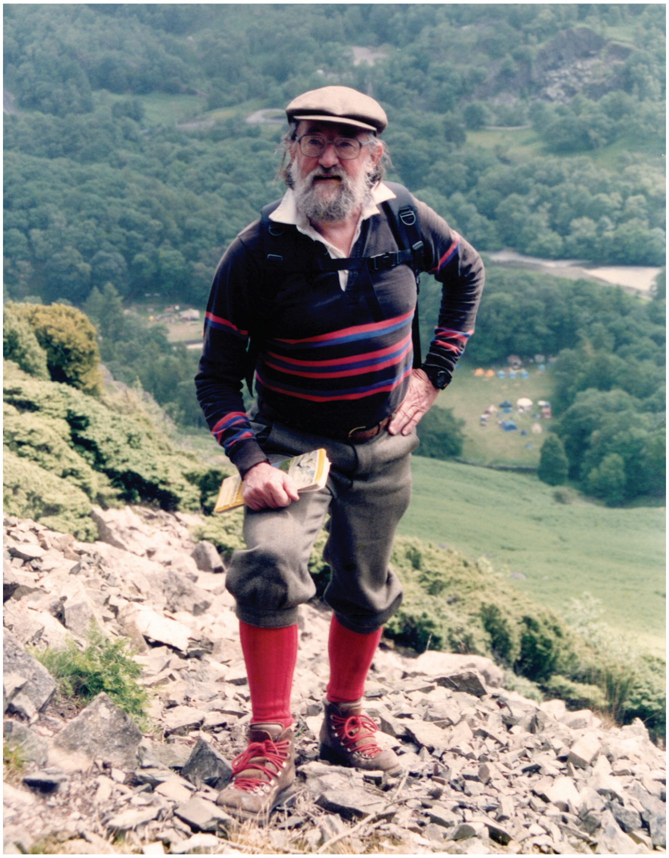

Above Borrowdale in the English Lake District. Photo by Mary Reppy.

# John D. Reppy

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# Reflections on 65 Years of Helium Research

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autobiography, superfluid helium, supersolids, persistent currents

## Abstract

In this autobiographical article, I discuss a number of topics that have absorbed my interest over the years and illustrate how advances in experimental technique, such as the superfluid gyroscope and torsional oscillators, were entwined with expanding knowledge of the properties of helium.

## EARLY YEARS

In writing this autobiographical article on my scientific career, I have to look back on more than sixty years of research on helium. Beyond this, my family connection to helium started with my father's occupation as a US naval officer. Much of his career was involved with the lighter-than-air branch of US naval aviation, where helium was the lifting gas.

I was born in 1931 while my father was stationed at the US Naval Air Station at Lakehurst, New Jersey. My father's assignments alternated, at least once a year, between duty at sea and the lighter-than-air branch of naval aviation. This resulted in frequent moves up and down the East and West Coasts and once out to Hawaii, when my father's ship was based at Pearl Harbor just before World War II. By the time I graduated from high school, I had attended 14 different schools, an experience that certainly affected my development and promoted a degree of independence and self-reliance.

This itinerant life came to an end in 1943, when my father was transferred from a desk job in Washington, D.C., to the western Pacific, where the action was. My mother settled the family in Haddam Neck, a rural location on the Connecticut River, where her father owned a vacation home. By my early teens, I had developed an interest in herpetology and geology. Haddam Neck was a wonderful place to pursue these interests—there were lots of snakes to catch and a number of abandoned quarries where I collected mineral specimens. It was also in these quarries that I first started rock climbing, an interest that I have continued to pursue ever since.

I graduated from high school in 1950 and immediately enrolled in summer school at the University of Connecticut (UConn). An important event that summer was the beginning of the war in Korea. During my years as an undergraduate, I came close to being drafted for military service and even was called for a preinduction physical exam. As an undergraduate, I had enrolled in the UConn ROTC (Reserve Officers' Training Corp) Program, which protected me from immediate induction into the army, but I would have been automatically inducted following graduation. Fortunately for me, the commanding officer of the ROTC unit decided I was more valuable to the nation as a mathematician or physicist than as an infantry officer, and he allowed me to drop out of the ROTC program without notifying my draft board.

