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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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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

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and UF_6 spectroscopy, involving as it did *heavy* spherical tops, was the first to reveal the clustering phenomenon fully, but this does not constitute discovery of the effect. Clustering was, in fact, of peripheral concern to our interest in identifying the SF_6 transitions that couple with CO_2 laser radiation, and it was not used in our analysis.

This is no way diminishes Dorney and Watson's discovery and explanation of the effect, nor Harter and Patterson's elegant further development of the theory. Their work has provided a useful semiclassical explanation for *why* the energy levels of spherical tops behave as they do, and it is currently finding application to the hyperfine structures of high-J rotational states that are now being observed as resolving power increases still further.

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Quantum Hall neglect

Five years have elapsed since Klaus von Klitzing's discovery of the quantized Hall effect (see PHYSICS TODAY, June 1981, page 17; July 1983, page 19; December 1985, page 17), yet little progress has been reported about its theoretical understanding. With all the experts who have had a go at it, one may now well assume that a theory of the effect cannot be a simple reduction to standard quantum methods. While this magazine is not the place to argue the pros and cons of alternatives, it does seem appropriate to call attention to a slight omission in your reporting.

Your reports of June 1981 and July