letters

that one is now treading on very subjective ground, as it is possible to search a multitude of interpretations of a numerical model until one is found that fits the data.

It is not true that none of these models anticipated the existence of such large voids. In fact, they are a natural outcome of the hot dark matter model, and have been extensively studied as such.[1] The news story misses the point that not only are such voids a generic prediction of models of this type, but that their size is precisely what is expected for reasonable values of the Hubble constant and the density of matter in the universe in such models. The study quotes objections to the hot dark matter model by proponents of the cold dark matter idea, primarily the excessively large autocorrelation length. That objection has been questioned in a variety of ways. It is based on equating the autocorrelation lengths of particles interacting purely gravitationally in a model in which hydrodynamic processes are known to be quite important. More generally, it is based on a reinterpretation of the model, in which galaxies form only in the collapsed pancakes; this increases the correlation strength. If one relaxes the assumption that mass traces light, one must be prepared to do so in a complicated way. It is easy to write a biasing prescription that reduces the amplitude again by judiciously choosing galaxy masses.

It is also not correct that the first structures form too late in hot dark matter models. The first nonlinearities appear at a redshift of 4 relative to the moment I describe above,[2] which is about the same as that of the first known quasistellar objects, the most ancient known nonlinearities.

I do not wish to appear to be an advocate of hot dark matter. In fact I think the question is open. All the models have positive and negative features when compared with the data and with "reasonable" theoretical interpretations. The exclusion of this model from serious discussion in your story seems to be based largely on the sociological phenomenon that it is not popular in North America. The numerical simulations can be more and more reliably interpreted when we concentrate on large-scale features, which we can be more confident are based purely on gravity. It is an objective fact that the hot dark matter model is the only one that predicted the existence of such voids. [See also the article on page 28.]

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5/86

Memories of Wheatley

Some time ago we received the sad news: John Wheatley was dead [see PHYSICS TODAY, September, page 73]. First the telegram from a former student, later the phone call from Los Alamos, and then the confusion between space and time came to my mind. I saw John, Martha and the children near the fire, Martha waiting for the bus in a cold winter, a long trip through Patagonia on a lonely dirt road, John planning one week's work for the lab—but let me put some order in this story.

Many people will be able to talk about the relevant scientific contributions made by Wheatley, and most of them will be better able than I to explain how his scientific and technical contributions have been crucial to the modern understanding of condensed matter. However, very few are in a position to talk about the Patagonian adventure in which Wheatley took the leading part.

In August 1955 a young Argentinian theoretical physicist, Jose Antonio Balseiro, supported by the Argentine Atomic Energy Commission, started a new teaching and research institute in Argentina with a small group of collaborators and students. They came to Bariloche, a small town 1100 miles from Buenos Aires at 41° latitude in a beautiful national park. Two flights and three trains per week and an almost useless dirt road were the only connections with the civilized world. Why a research and teaching institution was started in such isolation belongs to Argentinian history and is not the subject of this letter. What is important to point out is that Balseiro had a strong and clear commitment to build a research center based on experimental physics. Such a decision, which might seem trivial to people in industrialized countries, is by no means a simple task in ours. Balseiro asked for the support of Argentinian and international funding agencies. The first result of their positive response was a continuous flow of visiting professors teaching courses and advising on research projects.

In 1958, Jim Daniels came from Vancouver offering his collaboration in

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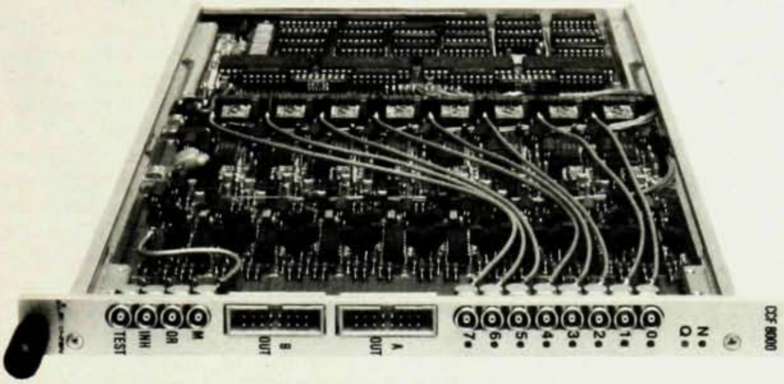

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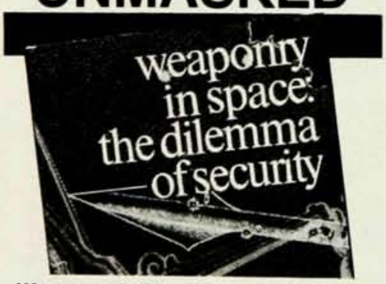

letters

building a low-temperature group. Three students were sent to Vancouver to design and start the construction of a hydrogen and a helium liquefier. After one year in Vancouver, Daniels and the students came back to Bariloche to start their pioneering work. It was Daniels who suggested inviting Wheatley to Bariloche to continue Daniels's work. That suggestion and Wheatley's acceptance were the crucial events that resulted in today's well-established low-temperature laboratory.

