Berkeley Experiments on Superfluid Macroscopic Quantum Effects

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Abstract. This paper provides a brief history of the evolution of the Berkeley experiments on macroscopic quantum effects in superfluid helium. The narrative follows the evolution of the experiments proceeding from the detection of single vortex lines to vortex photography to quantized circulation in ^{3}He to Josephson effects and superfluid gyroscopes in both ^{4}He and ^{3}He.

Keywords: macroscopic quantum effects, superfluid helium
PACS: 67.40.-w, 67.57.z, 01.65.+g

INTRODUCTION

I am greatly honored to receive the 2005 London Memorial Prize. The body of work that is mentioned in my citation was done collaboratively with a group of graduate students and postdocs who have shared this research adventure with me since 1969. We all share the honor of this prize and thank the London Prize Committee for selecting our work. Although my many co-authors over the years have contributed greatly to our various projects, there are several people that I wish to single out.

Michael Sanders was my thesis advisor. He introduced me to the topic of superfluidity and by example taught me a great deal about how to select experimental problems. Gary Williams was my first graduate student. He taught me more than I taught him and has been a continuing source of knowledge on all things superfluid. I especially thank him for organizing my nomination for this prize. Greg Swift and Jim Eisenstein shared lab space together and were my first students to study superfluid ^{3}He. Jim showed me how little I knew about formal quantum theory and Greg taught me how far I needed to go to be a more creative inventor. Jukka Pekola encouraged me to investigate ^{3}He flow in single submicron apertures. His frequent statement of "Let's do it!" was inspiring. Stefano Vitale has been a close friend, sailing buddy, and final arbiter of everything related to SQUIDs, gravity, signals and noise. I wish he were a more frequent visitor to Berkeley. My longest collaboration has been with Seamus Davis, one of the co-recipients of this year's prize. First as a graduate student, then a postdoc and finally a faculty colleague, Seamus taught me a great deal about organization and how to keep my mouth shut. He is a fine addition to the Cornell faculty and a great loss to me and Berkeley. More recently, Kostya Penanen let me realize again how much more I have to learn about physics and how lacking the US undergraduate education is compared to the FSU. Finally I wish to mention my two present students, Emile Hoskinson and Yuki Sato. Emile reminds me often how careful analysis can tease information out of data. His careful analysis is an excellent example of experimental detective work. Yuki is still rather new in the group but he gives me great hope for the future of low temperature physics. As long as young scientists like Yuki and Emile continue to probe Nature below 2 K, I am confident that the intriguing discoveries that have persisted in low temperature physics will continue unabated.

Since all the work cited in the London Prize is well documented in the literature, I will focus here on the personal narration of how some of the work came to fruition. For those who have no interest in the human or historical side of physics I encourage them to turn to the next paper in these proceedings. For those unfamiliar with our work, please visit my group web site (http://www.physics.berkeley.edu/research/packard/) and click on the list of publications. Since this is not a research manuscript I make no attempt to cite the literature broadly. Those important references are all included in the Berkeley papers which are included as footnotes.

ENTERING LOW TEMPERATURE PHYSICS

Surprisingly this story begins with the construction of a sailboat, something that occupied much of my time while I was an undergraduate. Having this many-faceted project to occupy my time gave me a convenient excuse to be absent from many of my physics classes, resulting of course in rather mediocre grades. Consequently upon graduation my first graduate experience was not in a diversified research department. Leaving the boat behind

CP850, *Low Temperature Physics: 24th International Conference on Low Temperature Physics;*
edited by Y. Takano, S. P. Hershfield, S. O. Hill, P. J. Hirschfeld, and A. M. Goldman

and focusing on classes let me attain rather high grades in an physics masters degree program. I used that improved academic record to reapply to graduate schools and achieved considerably greater acceptance than in the first round. In 1966 I chose to attend the University of Michigan for two reasons. 1. My wife Roseanne could also get a graduate degree there in her specialty and 2. They had eliminated the requirement of a comprehensive exam for the beginning grad students, substituting instead the requirement of good grades in all graduate courses. That seemed to me a system more logical than the usual prelim exams given by many other schools. It seemed unwise to me then (and now) to ask physics students to perform under enormous pressure on an exam that might mean more than their previous four years of classroom examinations.

At Michigan, my ignorance of most research fields made all the research offerings seem uninteresting. I had the good fortune to hear Michael Sanders describe a recent visit to the Soviet Union to attend a low temperature physics conference. His description of that trip made me feel he would be a good choice as an advisor. However the graduate student grapevine said that his group was at that time so large that he was not taking on new students.

Believing this to be true, I first approached another professor to inquire about research. That professor asked me about the phase transition in my grades from mediocre to the highest in my previous graduate school. I gave the feeble (but true) excuse of my building a sailboat and missing many classes. He offered me a project that involved classified research, a sphere of endeavor I did not care to join considering the political climate of the Vietnam War.

Following the dictum, “If you don’t fail once a day you aren’t trying hard enough.” I decided to go see Sanders and apply for a position. To my surprise Sander’s first question was, “Aren’t you the guy who built a boat?” When I answered yes he said, “Well if you can build a boat you can probably do the experiments I have in mind.” At the time I didn’t understand the basis for that judgment but upon graduation three years later I realized that my thesis was really just another boat project. All the skills it takes to do one (planning, design, materials acquisition, multitasking, fabrication, testing, and perseverance) are equally applicable to the other.

DETECTION OF SINGLE QUANTUM VORTICES

The problem Mike assigned (I note that it took three years to get up the nerve to call him Mike rather than Professor Sanders) was to use ion trapping to detect the creation of vortices in rotating superfluid helium. My first task was to build a rotating cryostat. This was enjoyable for me because it involved lots of machining and designing. I assumed that it all had to be built cheaply and looking back I don’t think the entire investment was more than $500. I took special delight in showing the 2 m tall spinning apparatus to passing grad students. The rotating electrical contacts were a series of about 18 concentric troughs of mercury into which dipped small nickel blades. This worked quite well except that when the speed of the contacts grew too large the mercury was thrown out of the troughs onto the floor. I just scooped it up, cleaned off the dust, and poured it back in. Contrast that action to the reaction on a campus today of a small mercury spill!

Sander’s suggestion was to detect the presence of vortices using the fact that electrons form bubble states (with diameter near 3 nm) which can become trapped on quantized vortices. This was well known from work in R.J. Donnelly’s group in Chicago and Careri’s group in Rome. Presumably the amount of charge trapped in a sample of rotating superfluid should be proportional to the number of vortices present. If we could measure the amount of charge trapped in the liquid we would have a measure of how many vortices were present. We hoped to see a staircase increase in the trapped charge as the number of vortices changed from 0 to 1 to 2, etc.

The primary technical challenge in the experiment was that one could only trap about 100 electrons on a centimeter length of vortex line. This was about one order of magnitude below the resolution of the best charge measuring amplifiers of the day. We hoped that the electrons could be brought out through the free surface of the rotating helium and then accelerated in the vapor to create charge multiplication due to collisions. However we knew from previous work that electron bubbles cannot be brought through the liquid surface in nonrotating helium (there is an effective work function). Would the presence of vortices change that?

To our delight and relief we found that the electrons emerged easily out the top of the vortices. We should have published that result but I was too focused on moving ahead with the experiment and didn’t think anyone but we would care. However another group discovered the same phenomenon and had the good sense to publish a Physical Review Letter on the topic.