## CHOOSING PHYSICS

While at UConn, I was able to satisfy my interests in several areas of science including geology, chemistry, physics, and also mathematics. In the end, I majored in mathematics. My career in physics started almost by accident. In my sophomore year, Professor Charles Reynolds, my instructor in a thermodynamics course, asked if anyone might like to have a job in his lab. I jumped at the opportunity, and this set the course for my scientific career. It was a lot of fun working in his lab. The group was small, consisting of one graduate student and one undergrad—me. I made myself useful by learning how to operate and repair the Collins helium liquefier, as well as mastering the basic machine shop skills required for the construction of a simple low temperature apparatus. I also had the good fortune to meet David Lee, who had come to UConn after being mustered out of the army. He has been a close friend and professional colleague now for 65 years.

A couple of years later, Dave and I ended up in the Yale Low Temperature group. Reynolds had been a graduate student of C.T. Lane, who was head of the Yale Low Temperature group, and had maintained close contact with Lane and the Yale group. Thus, there was an easy path for us from UConn to the Yale physics department. It did not hurt that Henry Fairbank, the junior faculty member of the Yale Low Temperature group, was also director for admissions to the physics department. Dave preceded me at Yale by two years and was supervised by Fairbank for his Ph.D. When I arrived in 1956, I was taken under the wing of C.T. Lane.

At that time, superfluid liquid $^4$He was understood in terms of the two-fluid model. In this model, superfluid helium was envisioned as two interpenetrating components, one with the properties of a normal fluid and the other, the superfluid component, with zero viscosity and zero entropy. This classical model was successful in describing most of the known superfluid phenomena, such as the creeping film, the fountain effect, and second sound. A notable exception was the behavior of rotating superfluid. In early experiments with rotating superfluid helium, the entire fluid appeared to be rotating as a solid body under conditions of steady rotation. This result was in contrast to the expectation based on the two-fluid model, where the superfluid component was expected to remain at rest. A resolution of this problem attracted the interest of several of the world's low temperature groups.

At Yale, Lane and his group had been studying the effect of rotation on second sound. Also at Yale, but in the chemistry department, Lars Onsager, the local theory genius, was thinking about rotating superfluid. According to Lane, Onsager appeared one day in the lab and announced that the solution to the problem of the rotating superfluid was "the fine structure," and then left without further explanation. Lane and his group were puzzled as to what the "fine structure constant" might have to do with liquid helium. Of course, Onsager was alluding to a fine structure of quantized vortices in the rotating superfluid. Earlier, Onsager had made a comment at a scientific meeting that the flow of the superfluid was "presumably quantized in units of $h/m$," again without further explanation. At this time, Richard Feynman hit upon these same ideas, but unlike the enigmatic Onsager, he gave a detailed exposition of his thinking (1).

The intrusion of quantum mechanics into the understanding of the helium superfluid had its roots in the early suggestion by Fritz London (2) that, based on the idea of Bose–Einstein condensation, the superfluid was a manifestation of a macroscopic quantum state. This was a paradigm shift in the understanding, and it gave those working in the field years of interesting employment working out all the implications.

The most important experiments of the time were those of Henry Hall and William "Joe" Vinen at the University of Cambridge, England. In a joint effort, Hall & Vinen (3) demonstrated with second sound that the rotating superfluid developed a linear structure along the axis of rotation. This structure was then an array of quantized vortex lines: Onsager's "fine structure." In a second experiment, Vinen (4) demonstrated that the vortex field of superfluid around a fine wire was quantized in the expected units of $h/m$.

For my Ph.D. dissertation, Lane suggested that I look into using the magnetic suspension developed by Jesse Beams. I copied Beams's design and developed an apparatus in which a container of liquid helium could be suspended and rotated in vacuum. The magnetic suspension was almost frictionless, so free rotation could go on for hours. The real limitation was a slow warming of the helium sample after the exchange gas used for cooling was pumped out. This apparatus proved ideal for measuring the angular momentum of the helium sample. In general, we found that the entire helium sample would come into solid body rotation; occasionally, however, we found that only the normal fluid would acquire solid body rotation while the superfluid fraction remained at rest, just the situation that would have been predicted by the classical two-fluid model. This metastable state was observed to continue for several thousand seconds, during which the only change in rotation speed was a slowing due to the slow warming of the sample and the formation of additional normal fluid.

