowed in particular orders with integral superfine splitting ratios of 2:1, 1:1 and so on. These and similar puzzles

were emphasized² in an article by K. Fox, H. W. Galbraith, B. J. Krohn and J. D. Louck, and are not discussed in any previous paper known to me. The explanation of superfine structure was made later using a semiclassical action and tunneling integral method.

Nevertheless, the authors of the Los Alamos article were unaware of discussions of fine structure by Dorney and Watson and by K. R. Lea, M. J. M. Leask and W. P. Wolf. Later a referee noticed the connection in one of our first papers and we then informed Los Alamos. In my opinion this is understandable because Dorney and Watson's paper, "Forbidden rotational spectra of polyatomic molecules—Stark effects and $\Delta J=0$ transitions of T_d molecules," is not devoted to the clustering effect; indeed, there are only a few paragraphs about it. The paper by Lea and his coworkers is from another field entirely. The history is further complicated by the fact that one of the Los Alamos authors was involved in a proprietary dispute with Watson over forbidden-transition theory. (See the first footnote in Dorney and Watson's paper.) So while he may have known about their paper, the relatively obscure facts about fine structure could easily have been overlooked.

It was in this context, then, that the Los Alamos group noticed "fewer lines than had been anticipated." The experimental discovery of the prevalence of clustering and superfine structure certainly belongs to the Los Alamos group, and they were the ones who emphasized their importance. The observation of superfine, hyperfine and superhyperfine structure by the Bordé group is the next story, and it has already shown the importance of understanding the details within the clusters. It is one thing to notice an effect, but it may be quite another to notice the importance of it.

References

1. A. J. Dorney, J. K. G. Watson, J. Mol. Spectrosc. **42**, 135 (1972).
2. K. Fox, H. W. Galbraith, B. J. Krohn, J. D. Louck, Phys. Rev. A **15**, 1363 (1977).

WILLIAM G. HARTER
Georgia Institute of Technology
3/85 Athens, Georgia

Value of physics

In reading the letter about the value of a physics education by Mark Mandelkern (June, page 13), I would remark that education in physics has helped me in my mathematics teaching. I teach both mathematics and physics on the high-school level. My education in physics helps me sort through many of the wasteful and unnecessary topics given to elementary and high-school

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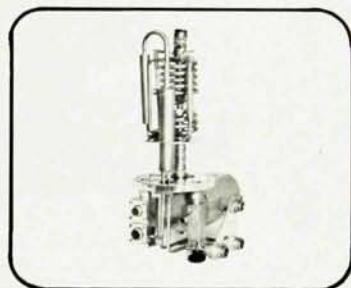
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