Up to that moment progress had been good but we then hit a barrier. Our plan was to accelerate the electrons emerging out the top of the vortices in order to achieve ion multiplication and collect the amplified charge on a metal plate. As I increased the accelerating voltage there was no noticeable gain until at some voltage the gas completely discharged in an electric breakdown. I assumed that the breakdown was caused by the collector electrode not being sufficiently smooth or not sufficiently planar with respect to the surface. I spent several months working on improving those electrodes but always found the same result: no amplification and then catastrophic breakdown.

One of the golden rules of science is "an afternoon in the library can save you six months in the laboratory". Unfortunately that rule was not included in the graduate course curriculum. Forced by desperation I finally did spend an afternoon in the library studying what was known about gaseous breakdown and the construction of gas proportional counters for nuclear physics. It finally dawned on me that the worst possible geometry for controlled amplification was the parallel plate system I was employing. What was needed was a small diameter wire collector that presented an electric field gradient. With parallel plate electrodes the electric field is uniform so that when there is sufficient acceleration to create amplification, the multiplication is enormous, on the order of 2^n where n is the number of mean free paths traversed. In contrast, a wire presents an electric field varying as r^{-1} so that the region of space active for multiplication is controlled and limited.

Armed with that late afternoon insight I went directly to the lab, made a wire collector, cooled down and, after working all night, by morning was able to show Sanders stable gain on the order of 100. I remember him saying, "Why didn't you think of that before?" to which I recall responding, "You're the advisor, why didn't you think of it?"

This was the final technical hurdle. Figure 1 shows some typical data[1] giving the charge trapped in the fluid as a function of rotation speed of the cryostat. The steps heralding the appearance of the individual vortices are clearly evident.

Although it was exciting to see this predicted phenomenon, one doesn't learn much new by simply proving a prediction. The more interesting aspects of this work came over the succeeding months as we learned a great deal about the nature of rotating superfluids[2]. The most significant qualitative thing we learned was that rotating superfluid helium is a very metastable system. The vortices often appear at angular velocities much greater than the equilibrium predictions (i.e. the speed where the free energy is minimized with the presence of the vortex) and once formed, tend to not leave. Even when the cryostat was stopped there remains remnant sections of vortex filament. This is presumably due to vortex pinning at microscopic pinnacles on the surrounding walls.

PULSAR GLITCHES AND SUPERFLUID VORTICES

After graduation I moved to the University of California at Berkeley, first as a postdoctoral researcher working

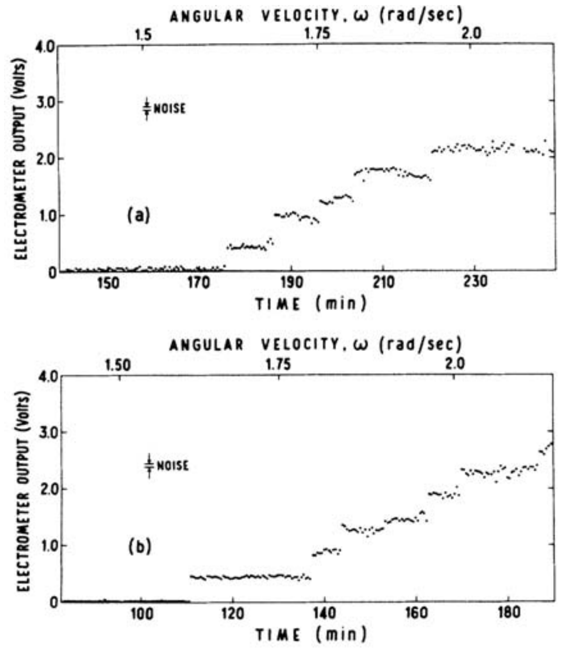

FIGURE 1. A plot of trapped charge as a function of time while the angular velocity is steadily increasing. The upward steps indicated the creation of individual vortices

with Fred Reif and later as an assistant professor. Soon after arriving in Berkeley I overheard a lunch conversation describing the strange sudden acceleration events (called glitches) that were being observed in pulsars. I learned that the pulsars were believed to be rotating neutron stars with superfluid interiors. Based on my work with rotating helium it was clear to me that if a rotating container of superfluid was slowly decelerating, the stick-slip metastable relaxation to equilibrium of the vortices would cause abrupt torques on the container. If the container was freely suspended the walls would glitch upward in speed. It took only a few days to write up this idea to explain the pulsar glitching[3]. A few years later P. W. Anderson developed this idea into a more detailed theory of pinning which is now part of the standard paradigm in the pulsar community. I was delighted that the things we learned about vortices in helium could contribute to a seemingly unrelated field. Experiments that I suggested in that paper were later performed successfully in Tblissi, Georgia, where the superfluid glitches were clearly seen.

PHOTOGRAPHY OF VORTEX ARRAYS

I joined the Berkeley faculty in 1971. Although my postdoctoral work focused on studying ultra violet emission from liquid helium and liquid and solid neon, my appointment to the faculty was going to require something more substantial to be promoted to tenure. One path to follow was to extend the vortex charge-trapping experiments to see if I could accelerate the electrons emerging from the surface and image them onto a phosphor screen in order to map the vortex distribution. This had been a long term goal of my work with Mike Sanders although there was insufficient time to pursue it at Michigan. Many years later I heard that Prof. Careri in Rome had conceived of similar experiments calling it the "TV method".

This experiment faced several technical challenges. My first estimates of the image brightness were based on the assumption that the electrons would be accelerated through 10 kV before striking a phosphor. However early efforts showed that there was electric breakdown above the helium when only 500 volts were applied between electrodes. This implied that only a few photons would be available to "see" each vortex line. Furthermore, it was not known if any of the conventional phosphors would emit light at cryogenic temperatures after electron bombardment. In additon, to perform electron optics above the liquid helium surface one needs a long electron mean-free-path. Typical pumped-bath temperatures are not sufficiently low to achieve the correct limit. This technical challenge called for a ^{3}He refrigerator, and a rotating one to boot. Fortunately Mike Sanders had offered to send my old rotating Dewar set to Berkeley since it wasn't being used in Michigan. At that point I set out to make an adsorption pumped ^{3}He refrigerator along the lines described in a paper by J. Daunt. This was my first involvement with sub-Kelvin technology.

Early in this project I received the best gift any assistant professor could receive. Gary Williams, a new graduate student at Berkeley, walked into my lab and said he would like to work on superfluid helium. It became clear very quickly that Gary had already learned more about superfluids while he was an undergraduate than I knew at that time as a beginning academic. Since we were only a few years apart in age we made a good team. We were immediately equals in the project and set out on a challenging yet very exciting path. Both of us immensely enjoyed the challenge before us and our wives had a difficult time keeping us out of the lab.

Our first attempts to image the vortices were frustrating: the photographs showed only blurs of light. Gary developed a cold cathode emitter that produced a triangular pattern of emerging electrons to simulate the predicted vortex lattice. We learned from this "phantom" that the electron paths diverged due to space charge. We fixed this problem by surrounding the experiment with a home-made superconducting magnet which created electron focusing using the small cyclotron orbits characterizing the system. We again tried to image the vortices but still found only blurred patterns with no structure. However we now knew that the electron optics were not a limitation.

One feature that didn't make sense was the measured lifetime of the electrons on the vortices. Since the electrons escape by thermal activation, the lifetime, τ, should have been immeasurably long at 300 mK, our operating temperature. However we never found a τ greater than a few seconds. Gary suggested that perhaps we were not measuring the lifetime of electrons on the lines but rather the lifetime of the lines in the container. If the vortices were continually moving about they would eventually encounter a wall and be destroyed, or at least electrically discharged. At 0.3 K there is insufficient normal fluid to damp vortex motion so it seemed reasonable that the vortices might be randomly moving around searching for their equilibrium position. We decided to trick the vortices by introducing a bit of ^{3}He into the fluid. The ^{3}He would serve as a fixed amount of normal fluid even at the lowest temperatures. The vortice's motion would then be damped, thus driving them to their equilibrium positions where they are at rest in the rotating frame.