Although we graduate students spent long hours in the lab, including most evenings—we always said that the best data came after midnight—all was not work. For instance, Dave Lee and I took off from the lab in the summer of 1958 for a monthlong climbing trip out west. We were accompanied by my roommate, Frank Carey, and another friend, Gil Young, who supplied the car


**Figure 1**

Yale Low Temperature group in June 1960. Shown left to right are David Sandiford, Bob Meservey, Ed Walker, unknown, Henry Fairbank, Mike Crooks, C.T. Lane, Jim Vignos, Myron Strongin, John Reppy, David Caplin, and George Zimmerman. Photo provided by the Zimmerman Estate.

for our trip. We climbed in the Black Hills of South Dakota and then went to Wyoming, where we visited Devils Tower National Monument. I have a photo of our group on the top of the Tower posed next to a sign dragged up by a previous climber, which stated "No Climbing Beyond this Point." The four of us went on to Grand Teton National Park. After a couple of ascents, Dave and Gil headed back east, and Frank and I got a ride down to Colorado with Layton Kor, who later became a famous climber. We then spent a week in Rocky Mountain National Park, climbing several routes on Longs Peak and making our most notable achievement, the first ascent of the spectacular "Flying Buttress" on Mt. Meeker next to Longs Peak.

That fall, after returning to New Haven, I met my future wife, Judith Voris, at a party given by several graduate student members of the Low Temperature group. Judith made an immense impression on me with her intelligence and tales of canoeing in the wilds of the Quetico with her younger brother. By early winter, I had asked her to marry me, and by midwinter she agreed. More than 60 years later, we are still together.

I finished my dissertation in 1960. **Figure 1** shows the Yale Low Temperature group in June of that year.

The following January, I moved to Oxford in the United Kingdom to take up a National Science Foundation (NSF) Fellowship working for Nicholas Kurti. Kurti's interests were in the area of nuclear cooling. This involved high-field, water-cooled magnets, which were designed and built by Martin Wood, who later founded Oxford Instruments. These magnets were impressive, cooled by huge volumes of distilled water that were pumped through the magnets using 2-inch-diameter fire hoses and powered by DC current from a large generator originally designed to power streetcars in the city of Bristol. On one occasion, the magnet being used by Douglas Brewer in the lab next to mine shorted and exploded—it was most spectacular. I can't say that I accomplished anything in the way of significant physics during that year, but there were many interesting experiences. I learned to love Indian food and was appalled by the strength of the English class system. Having recently been lowly graduate students, it was shocking for Judith and me to be deferred to by our neighbors in the small village where we were living outside of Oxford. Of course, after a year one got used to it.

Meanwhile, back at Yale, Lane had arranged an appointment for me as an assistant professor. I was quite anxious to return because I had an idea for how to make an observation of superfluid persistent currents utilizing the magnetic suspension apparatus that I had built for my Ph.D. dissertation. So I declined Kurti's offer to support me for additional year in his lab and instead returned to New Haven in January 1962.

During the year I was in Oxford, David Depatie, the graduate student following me with Lane, had solved the problem of how to construct superfluid tight thin-walled containers of magnesium. Using his technique, we constructed a cell consisting of a stack of mica discs separated by narrow spacings encapsulated in a thin-walled magnesium can. This cell replaced the glass containers I had used for my thesis experiments.