Wheatley came in 1962. He found little technical knowledge and support. Nobody knew about hard soldering, nor about the machining and welding of stainless steel. The compressors to be used with high-pressure hydrogen were bought in Argentina; they were modified and repaired in Bariloche to make them leak-tight to reduce operating risks, but hydrogen liquefaction is hazardous. Solid air plugs were common, as Argentinian hydrogen was bottled with up to 4% air. An air liquefier was working, but the water pressure was not high enough to operate the liquefiers to be built and the water-cooled magnet to be used for adiabatic demagnetization. We had no vacuum piping, no electronics. But when Wheatley left Bariloche $1\frac{1}{2}$ years later, 50 mK had been reached in Patagonia.

A lot had yet to be done, but Wheatley taught us how to solve problems, how to feel proud of scientific experimental work. Wheatley's outstanding work capacity and his personal approach toward the local people made possible what even today seems an impossible dream. Wheatley decided to live in Bariloche as the senior local scientists did. At that time almost nobody could afford to have a car and Wheatley did not have one. He was able to speak good Spanish after less than one month's stay. We students practiced Spanish with him, and he decided which new verb was to be used each day in the lab. I am not sure he ever understood why Argentina has had to live with the problems that delay our progress—neither do I—but he was able to show that hard work leads to success. Progress has been made but there is much to be done in his low-temperature laboratory, here down south in Patagonia.

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5/86

Molecular rotation spectra

The July 1984 Search and Discovery story on high-spin molecular rovibration spectra (page 17) has generated a continuing controversy (PHYSICS TODAY, January 1985, page 13; June 1985, page 15; March 1986, page 156), which I have followed with interest and some concern, as I number the disputants on both sides among my personal friends. Several of the letters have referred to the infrared spectra of SF_6 and UF_6 obtained at Los Alamos in the mid-1970s. Perhaps I can contribute a clarifying historical note without hopelessly exacerbating the dispute.

Highly resolved infrared spectra of flow-cooled UF_6 and statically cooled SF_6 were obtained here at Los Alamos beginning in 1974. In assigning and analyzing these spectra, we used the formalism of J. Moret-Bailly,[1] for his vector coupling coefficients were available for total angular momentum quantum numbers J up to 21. After C. D. Cantrell and Harold W. Galbraith[2] derived the correct spin-statistical weights for octahedral XY_6 molecules, we were able to assign the fine structure of the SF_6 stretching fundamental.[3] (G. A. Natanson has recently called my attention to an earlier treatment[4] of XY_6 spin-statistical weights.) In 1975 Burton J. Krohn[5] calculated vector coupling coefficients for values of J up to 100, enabling us to carry the assignments to higher J and to identify, in diode laser spectra recorded by E. D. Hinkley in 1970, the transitions that coincide with CO_2 laser lines.[6]

The clustering of the rovibrational levels had, of course, been noticed during this work: It is implicit in Moret-Bailly's[1] and Krohn's[5] tables of coupling coefficients. Hence it was somewhat misleading for William G. Harter (as quoted in the PHYSICS TODAY story) to suggest that we were surprised to see far fewer fine-structure lines "than anyone had ever dreamed," when we were in fact observing precisely the number of lines, and in the exact patterns, predicted by these theoretical treatments.

Although we were initially unaware of their paper, Anthony J. Dorney and J. K. G. Watson[7] were the first to call attention to clustering of the energy levels of tetrahedral molecules, and they gave the correct classical interpretation of the effect. Later the mathematical properties of clusters, including their internal structure, were investigated here.[8] It remained for Harter and Chris Patterson to account quantitatively for both the positions of the clustered lines (using 3-j coefficients) and the nature of the patterns within the clusters (by quantum tunneling).

It is unfortunate that some claims made for cluster theory have confused the role it played in our work. Our SF_6