After adding the ^{3}He, the observed trapped lifetime immediately jumped to immeasurably high values thus suggesting our model was correct. Now we thought the vortices might be immobile enough to let their picture be taken. However, the addition of the ^{3}He raised the vapor pressure so that at 0.3 K sharply focused electron imaging was no longer possible above the liquid. Lower temperatures were required but that would require a dilution refrigerator, and a rotating one at that. While I was a graduate student I had observed one of the first dilution refrigerators being assembled. It was an enormous undertaking and I decided then that I would never be involved with something so complex. However since any temperature below 0.1 K would suffice to get the vapor pressure low enough, we didn't need a great dilution refrigerator

A short while earlier John Wheatley has started selling dilution refrigerators through his company SHE. Since there was no such thing as start-up funds in those days I had little money to spend on equipment. However, for about $1000 I could just afford to purchase the dilution stage of what SHE called their "minifridge", which used a simple countercurrent heat exchanger capable of cooling to ~ 40 mK.

With the tenure clock ticking we decided to undertake to build from scratch a rotating dilution refrigerator based on the SHE stage. Everything but the stage would be built by ourselves or by our department shops. Fortunately that was an era where we paid no shop charge, one of the main attractions of Berkeley. If I had to pay a substantial shop charge I never would have undertaken to build that machine. Gary and I put in a very intensive seven weeks, building as fast a possible. At the end of that period we had our rotating dilution refrigerator and even used it to perform a quick experiment to look for thinning of flowing superfluid films[4].

Soon the day came when everything was in place and we began the vortex photography run. We had run out of tricks to play and Gary was probably thinking that he had made a poor choice in research topic. Like almost all low temperature work, it wasn't until the middle of the night that we were ready for the ultimate test. We rotated the cryostat at a speed favorable for one vortex to exist in the little bucket. We charged up the lines with electrons for about 20 seconds, discharged the lines, and accelerated the charge toward a home-made phosphor screen deposited on a coherent fiberoptics light pipe that extended from 40 mK up to a home-made room temperature image-intensified camera. We then reached into the slowly rotating machine to remove the single piece of high speed film that might hold the vortex image, Gary's Ph.D. and my professional fate.

Going into the dark room together, Gary ran the film though the developer and fixer, turned on the lights and held the still dripping film up to a light. He looked at me and made the greatest understatement I have yet heard: "I think there is a spot in the center of the film."

Some of the early images we acquired were published in Phys. Rev. Letters[5]. I received tenure and Gary became a faculty member at UCLA. A few years later my postdoc Ed Yarmchuck made signal-averaged pictures[6] of the vortices which displayed the regular predicted patterns up to about N=10. Figure 2 shows some of the vortex states. Ed even made some movies showing how the vortices moved to adjust to a transition from N to N+1 vortices. This was an enormously fun experiment for me. We were all very young and did so much with our own hands. One of the nicest souvenirs of that time is a letter that Gary and I received from Richard Feynman. He told us how pleased he was "to see visions in my head at night coming out in black and white reality."

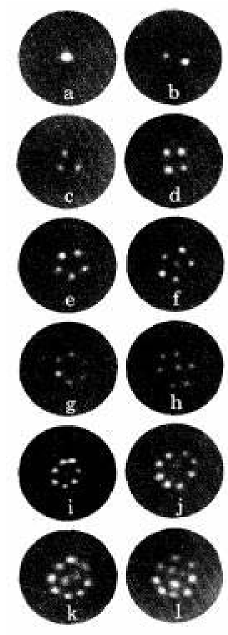

FIGURE 2. Photographs of the low lying stable vortex states in rotating HeII. The black outline is the boundary of a 2 mm diameter rotating cylinder. The angular velocity is varied from a 0.3 rad/s to 0.59 rad/s. Each image is a 60-fold multiple exposure taken at the same angular velocity. From Ref. [6].

ENTERING SUPERFLUID ^{3}HE PHYSICS

By the mid 1970's the discovery of new phases of liquid ^{3}He began to turn my attention away from vortices. Here was a new quantum liquid waiting to be explored. Unfortunately this state of matter only exists below about 2 mK and I had no experience working below 40 mK. The thought of building dilution refrigerators, etc. seemed overwhelming to me. However in the Summer of 1976 I attended a Gordon Conference in New Hampshire where all the talk was about ^{3}He. Two short conversations there had a big impact on my research direction.

The first occurred when I was wading in Lake Winnipesaukee with Matti Krusius. Matti was then a postdoc of John Wheatley. I must have mentioned to Matti my trepidation about trying to do millikelvin physics, especially concerning the small size of my group which was never more than two students. Matti made the optimistic statement: " The colder you go the easier it becomes." He meant that thermal conductivities go to zero with temperature so thermal isolation becomes ever easier. Fortunately he forgot to mention that the thermal boundary resistances go to infinity which, combined with vanishing heat capacities of ^{3}He, makes the very lowest temperatures still rather difficult to achieve.

The second conversation occurred the next evening. I was tagging along with some East Coast folks and we settled down in a pizza parlor. At that time there were three methods being used to cool into the ^{3}He superfluid phases. Wheatley at San Diego used CMN demagnetization. Cornell used Pomeranchuk cooling and Helsinki used copper nuclear adiabatic demagnetization. I asked John Reppy, "If you were starting from scratch to go into the ^{3}He business, which cooling technique would you choose?" After draining the last of his large glass of beer and wiping the suds from his ever- present beard, he replied "Absolutely nuclear cooling."

I returned to Berkeley with the sure knowledge that nuclear cooling was the way to go and that "things would get easier when we got colder". At that moment a document serendipitously passed my way. My chemistry colleague Norman Philips had just returned from a sabbatical leave in Helsinki and brought back to Berkeley a thesis by Robert Gylling entitled something like: "The design, construction and operation of a nuclear demagnetization cryostat." Armed with this instruction manual on how to be an ultra low temperature physicist I convinced my second graduate student Keith DeConde to help me build a cryostat capable of doing experiments on superfluid ^{3}He. It is significant to mention that Keith had already gathered data on an experiment on ^{4}He that might have served as a thesis. However, he had used a rather simple apparatus and we both agreed he should build something more substantial before graduation. I somehow neglected to tell him that the team in Helsinki consisted of about 6 experienced researchers and that these machines typically cost much more money than I possessed.

My thought was that I needed to buy only the dilution stage of an SHE fridge, a demagnetization magnet, and a dewar. All the rest I planned to beg, borrow, build or steal. Using again our zero charge machine shop we made some drawings and got to work. By the time that the dilution stage arrived in its little box we had the homemade cryostat built and had read enough papers to figure out how to do nuclear orientation (NO) thermometry. It was easy to borrow the nuclear counting electronics because there was a lot of this type of physics performed at the Lawrence Berkeley Laboratory. I was even able to borrow a radioactive Cobalt source to provide the thermometry signal.

Within about six months we had the DR working as determined from the NO measurements and then had to figure out how to do the nuclear demag part. The most difficult thing was measuring the temperature. It seemed that platinum NMR thermometry was the most promising technique but neither of us knew anything about NMR. Furthermore, although Berkeley had some of the world's greatest NMR experts (Hahn, Knight, Portis), they all knew too much to be of help to novices like us. Also, none of them had any experience doing NMR at the relatively low frequency of 200 kHz using op amps for the electronics. We thought we would just copy the things the Helsinki group had published and couldn't go too far wrong. They even specified the kind of platinum powder to use. It came from a company called Leico and we ordered a similar sample .