With this apparatus, Depatie and I were able to make the first clear observations of persistent currents in liquid $^4$He (5). Our technique was to rotate the cell filled with liquid helium at a constant rate above the superfluid transition temperature. The narrow spacing of the mica discs ensured that the fluid rotated as a solid body with the container. The cell was then cooled through the transition temperature while maintaining the steady rotation. When the desired temperature was reached, the cell was brought to rest with a magnetic brake, and the exchange gas used for thermal contact between the cell and an external helium bath was pumped away while the cell was held at rest. When the cell was brought to rest, the normal fluid would also come to rest within a short time, and one might be left with a rotating frictionless superfluid forming a persistent current. Next, with the cell suspended in vacuum, the brake was released. After waiting a period of time, we heated the cell and its contents back above the transition. The angular momentum that had been stored in the persistent current was then distributed to the entire cell and its contents, and the cell would be set into rotation. By this technique, we determined the angular momentum for a number of persistent currents formed at different temperatures. We found that as the temperature was lowered, the magnitude of the stored angular momentum increased. Thus far, our results were in keeping with the classical two-fluid model, in which the magnitude of the currents was constrained by a superfluid critical velocity that increased with decreasing temperature.

Our next experiments yielded an unexpected and surprising result (6). We formed a persistent current near the superfluid transition, where we knew that the stored angular momentum would be small. Then, rather than analyzing the angular momentum by heating from this temperature, we held the cell at rest and continued cooling to a lower temperature before measuring the stored angular momentum. We were surprised to find that the angular momentum of the persistent current had increased with reduced temperature, even though the container was at rest. Further measurements showed that this increase in angular momentum as the temperature was lowered tracked the known increase in the superfluid density with decreasing temperature. Since the persistent current angular momentum is proportional to the product of the superfluid density and the superfluid velocity, our result implied that it is the velocity field, given by the phase gradients of the superfluid macroscopic wave function, that is conserved with changing temperature, not the persistent current angular momentum.

I realized that persistent current observations had the potential to be used as a tool for the determination of the temperature dependence of the superfluid density and critical velocities. The problem was to develop a method for persistent current measurements that did not require the destruction of the current. The solution turned out to be the development of a superfluid gyroscope (7). A suggestion by a Yale undergrad who was taking flying lessons at the time set me on the path to the development of this instrument. He mentioned that what I was looking for seemed similar to the "rate of turn" meter in an aircraft. The successful design consisted of a torus containing the persistent current and supported by a tungsten fiber across the inside diameter of the torus; in effect this was a gyroscope in which the rotating element was a frictionless superfluid.

## ON TO CORNELL

About this time, I received an offer from the Cornell University Physics Department of a tenured position as an associate professor. Yale countered with promotion to associate professor, but untenured. The choice was easy: I accepted the Cornell offer and we moved to Ithaca in the winter of 1966.

When we arrived in Ithaca, we were faced with a recent 20-inch snowfall. Barbara Holcomb, the wife of Don Holcomb, who was the lab director at the time, organized some of the local children to clear the driveway to the house that Judith and I had rented. This was the first instance of the warm and welcoming atmosphere of the Cornell Physics Department. It was in stark contrast to the situation at Yale where, as a graduate student and then faculty member, I was invited to the home of a faculty member only once, by Henry Fairbank, when he was leaving Yale for Duke University and thought that we might be interested in buying his house. Looking back, I attribute the atmosphere in the Cornell department in large part to the influence of Hans Bethe, who set the intellectual and ethical standards for the department. I would liken him to the munificent spirit of Sarastro in the Mozart opera *The Magic Flute*.

After I moved to Cornell, Jim Clow, a student under my supervision back at Yale, continued to make measurements with our superfluid gyroscope. He was able to make precise measurements of the superfluid density as the system approached lambda transition temperature from below (8). In many systems, various properties of the system can be characterized by critical exponents. In the case of our measurements, the critical exponent for the superfluid density was determined to be $0.67 \pm 0.03$. Brian Josephson was quick to point out that this exponent was connected to the exponent characterizing the heat capacity at the lambda point (9). At that time, based on the seminal lambda point heat capacity measurements of Buckingham & Fairbank (10), the heat capacity in the neighborhood of the transition was believed to be logarithmic, implying an exponent of zero. Following the scaling relationship between these two exponents, a two-thirds value for the superfluid exponent would imply zero for the heat capacity exponent. A short time later, Tyson & Douglass (11) made a measurement of the superfluid density near the lambda point. Their measurement employed the torsional oscillator technique of Andronikashvili and produced a value for the superfluid exponent of $0.666 \pm 0.006$.