Our first attempts to determine temperature using NMR thermometry failed. In fact we couldn't even detect an NMR signal. Assuming that being novices this was par for the course, we carefully redesigned the electronics and tried again. Still nothing. Over and over this went, week after week until after a few months we lowered our sights and simply desired to see any kind of NMR signal. My colleague Walter Knight still had one viable NMR system and he helped us to see a signal with an old platinum sample from Erwin Hahn. We then tried our much better Leico powder and saw nothing! This just didn't make sense. We then sent our sample to an analysis laboratory which returned a report telling that the "very pure" Leico powder was highly contaminated with iron, a magnetic impurity that surely would wash out any NMR features. We had always wondered why the powder didn't look silvery the way we expected but was instead a deep black. We then called the manufacturer who said something to the effect, "Oh yes, we discovered we had sent a bad batch of platinum but we didn't want to upset you." These were really considerate people. Later I read a remark in a paper by George Pickett who wrote something like: "This powder looks black because it is black and any thoughts about the wavelength of light are completely irrelevant."

With hindsight the months spent chasing the elusive NMR signal were not a complete loss. By the time we finally acquired the NMR signal in powder given to us by Erwin Hahn, we had become quite expert in the NMR technology. Thermometry with this technique has never let us down yet. If the signal had appeared immediately we never would have gained the depth of understanding of the electronics or the physics which came in very useful in later experiments.

After Keith graduated two new students agreed to work with me: Jim Eisenstein and Greg Swift. They were an interesting combination. Jim was a committed Easterner and loved formal quantum theory. He considered the West Coast a primitive environment totally lacking class. Greg was thoroughly a Nebraskan and had won the state science fair by building a binary computer out of cloths pins mounted on rubber bands. Their skills and personalities complemented each other very well and we all had a good time. I enjoyed playing the Philistine for Jim's refined sensibility and I enjoyed observing the inventive genius of Greg who could find short cuts for some very difficult technical tasks. I still smile when I reread Greg's paper on how to repair a millikelvin cryostat in 24 hours turnaround.[7]

Greg and Jim's experiments were selected to be the simplest we could do to enter the field of ^{3}He superfluid physics. Greg would demonstrate a predicted anisotropy in the superfluid dielectric constant[8] and Jim would determine the depairing critical current for ^{3}He flowing in small tubes[9].

The effect that Greg sought turned out to be almost four orders of magnitude smaller than the original predictions but he eventually succeeded in seeing the effect at $\delta\varepsilon/\varepsilon \sim 10^{-10}$. I think that is still probably a record for dielectric measurements. The clever geometry of Greg's capacitor was something he learned in kindergarten in a paper-folding lesson. The capacitance bridge expertise that we gained from this experiment proved invaluable in future years.

Jim's experiment also worked very well and he was able to make several measurements that we had not planned originally. It was this experiment, measuring the flow through tubes of $\sim$25 micron diameter that led eventually years later to our research on Josephson weak links. But that came long afterwards.

I had enormous good fortune in having as my first four students: Gary Williams, Keith DeConde, Greg Swift and Jim Eisenstein. Because of all that we accomplished I came out of that period feeling we could do anything! That was a too optimistic assessment but it served me well in the future to go in directions I would not have moved had we not had the early successes.

DETECTION OF ^{3}HE PERSISTENT CURRENTS

In 1984 I had the good fortune to spend six months in Olli Lounasmaa's laboratory in Helsinki. Olli had first spent several months in my lab helping to develop a new experiment to search for persistent currents in ^{3}He. I was focusing on this problem because Jim Eisenstein and I had difficulty in understanding the large flow dissipation in his ^{3}He depairing apparatus. We were led to speculate[10] that there might be an unsuspected intrinsic dissipation present that would prevent the ^{3}He from being truly "super". The way to rigorously test for superfluidity is through the presence of persistent currents. It turned out that our mystery dissipation was just our lack of understanding of the phenomenon called "second viscosity"[11]. Even with this explained I felt it was useful to observe persistence in flow. Our plan was to detect trapped angular momentum by detecting the reaction to imposed torques on a powder-filled torus that had been previously rotated. The apparatus was built in Berkeley and then I took a six month leave to work in Olli's lab where he had the ROTA 1 rotating cryostat.

While in Helsinki I worked closely with graduate student Jukka Pekola. We were very compatible and the experiment progressed rapidly. The apparatus showed clear evidence of persistent currents[12] and in addition confirmed a vortex phase transition[13] that had previously been observed through their NMR studies.

I took away two wonderful souvenirs from that visit. The first, my son Ben, was born in the Helsinki women's hospital. Since it never occurred to me to take a marriage license to Finland, Ben was issued a passport by the U.S. Consulate under the illegitimacy statute, which states that the mother is a U.S. citizen but the father is unknown. The second souvenir was Jukka Pekola who, after his graduation, came to Berkeley to work as a postdoc with me.

DETERMINATION OF QUANTIZED CIRCULATION AND COOPER PAIRING

After returning from Finland I decided to follow the example of the Helsinki laboratory and acquire two nuclear demagnetization cryostats. One cryostat could be used for developing new experiments while the other was used to take data on more mature projects. Working with Seamus Davis, Rena Zieve and John Close, we developed a rotating refrigerator capable of achieving temperatures in ^{3}He as low as 160 μK. Although my original motivation to have the new machine rotate was to perform NMR experiments on ^{3}He vortices, we began to formulate more ambitious plans when we saw that we were achieving really quite low temperatures.

For many years I had harbored the desire to determine if fluid circulation in superfluid ^{3}He was quantized in units of $h/2m_3$. Such an experiment would simultaneously demonstrate macroscopic quantum phase coherence (this is the origin of quantized circulation) and also provide the smoking gun for Cooper pairing by the presence of the factor 2 in the denominator. Everyone in this field accepted the paradigm that included both these points but paradigms need experimental confirmation.

The method to determine circulation in superfluids was invented by W. F. Vinen in about 1960. Using ^{4}He, he monitored the precession of the vibration plane of a vibrating wire immersed in rotating helium. The rate of precession of a round wire gives directly the fluid circulation around the wire[14]. For this measurement to be feasible requires the damping due to normal fluid to be sufficiently small for the wire's vibration to have a high Q. This criterion is easily achieved in ^{4}He where the fluid viscosity is small even above the transition temperature. By contrast, in ^{3}He the normal viscosity above T_c is similar to machine oil and a thin wire won't vibrate at all. However at temperatures below $0.2T_c$ the normal fluid density becomes exponentially small and the background damping drops dramatically. When we found that we could achieve 160 μK ($\sim 0.2T_c$ for ^{3}He) we decided to try the Vinen technique in ^{3}He.

The experimental cell is really quite simple. A persistent magnet provides a field transverse to a vertical superconducting wire. Leads across the wire's end provide both for the input of a current pulse (which plucks the wire) and a route to convey the voltage induced by the vibration up to a room temperature preamplifier. When the wire vibrated perpendicular to the magnetic field there is maximum signal and when the wire vibrates parallel to the field there is no induced voltage. The cryostat is rotated about the vertical axis. One simply monitors the modulation envelope of the vibration and computes the circulation from a simple formula.

Figure 3 shows the measured circulation as a function of the cryostat's rotation speed[15]. The quantization levels 1, 0, and –1 are very clear and the unit of quantization is $h/2m_3$, just as predicted from theory. So the accepted paradigm is correct but we were fortunate to discover a new phenomenon that was completely unexpected.

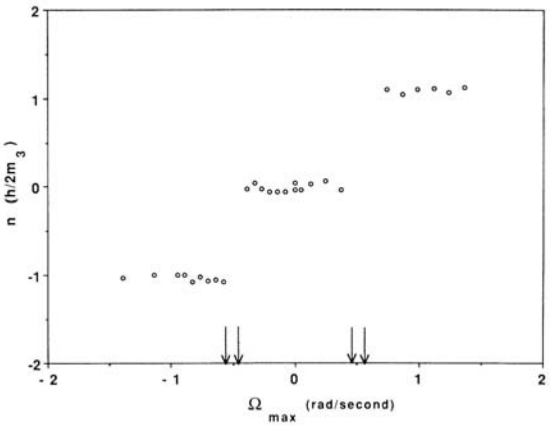

FIGURE 3. The value of circulation around a wire as a function of the maximum angular speed at which the cryostat had been rotated. The circulation level is unstable between the sets of arrows.

Figure 4 shows the circulation as a function of time after the cryostat has been brought to rest. The circulation remains trapped for many hours but then, as one end of the vortex filament becomes unpinned from the wire, the circulation slowly decreases as the filament becomes "unzipped" from the wire. The new feature is the slow oscillation in the circulation that is superimposed on the decreasing signal. Through a phenomenological force balance model we conjectured that the unpinned segment of vortex must exhibit a kind of "helicopter" motion in order for the vortex tension to be balanced by Magnus force. We derived an expression for the helicopter frequency that agreed within 1% of the observed value[16]. Later Klaus Schwartz observed this helicopter motion in numerical simulations[17] and GrishaVolovik[18] showed that the frequency was just the ac-Josephson frequency associated with cyclic 2π phase decrements which must accompany the hydrodynamic pressure head across the cylindrical container. Discovering and then explaining the helicopter motion was at least as exciting for us as observing the quantized circulation.

SMALL APERTURE EXPERIMENTS

Shortly after my stay in Helsinki, Jukka Pekola joined my group as a postdoctoral fellow. The project we pursued came out of discussions we began in Helsinki. I had recently finished studying ^{3}He flow in single long

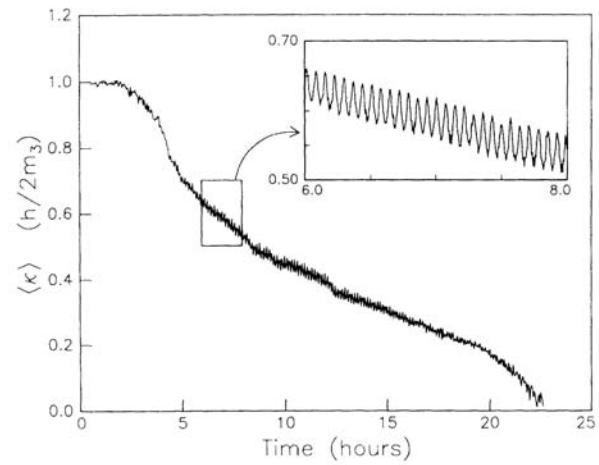

FIGURE 4. The decay of circulation trapped on a vibrating wire. The effective circulation varies periodically as the detached end of the vortex precesses around the wire at the Josephson frequency.

tubes of about 25 μm diameter. This dimension is much greater than the ^{3}He healing length, $\xi_3 \sim$60 nm. Jukka had previously pursued a search for Josephson signatures in Nucleopore filter, a material containing millions of long tubes of submicron dimensions. On the blackboard we had determined that it should be possible to study flow in a single submicron pore. Although the experiment looked difficult our desire to find a superfluid "weak link" was high.

In 1983, just before Jukka arrived, Seamus Davis joined my lab as a new graduate student. He and Jukka worked well together and soon they were making single submicron holes by bombarding materials with heavy ion fragments from a radioactive source. We were able to do this because Reimer Spohr was visiting the Berkeley group of Buford Price, the co-inventor of track etching technology. Reimer helped us to make a simple apparatus that permitted a single nuclear fragment to pass through our sample foil. Chemical etching later turned that track into our pore.

We used our sensitive capacitive techniques to study the deflection of a flexible diaphragm that formed one wall of a chamber connected to a larger bath through our small hole. Jukka and Seamus studied the flow of ^{3}He through the small pores under a variety of conditions. Unfortunately there was no flow signature that resembled anything that we anticipated as connected to weak link behavior. However, the flow signature of the pores also did not fit into our understanding of superfluidity. In particular there was no obvious critical velocity: dissipation was always present[19]!

While Jukka and Seamus continued the project I went off for a one semester stay in the Kyoto Laboratory of

Akira Hirai and Takao Mizusaki. While there I became even more interested in pursuing Josephson physics. My goal was to develop a superfluid analog of a SQUID, (superconducting quantum interference device). By hand written letter (this was pre-email days) I urged Seamus and Jukka to detour from their I-P measurements to seek interference effects. This suggestion of changing direction coming from so far away was not well received by my young colleagues. I have never been very successful in changing the course of a student researcher's direction, and in this case, as in several others, I just had to wait for new opportunities.

Returning to Berkeley my students Seamus, Ajay Amar, and I began a short experiment to investigate some interesting claims about the superflow properties of ^{3}He films. Our experiments observed superfluid film flow for sufficiently thick films and allowed us to map out the phase boundary (thickness vs. T_c) for ^{3}He films[20]. We found behavior consistent with prevailing theory and established some familiarity with film flow which would serve us well ten years later when we detected third sound in ^{3}He.

After the film work was concluded Ajay pursued experiments on ^{4}He flowing through small apertures. In 1985 Eric Varoquaux and Olivier Avenel detected single 2π phase slip events in superfluid ^{4}He. Using an ion-milled slit in submicron thick nickel foil, they presented clear evidence of the phase slippage process and showed how one could determine the nucleation processes for primordial vortices[21, 22]. Their apparatus used the geometry equivalent to an rf-SQUID. It seemed possible that the phase slip process in this geometry might be used to make a sensitive gyroscope. Such an instrument might be sufficiently sensitive to make contributions to geodesy or general relativity. Amar's thesis project was initially intended to demonstrate the superfluid ac-SQUID but ended up being a study of the phase slip process[23]. We had a lot to learn before we could make a rotation sensor using superfluids.

In 1992 I spent one semester in Trento, Italy as a guest of Stefano Vitale and Massimo Cerdonio. They were also pursuing development of a superfluid gyroscope using phase slippage. Their goal was to develop instruments to test general relativity. Stefano was already an expert on rf SQUIDs and understood all the subtleties contributing to the noise of these devices. For five months I shared Stefano's office and had an extraordinarily enjoyable visit. It turned out that Stefano shared my passions for sailing and physics. In fact at age 18 he had been European champion in a small dinghy class. During that sabbatical we wrote a paper describing the principles of superfluid gyroscopes, including devices that might exploit a dc-Josephson effect if that was ever achieved, as well as linear phase slip devices[24].

I returned to Berkeley fully enthused to pursue both ac and dc versions of superfluid gyros. In ^{4}He it became clear that we would have to understand the phase slip process better. The Saclay group were observing flow signatures that were elusive in Berkeley. This led us to abandon the double path geometry of the ac-SQUID in order to focus on the processes occurring solely in the small aperture. Fortunately this different geometry enabled us to study two regimes of flow: low frequency Helmholtz motion, similar to the Saclay research, in addition to dc pressure drives. Comparing the critical velocities for phase slippage in the two kinds of flow led us to test a theory that Stefano and I had developed[25]. The experiments were successful and led to discovering what seems to be a universal energy barrier for the creation of primordial vortices[26]. This provides important insight into the long standing problem of understanding how vortices enter superfluid ^{4}He.