This satisfactory situation did not last long, as very much improved heat capacity measurements by Guenter Ahlers (12) showed that the heat capacity of $^4$He in the neighborhood of the lambda transition was not logarithmic but was characterized by a small negative exponent. The implication for the superfluid density was that its critical exponent should be slightly larger than two-thirds. Today, the accepted values for these exponents are $-0.015 \pm 0.002$ for the heat capacity exponent below the transition and $0.6749 \pm 0.0073$ for the superfluid density exponent, in keeping with the Josephson scaling relation (13).

At Cornell, the subject of critical phenomena was beginning to blossom. The theoretical effort was led by Michael Fisher and Ben Widom, to be joined by Ken Wilson. On the experimental side, Watt Webb, a professor in the Applied Physics Department was interested in applying light scattering to the problem of critical behavior of $^3$He–$^4$He mixtures. At the time, only the rough outline of the phase separation diagram as a function of mixture concentration and temperature was known. It appeared that the phase separation diagram had a rounded peak, similar to that of a liquid gas system, at the critical point. If this was the case, then strong light scattering might be expected as the peak in the mixture phase separation diagram was approached.

It occurred to me that one might easily make a preliminary determination of the shape of the phase diagram for the mixture by a capacitive measurement of the dielectric constant of the two phases. During the 1966–67 academic year, Dave Lee was away on sabbatical leave at Brookhaven National Laboratory, where he was pursuing his dream of Pomeranchuk cooling. Back at Cornell, one of his students, Erlend Graf, was somewhat at loose ends and suggested that he would like to work on the dielectric determination of the $^3$He–$^4$He phase diagram. In this he had been inspired by a lecture on critical phenomena by Ben Widom of the Cornell Chemistry Department.

Dave had no objections, and the experiment went forward. As it turned out, the determination of the shape of the phase diagram was an easy experiment and, to our surprise, the peak of the diagram was not the expected rounded critical point but rather an apex formed by two relatively straight lines (14). In these measurements, we also determined the lambda superfluid transition line, a more difficult measurement. We found that the lambda line terminated at the apex of the phase transition diagram to form what has become known as the tricritical point.

At the same time, two other graduate students, Ray Henkel and Eric Smith were successful in observing persistent currents in a thin $^4$He film (15). A remarkable finding was that, if the thickness of a film was increased by condensation of additional $^4$He atoms onto the film while holding the temperature constant, the angular momentum carried by a persistent current in the film would increase reversibly in proportion to the thickness of the film. This was a striking demonstration of the robust nature of the phase gradient structure of the superfluid macroscopic quantum state.

When Dave Lee returned to Cornell from sabbatical leave, he continued his work on the Pomeranchuk cooling project, leading to the discovery in 1972 of superfluidity in liquid $^3$He at a transition temperature near 2 mK. For this discovery, Dave Lee, Doug Osheroff (Dave's student on the project), and Bob Richardson (who had joined the group by this time) were awarded the 1996 Nobel Prize in Physics.

The discovery of a new superfluid opened up a wide range of experiments to be done. I set out to develop a method for making precise measurements of the viscosity and superfluid density of $^3$HE. I considered several different designs and finally settled on a high-frequency version of the Andronikashvili torsional oscillator. The large viscosity of liquid $^3$He allowed us to operate the oscillator at kilohertz frequencies. Our final design consisted of a thin pancake region containing the $^3$He sample mounted on a hollow Be–Cu torsion rod. The experimental sample was thermally connected by the fluid in the hollow torsion rod to a larger reservoir of liquid, which was in turn cooled by a thermal link to Dave Lee's Pomeranchuk cell. The torsional motion of the cell was driven and detected capacitively. The intrinsic dissipation in the empty cell was minimal, resulting in a high $Q$ value on the order of $10^6$. The high $Q$ made it relatively easy to achieve a high level of frequency stability when the oscillator was driven in a feedback loop. An analysis of the hydrodynamics of the fluid between parallel oscillating planes, the approximate geometry of our pancake cell, allowed us to calculate the superfluid density and the viscosity from the period shift and dissipation data obtained as a function of temperature (16).