With the knowledge and experience acquired from our phase slip experiments we returned with renewed enthusiasm to developing a superfluid analog of an ac-SQUID. Whereas quantum phase coherence throughout a sample lets the superconducting SQUID exhibit sensitivity to magnetic flux, the superfluid analog would be sensitive to rotation flux, $\Omega \cdot A$ where A is the area vector normal to the plane of the pick-up loop. Due to the dot product in the flux, the specific signature for the phase slip gyroscope would be a cosine modulation in the phase slip critical velocity as the plane of the device was reoriented with respect to the rotation axis of the Earth.

Keith Schwab and Jeffrey Steinhauer were the two students who, in 1995, felt the satisfaction of seeing the signal induced by the Earth's rotation[27]. Figure 5 shows the modulation in the phase slip critical velocity caused by reorientation of the ^{4}He phase slip gyroscope with respect ot he Earth's rotation axis. Avenel and Varoquaux had observed a similar phenomenon a few months earlier[28] but we were nevertheless thrilled to have achieved our first gyroscope in Berkeley. It had been almost 13 years since I first embarked on the path to see this phenomenon! It still seems miraculous to me that all the atoms in a toroidal sample of helium communicate in order to maintain the spatial phase coherence that underlies the superfluid gyroscope.

Much of our research overlapped the Saclay group and led to some hard feelings between the two laboratories. This was the only time in my career that we did not enjoy friendly relations with competing groups. It was a great pity and a loss for both groups. Professional scientists are too often more concerned about who is first rather than the more important questions about what is actually learned. When ego takes priority over knowledge, science is not well served.

My student Niels Bruckner later developed a multi-turn geometry for the phase slip gyroscope that increased its sensitivity by almost two orders of magnitude[29]. No

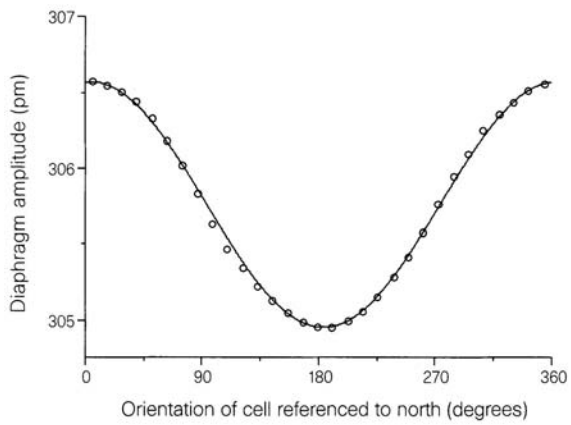

FIGURE 5. The modulation of phase slip apparent critical velocity as a function of the orientation of the superfluid gyroscope. The solid line is the predicted cosine pattern arising from the rotation flux.

new noise sources have appeared thus far so it may be possible to extend the device to useful sensitivities. Recently however our efforts in Berkeley have been focused on other kinds of rotation sensors which are analogs of dc-SQUIDs.

At the same time that some of my students were pursuing phase slip studies in ^{4}He, others were trying to observe physics related to a sine-like current-phase relation characterized by a Josephson weak link. The criterion to enter this regime is to have an aperture whose spatial dimensions are on the order of the superfluid healing length. Our program evolved from Eisenstein's 25 μm tube, progressed to Pekola and Davis's single submicron long tube, and eventually reached small single apertures machined in thin silicon nitride using e-beam technology. None of these apertures displayed flow characteristics that we could associate with Josephson-like behavior. This was a very discouraging time for this investigative avenue.

In the late 1980's the Saclay group reported flow experiments in ^{3}He using a slit aperture whose width was comparable to the superfluid healing length. The aperture was part of the inductive element in a hydro-mechanical oscillator. This so-called Helmholtz oscillator displayed nonlinear behavior which they modeled as consistent with a sine-like current phase relation, the signature of a Josephson weak link[30]. I found their findings sufficiently encouraging to continue our investigation of ^{3}He weak link physics.

The most striking signature displayed by a Josephson weak link is the quantum oscillation resulting from a chemical potential differential. For superfluid ^{3}He, a chemical potential difference is associated with an applied pressure head. Our experiments therefore often consisted of measuring the mass-current through an aperture resulting from a given pressure head. These current-pressure characteristics are the analog of I-V curves studied in superconducting systems. For several years we observed neither Josephson oscillations nor an I-P curve that resembled anything anticipated for a weak link. Our expectation was that the I-P curve should extrapolate to zero pressure at a finite critical current. In contrast all the I-P curves passed through the origin. An understanding of this mystery came almost ten years after we first observed the phenomenon[31].

As a final effort in the project I wanted to see if an array of many nanometer sized apertures might provide signatures that were more informative than our single aperture experiments. My hope was that an array might behave quantum coherently with all the apertures locked together. If there are N holes undergoing coherent Josephson oscillations, the mass current would be amplified by N and single aperture signals hidden by noise might be revealed. The basis of this hope was that gradients in quantum phase represent velocity fields. Therefore the lowest state of mechanical energy should have no phase gradient along a surface running parallel with the array. A crude estimate of the phase gradients induced by k_BT fluctuations indicated that phase coherence should be quite strong at the 1 mK temperature regime characteristic of superfluid ^{3}He.

Our first attempt at an array experiment produced an I-P curve similar to those in one aperture. However Yuri Mukharsky, a postdoc in my lab, suggested that the array we were using contained apertures too closely spaced. He explained that close spacing would induce an effective linear kinetic inductance in series with the apertures that would hide the possible nonlinear Josephson effects we were seeking.

During these early years of aperture experiments the technology of e-beam nano-fabrication was not readily available to us. Our first array samples were made gratis by Perkin Elmer corporation. Later my students began to learn the technology in the steadily developing microlab in our electrical engineering department. It took many months for a student to learn how to manufacture the apertures and our existing e-beam writers would only hold one small sample at a time. We found it most efficient to send a trained student to the Cornell National Microfabrication Facility, to work with a technician there. In the Cornell facility one could write on an entire 4" wafer and generate dozens of aperture arrays with various hole sizes and grid dimensions. My student Alex Loshak brought back many samples of aperture arrays, some of which we are still using today, almost ten years later.

One challenge that has not been satisfactorily met is to accurately determine the aperture size before we do experiments. The problem is due to the difficulty of performing e-beam microscopy on insulating materials such as the silicon nitride "window" that contains our arrays. When the nitride becomes charged by the electron beam, the images get easily distorted. In addition, the act of viewing an aperture changes the microstructure of the device due to the e-beam bombardment. Some of the images are so deceiving that what appears as an aperture in a picture is in fact a closed indentation in the surface. At present it is sufficient to make optical microscope observations of the typically 50 nm apertures. Although this dimensions is only 1/10 the wavelength of the light, the observer can discern whether or not the holes are open.

The arrays that have proven most productive were those designed following Mukharsky's suggestion that the apertures be well spaced from each other. We used a 65x65 array of nominally 60 nm diameter holes arranged on a square lattice with 3 μm spacing. With the array immersed in superfluid ^{3}He below 1 mK, we applied a sudden pressure differential across it. We sensed the mass current through the array by the resultant deflection of one flexible wall of the chamber. This wall is typically a plastic membrane about 10 μm thick. The plastic is coated with a superconducting layer which, when it moves relative to an adjacent coil, changes that coil's inductance in a manner detectable using a superconducting SQUID. This type of displacement transducer was developed for gravity wave research and can detect deflection as small as 10^{-15} m.