Several years later, my rotating cryostat was modified to enable superfluid $^3$He experiments. Peter Gammel with the help of Henry Hall, who was a visitor to Cornell at the time, applied the high $Q$ oscillator technique to the problem of the detection of persistent currents in the $^3$He superfluid (17). This turned out to be a difficult experiment, in part due to the small critical velocities in the A and B phases. The idea of the experiment was to create a persistent current by rotation of the entire cryostat. The angular momentum of the persistent current was measured by tipping the torsional oscillator back and forth. The angular momentum vector was forced to follow the tipping, and this motion then produced an oscillating torque at right angles to the motion. This torque was expected to drive another high $Q$ tipping mode. The difficulty with this method was the delicate balancing of the torsional oscillator structure to ensure sufficient overlap of these two high $Q$ tipping modes. After considerable effort, Peter was successful in observing $^3$He persistent currents.

During the course of these measurements, we serendipitously happened on a much more powerful method for detecting persistent current in the superfluid phases of $^3$He (18). We found that the dissipation of the torsional mode was highly sensitive to any velocity difference between the super- and normal fluids liquid in the $^3$He sample. In the absence of a persistent current, a minimum in the dissipation was observed when the cryostat was not rotating, and both the normal fluid and superfluid were at rest. If the cryostat was then rotated, a velocity difference, eventually limited by a critical velocity, would develop between the normal fluid, rotating with the cryostat, and the superfluid component, which remained at rest. Under these conditions the dissipation of the torsional mode was observed to increase in a parabolic fashion with increasing rotational speed. With a persistent current present, however, the minimum of dissipation occurred at the rotational velocity where the normal fluid velocity matched that of the superfluid persistent current.

In parallel with the $^3$He work, I continued experiments with $^4$He, with an emphasis on critical behavior as a function of dimensionality and disorder. The torsional oscillator technique proved to be an invaluable tool for these experiments. Two of the most important measurements involved two-dimensional (2D) systems, where a thin $^4$He superfluid film was adsorbed on 2D substrates. For the first experiment, David Bishop constructed a torsional oscillator cell containing a "jelly-roll" of mylar film that provided approximately 0.4 m$^2$ of surface area. We found that the superfluid transition for $^4$He films adsorbed on the mylar substrate consisted of a sharp drop to zero of the superfluid areal density. Accompanying this jump in the superfluid response was a sharp peak in the torsional oscillator dissipation (19).

As it turned out, this experiment was a realization of the Kosterlitz–Thouless (K-T) theory (20) for the phase transition in a 2D system with two degrees of freedom for the order parameter. The K-T transition was an early example of what is now known as a topological phase transition. In the case of superfluid helium, the two degrees of freedom are the superfluid velocity and the density; thus, a superfluid film meets the requirements for K-T theory to apply. An important feature of the theory is an exact prediction in terms of fundamental constants for the ratio between the magnitude of the jump in the superfluid areal density and the transition temperature. A comparison to our experimental data for a range of transition temperatures was found to be in remarkable agreement with the K-T prediction. In 2016, Kosterlitz, Thouless, and Haldane were awarded the Nobel Prize for their theoretical contributions to the theory of topological phase transitions, including the K-T transition in $^4$He films.

Another significant contribution to the physics of 2D films was the unexpected observation of a re-entrant superfluid phase for $^4$He adsorbed on the second layer of a graphite substrate. This discovery was made by Paul Crowell in the course of his Ph.D. thesis research (21).

My last contribution to the field of critical phenomena came in the solution of a long-standing problem involving the superfluid transition for liquid $^4$He in porous Vycor glass. During my first sabbatical visit to Manchester University in 1972–73, Henry Hall and I, along with a graduate student, Keith Kiewiet, measured the superfluid density for helium liquid in the randomly connected fine pores of the porous Vycor glass (22). The specific heat had been measured in this system years before and found to exhibit a rounded lambda peak at nearly the same temperature as the bulk fluid. At the time, the rounding was attributed to finite size effects resulting from the confinement of the helium to the 10-nm diameter pores of the Vycor. It was then a surprise when our measurements showed a sharp transition in superfluid density which followed the same, nearly two-thirds, power-law as seen for the bulk superfluid. The amplitude of the superfluid response was reduced below that of a bulk sample of the same size by a factor of approximately 20.