In our first attempts with these arrays the transient signals were viewed in real time on an oscilloscope. The traces revealed no Josephson oscillation behavior. However when we put the electrical signal into an amplifier and listened to it on a set of head phones we could clearly distinguish a whistling tone that changed continuously in frequency. Sergei Pereversev, a postdoc in the group, was the first person to hear these Josephson oscillations. My student Scott Backhaus had been looking for Josephson sound in phase slip phenomena in ^{4}He. He agreed to join our ^{3}He effort and quickly developed a computer program to Fourier transform the transient signals and look for a quantum whistle signal that was correlated with the changing pressure head across the apertures. The result of that analysis was striking. The frequency of the oscillation increased linearly with the pressure head and the slope of that curve agreed within the systematic error of our pressure calibration with the Josephson frequency formula[32] $f_j = \Delta\mu/h$, where μ is the chemical potential and h is Planck's constant. For the first time we were convinced that we had Josephson weak links.

Our discovery of Josephson oscillations opened up a very rich field of physics to us. The Josephson frequency provided an absolute in-situ calibration of our membrane pressure gauge. This in turn led us to a method which permits determination of the current-phase relation for

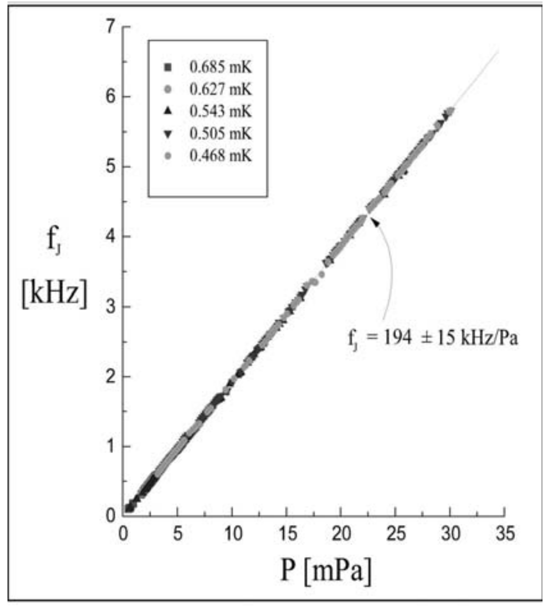

FIGURE 6. A plot of the frequency of the "quantum whistle" as a function of the pressure difference across a weak link array. The various symbols correspond to different temperatures. All the points lie on a straight line whose slope is given by the Josephson frequency formula.

the weak link arrays. We discovered that although the arrays were characterized by the Josephson sine-like relation when the temperature dependent healing length was comparable to the aperture dimensions, at lower temperatures the $I(\phi)$ function morphed into an almost π periodic curve. This unexpected feature was later explained in microscopic theory by E. Thuneberg and his students[33]. The $I(\phi)$ curve also explained a mysterious π-state we discovered that had attracted considerable theoretical attention. We also found that the arrays had two stable states characterized by different $I(\phi)$ functions and different critical currents. We speculated that the origin of the bi-stability was due to two possible orientations of an internal vector field near the array surfaces. This was later shown to be the case by further calculations by Thuneberg's group[33].

We went on to observe several classic phenomena characteristic of superconducting Josephson weak links[34]. These included quantitative agreement with the Shapiro step theory, Fiske steps and plasma mode motion. In fact the superfluid array turned out to be a better example of these phenomena than the 1000's of papers on the superconducting analogs!

Much of the research on weak links had been driven by the goal of making sensitive rotation sensors, i.e. superfluid gyroscopes. Since most superfluid phenomena are of a rather esoteric nature, it would be satisfying to see some superfluid phenomenon make a contribution to applied technology. As described earlier we had already made a ^{4}He gyroscope using quantized phase slippage but we felt that a true dc-SQUID device would offer potentially lower noise. A superfluid-SQUID is a direct analog of the superconducting dc SQUID wherein rotation plays the role of magnetism.

Graduate student Ray Simmonds had the primary goal for his thesis to make the first proof-of-principle demonstration of such a device. After several discussions related to the geometry of this experiment, Ray settled on a design that minimized the superfluid complexity. He included a pair of similar weak link arrays in a flow circuit containing an enclosed area of sufficient size so that the Earth's rotation would provide slightly more than one quantum of rotation flux. The idea was to reorient the "pick-up loop" with respect to the Earth's rotation axis. Our rotating cryostat, originally used to observe quantized circulation was a perfect reorientable platform for this project.

Early trials showed the unmistakable interference signature we sought but building vibrations were limiting our signal to noise. Our cryostat already contained extensive vibration isolation and our laboratory was in a second basement which had the lowest vibration noise in our department. Nevertheless, the sensitive superfluid device was limited in its rotation resolution by motion in the local environment. Almost all vibration in buildings is generated by the occupants and the machinery that supports their occupation of the building. It was clear that the way to get the quietest conditions was to shut down all of the buildings mechanical infrastructure and evacuate all the occupants except for ourselves. Although this is a simple technical solution it is politically very difficult. After extensive negotiations we were able to have the building "shut down" over one Christmas and one New Year weekend. The building on these days was remarkably quiet. We were able to get good quality data which we published in Nature showing a respectable proof-of-principle demonstration of the superfluid ^{3}He dc-SQUID gyroscope[35]. Figure 7 shows the interference pattern induced by reorientation of the gyroscope loop with respect to the Earth's rotation axis.

This proof-of-principle experiment was in some sense the goal of much of our research for over a decade. Getting the results, which agreed so well with theory, was very satisfying. On the other hand it raised the obvious questions of how sensitive a gyroscope could be made and could such a device find utility in geodesy, seismology or navigation. On paper the sensitivity of a superfluid SQUID looks very good and might be greater than competing technologies including laser gyroscopes and atomic beam devices. Superfluid gyros, atomic beam gyros and optical gyros all utilize the same principle: a rotationally induced phase shift in a wave function that senses the absolute inertial frame. The relevant phase

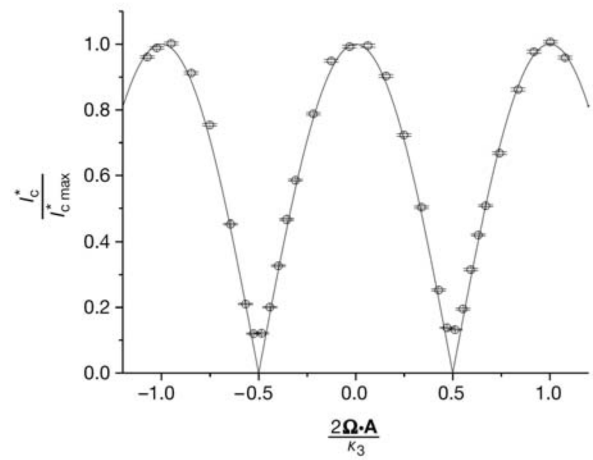

FIGURE 7. The critical current of a superfluid ^{3}He SQUID gyroscope. Two spatially separated weak link arrays behave as a single weak link whose critical current is modulated by rotation flux. The solid line is the theoretical prediction.

shift can be written

$$\delta\phi = \frac{2\pi\Omega\cdot A}{h/m}, \tag{1}$$

where Ω is the angular velocity of the interference loop, A is the area vector, and m is the mass of the relevant particle: the atomic mass or, in the case of photons, $h\nu/c^2$. Since this photon "mass" is at least 10^{10} times smaller than atomic masses, the superfluid devices and atomic devices offer great potential. On the other hand, there has been fifty years of progress in measuring small laser shifts. Atomic beam devices may be limited by shot noise in the particle number involved and also are problematic thus far regarding creation of a substantial enclosed area. Helium gyroscopes have large phase shifts relative to photons, very large particle numbers and simple methods to enclose a large area. However they inherently involve cryogenic technology which is not user friendly, particularly to the targeted user communities.