The mystery lay in the fact that the superfluid transition in Vycor did not coincide with the peak in the heat capacity for liquid $^4$He in Vycor, but fell, with no visible indication of the superfluid transition, on a smoothly rising slope of the specific heat curve at a point several tenths of a degree below the rounded peak. The apparent absence of any signature in the specific heat at the superfluid transition remained a conundrum for several years. Eventually, Pierre Hohenberg pointed out that a theoretical explanation was provided in his paper with Eric Siggia and their colleagues (23). According to this theory, the magnitude of the specific heat anomaly associated with the fluctuations at the superfluid transition would scale as the inverse amplitude to the third power of the superfluid density response. Because the superfluid response was much reduced below that of the bulk fluid, we should expect the heat capacity peak to be reduced by a factor of about $10^{-4}$, which is well below the resolution of heat capacity experiments of the time. Thus, the situation remained for many years with a prediction that was out of reach experimentally.

After about twenty-five years, however, the development of SQUID (superconducting quantum interference device)-based magnetic thermometry had improved the state of the art to the point where the lambda anomaly in the specific heat associated with the He–Vycor transition might be resolved. In 1998, my student Geoffrey Zassenhaus, using SQUID-based magnetic thermometry, succeeded in resolving, barely above the noise level, a small peak in the heat capacity coincident with the Vycor superfluid transition (24). Fortunately, we hit on a new way to measure the heat capacity, which allowed a large reduction in the noise in our measurements. Instead of applying a heat pulse of known energy and measuring the change in temperature, as in a conventional heat capacity measurement, we applied a small steady heat to the sample. In this approach, the rate of increase in temperature of the sample and its magnetic thermometer will be inversely proportional to the heat capacity. The changing magnetization of the thermometer was monitored through a Faraday voltage induced in a coil wrapped around the magnetic thermometer. The current created by the induced voltage was monitored by a SQUID and limited by a small micro-ohm resister in series with the coil. This method provided a twenty-fold reduction in the noise in the heat capacity measurement to the one part in $10^6$ and permitted a clear resolution of the lambda anomaly at the $^4$He–Vycor transition.

## RETIREMENT AND THE SUPERSOLID DEBATE, 2004–PRESENT

In 2004, I retired after 44 years in the Cornell Physics Department. I had the intention of resuscitating my climbing career. As fate would have it, however, I got sucked back into the lab for another ten years by the emerging problem of the supersolid.

The idea of some sort of superfluid behavior in solid $^4$He had been around since the late 1970s, when the possibility of Bose–Einstein condensation in solid $^4$He was discussed by a number of theorists (25, 26, 27). The first positive indications were reported in 2004 by E. Kim & M.H.W. Chan (28). In a series of experiments with solid $^4$He samples contained in the torsion bob of a torsional oscillator, an anomalous drop in the oscillator period at temperatures below 200 mK was observed. These results were quickly confirmed in a number of labs throughout the world and were interpreted as evidence of a superfluid-like decoupling of a portion of the solid moment of inertia from the torsional oscillator. This discovery created considerable excitement in the low temperature community. Over time, however, an alternate explanation for the Kim–Chan observations has emerged. The gradual unraveling of the attractive concept of a supersolid occupied the last decade of my research career.

Although officially retired, I retained a lab and NSF support. Following the initial reports by Kim & Chan, Sophie Rittner, my last graduate student, and I undertook to repeat the Kim & Chan measurements. Our experiments confirmed their observations (29). We also found that annealing our solid samples could reduce the size of the supersolid signal, suggesting that disorder in the solid was key to the observed phenomenon. Following this idea, we constructed a cell in which a high degree of disorder could be created in a solid sample while at low temperature. When the disorder was increased by inducing plastic flow in the sample, the supersolid signal showed a sizable increase, as indicated by the period shift of the oscillator. In contrast to the expectation for a superfluid-like response, however, no shift in period was observed at the lowest temperature; the increase in period after deformation of the solid occurred at the high-temperature end. Thus, by 2010 we had concluded that period shifts observed in this apparatus did not arise from a superfluid-like phenomenon but required another explanation (30).