A SURPRISE IN ^{4}HE

Following our proof-of-principle ^{3}He gyroscope I hoped to extend the technology to determine its practical limitations as a rotation sensor. Postdoc Tom Haard and graduate student Emile Hoskinson joined the project and were assigned the task of developing a more sensitive ^{3}He gyro. By the time Tom left, a cell had been designed which was intended to make measurements of ^{3}He Josephson phenomena away from zero pressure which was the only regime we had explored previously. Testing ^{3}He experiments is quite expensive and time consuming since submillikelvin cryostats are inherently large, complex and consume large amounts of liquid helium and researcher time. It is therefore common for us to pretest experimental cells in superfluid ^{4}He in simple pumped-bath cryostats. Much can be learned from such tests. Emile's cell used our standard 65x65 array of 50 nm apertures. Although his test goal was to determine if the displacement transducer was operational and if the apertures behaved as expected, he made a discovery which has temporarily shifted our research direction and raised scientific questions that are presently the focus of our research program. One paragraph is needed to set the stage for Emile's discovery.

I mentioned earlier that Scott Backhaus had been looking for "Josephson phase slip sound", a phenomenon I conjectured while on sabbatical leave working with Stefano Vitale in Trento. The idea was an extension of the concept of quantized phase slippage. As P.W. Anderson envisioned the dissipation process in ^{4}He, a pressure gradient will accelerate superfluid through an aperture until at some critical velocity, v_c, a quantized vortex is stochastically nucleated near some asperity. The vortex grows in size at the expense of the flow energy in the aperture and eventually passes across the aperture removing energy from the flow, causing the phase difference across the aperture to change by 2π. If the pressure gradient is maintained, these 2π phase slips will repeat at the Josephson frequency, $f_j = \Delta\mu/h$, where $\Delta\mu$ is the chemical potential difference. I assumed that such a repetitive process would lead to the creation of an acoustic wave at the same frequency and that this sound could be detected by coupling the aperture to a resonant organ pipe tuned to the Josephson frequency. However Scott demonstrated that due to the stochastic nature of the phase slip nucleation process, there is no detectable signal at f_j unless $\Delta v_c/\delta v_{\text{slip}} < 1$, where Δv_c is the statistical width of the stochastic distribution in critical velocity and δv_{slip} is the velocity drop in the aperture due to a 2π phase slip. In all of Scott's experiments this ratio was greater than unity and the sound is similar to a shot noise source rather than a periodic excitation[36]. Thus as Emile began his test experiments using ^{4}He to determine the functionality of his cell (for future ^{3}He experiments) there was no reason to think that an array of apertures would exhibit phase slip sound.

One evening in March 2004 Emile was testing his cell by applying small differential pressure steps to an aperture array. The experimental cell was immersed in a simple dewar with no sophisticated temperature regulation. When the vacuum pump was turned off the temperature of the bath and apparatus slowly drifted toward T_λ. It had become practice in my laboratory to wear head phones connected to the output of our cell's displacement gauge. This permits us to gain insight into the various environmental acoustic noise sources that might degrade our experiments. As the temperature drifted upward, just below T_λ Emile heard a faint and very brief "chirp" sound in the head phones. Could the ^{4}He in the aperture array be exhibiting some form of Josephson oscillations several millikelvin below T_λ? Our experiments for the past year have now focused on answering this and related questions.

Emile added a temperature regulation system that stabilized the temperature of the entire helium bath to ± 50 nK. The Josephson frequency is given by

$$f_j = \frac{\Delta\mu}{h} = \frac{m_4}{h}\left(\frac{\Delta P}{\rho} - s\Delta T\right). \qquad (2)$$

In his first experiments Emile did not have a method to determine ΔT during a transient so we could not test this complete formula. However, in the first instant after applying a pressure step, there is no temperature differential and $\Delta\mu$ is given solely by the pressure term which can be measured from the deflection of a diaphragm. A plot of frequency vs. that initial pressure confirmed the Josephson frequency formula for this limited case[37]. Soon after, an internal heater was added to the cell that permitted thermal measurements, giving calibration constants needed to determine the time evolution of ΔT during the flow transient. With this in hand we were able to confirm the Josephson frequency formula containing both pressure and temperature terms[38].

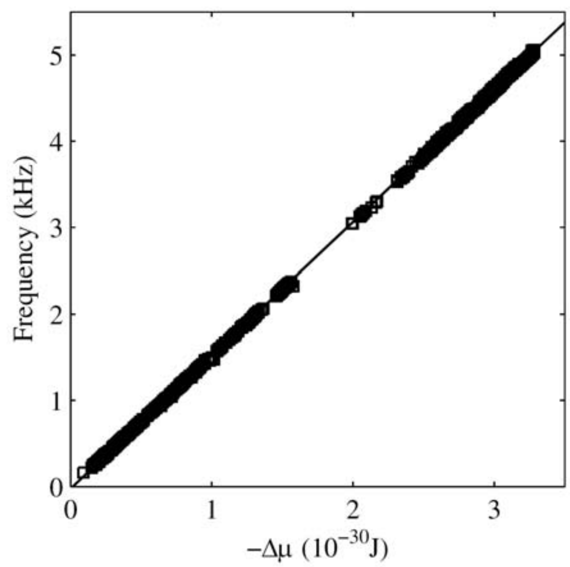

FIGURE 8. A plot of the ^{4}He quantum whistle as a function of chemical potential difference. The straight line slope is given by h^{-1} to within the experimental accuracy.

A subsequent experiment revealed the current-phase relation as the aperture array evolves from a sine-like $I(\phi)$ when ξ_4 is comparable to the aperture dimensions to a linear form when, at lower temperatures, ξ_4 is much smaller than the apertures. We have also recently investigated how the 4225 apertures can oscillate collectively while exhibiting dissipative phase slippage at the lower temperatures. At present this is the immediate focus of our research. In the near future we hope to develop a ^{4}He SQUID gyroscope. Since this device might operate near 2 K rather than the submillikelvins of the ^{3}He device, it may be possible to use a cryocooler to maintain the required temperatures. A cryo-cooler based superfluid gyroscope would not require that the user have specialized cryogenic knowledge. Perhaps we are approaching the time of creating a practical device using the macroscopic quantum properties of the superfluid state.

CONCLUSION

This paper is a personal sketch of the historical development of the Berkeley experiments on macroscopic quantum phenomena. I have focused on those experiments where the results explicitly involve Plank's constant in the macroscopic world. Since this is not a research article I have not referenced much of the work of others in this field although their contributions have been enormous and their work is referenced in the Berkeley research papers.

I hope the reader will feel some of the excitement of the discovery process that we have been privileged to experience. The superfluid states of helium have proven to be outstanding model systems to clarify principles of correlated states of matter. They can be considered the hydrogen atom of condensed matter systems because the properties displayed are often in perfect agreement with theory.

ACKNOWLEDGMENTS

The research described here has been supported continuously by the National Science Foundation. My coworkers and I are grateful for that support. In recent years we have also received support from the NASA program in Fundamental Physics.

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