Following a separate line, we undertook to investigate further Kim & Chan's blocked annulus experiment (31), which seemed to give strong support to the idea of superflow in the supersolid state. This experiment was performed in two stages. First, a supersolid signal was observed for an unblocked annulus. In a second oscillator of the same design, the annular channel was blocked by a partition. As expected, the supersolid signal was much reduced because any superflow around the annulus would be blocked by the partition. At the time, this was considered convincing evidence for superflow in the supersolid.

In 2008, along with Wansuk Choi, a graduate student visiting from Kim's group in Korea, I decided to repeat the blocked annulus experiment in a slightly more elaborate version that would also serve to demonstrate the potential flow of the superfluid component of the supersolid. As in the Kim–Chan experiment, we constructed a cylindrical cell with a circular annular channel, but in addition we cut a channel along a diameter connecting two sides of the annulus, thus forming a D-shaped flow path for the supersolid. When the annular channel was blocked at one point by a partition, the signal would be reduced in magnitude, but flow should still be possible around the D-shaped path. The final step would be to block the D path with a partition placed in the cross channel at the center of the cell. This final block would prevent any flow around the D-shaped loop and reduce any superfluid signal to almost zero. At each stage, we checked the system with superfluid liquid helium and found that all was as expected; i.e., when the last block was put in place, the superfluid response disappeared.

When the experiment was repeated with the presumed $^4$He supersolid, the response up to the point of introducing the block in the cross channel followed that of the liquid superfluid, with a sizable reduction in signal size with the first block in the annular channel, but not to zero, as supersolid flow could still take place around the D-shaped loop. In contrast to the liquid results, however, placing the block in the cross channel had no effect on the size of the supersolid signal. Thus, it was clear that potential flow was not taking place in the solid sample.

These results were presented as a poster at the International Symposium on Quantum Fluids and Solids at Northwestern University in the summer of 2009 and in a later publication (32). Our poster attracted little attention, mostly I suppose because I could not, at that time, explain the contradiction with the Kim–Chan result. Today, these experimental results can be understood as a consequence of an increase, described by Day & Beamish (33), in the shear modulus of solid $^4$He occurring at temperatures below 200 mK. The elastic stiffness of a torsion bob containing a sample of solid $^4$He is determined in part by the stiffness of the solid within. Thus, a change in the elastic stiffness or shear modulus of the solid $^4$He will produce a change in the period of the oscillator. In the case of the blocked annulus experiment, the solid $^4$He in the annulus served to couple the inner and outer sections of the oscillator. The block placed across the annular channel provided a much stronger coupling between the parts of the oscillator than did the solid helium and led to a large reduction in the influence of changes in the solid shear modulus on the period of the oscillator. A block placed at the center of the cell on the axis of rotation will block any superflow but leave the elastic properties of the torsion bob almost unchanged. These experiments, which cast doubt on the existence of the supersolid, resulted in my being deemed "The Supersolid's Nemesis" in an article in *Nature* (34).

With time, it has become clear that most, if not all, of the oscillator period shifts ascribed to the supersolid in fact arise from changes in the shear modulus of the solid contained in the structure of the torsion bob. A key to the experimental demonstration of the influence of changes in the solid shear modulus is the fact that the relative magnitude of the effect is frequency dependent, tending to zero as the frequency is reduced. This is in contrast to what is observed with a real superfluid, in which the relative response, or superfluid density, is frequency independent. A series of measurements with multimode torsional oscillators, confirmed that most, if not all, of the observed period shifts were due to changes in the shear modulus of the solid (35). Thus, the beautiful dream of the supersolid dies.

## AFTERWORD

Over the course of my career, I have been fortunate to receive a number of honors, most significant being the 1981 Fritz London Award, followed by election to the National Academy of Sciences in 1994. In 2000, I received the NASA Distinguished Public Service Medal for my role in NASA's Microgravity Research Program.

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