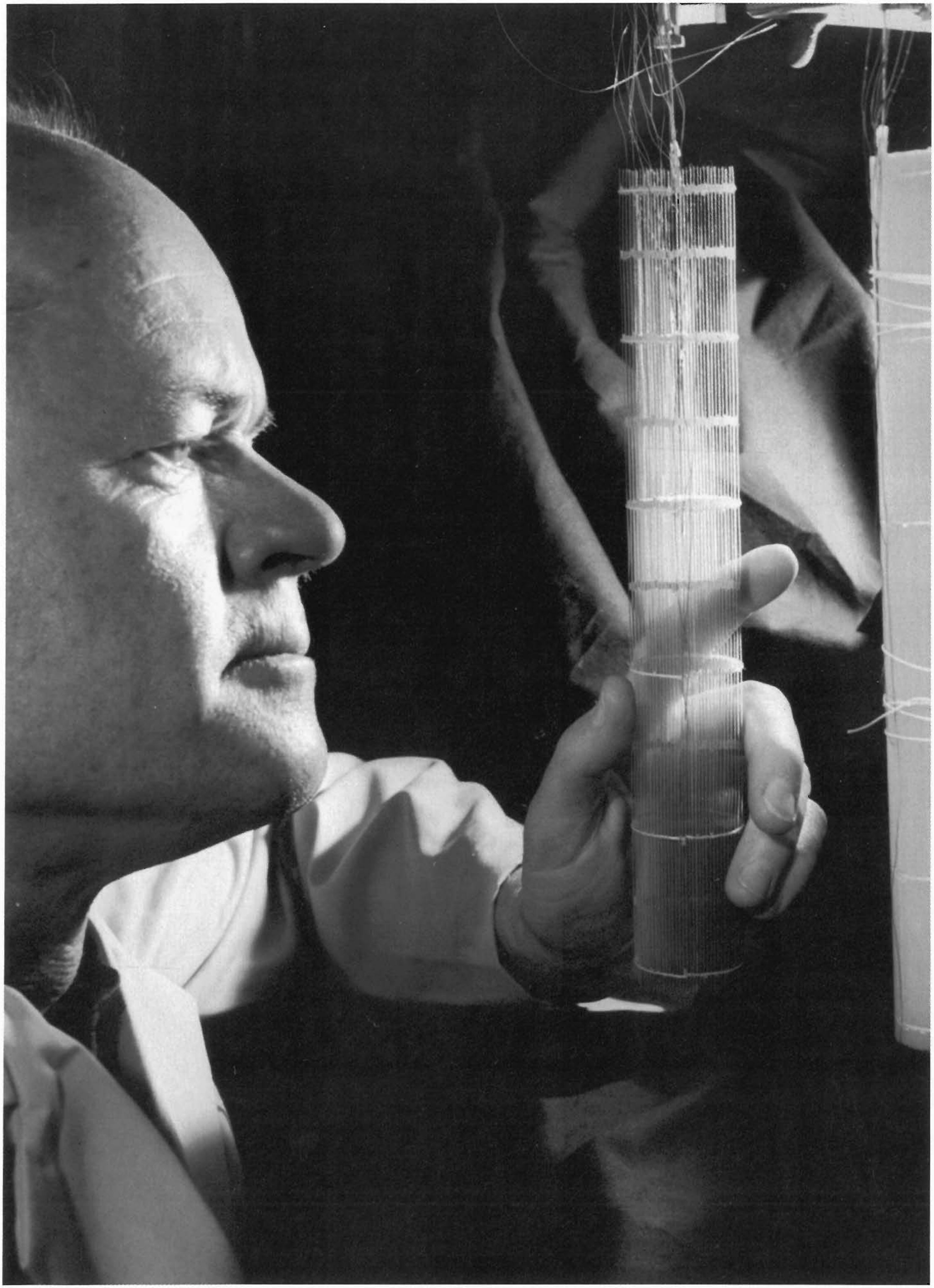
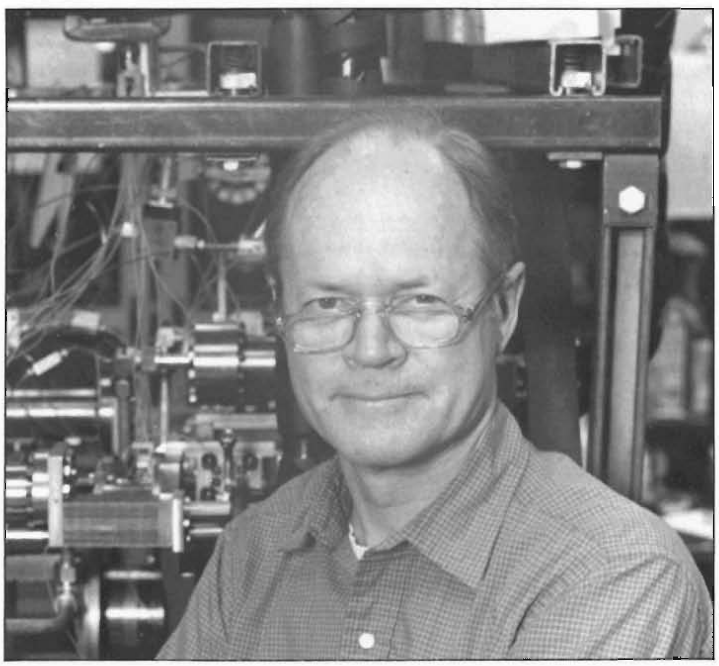


PUSHING *the* LIMITS

John Wheatley (1927-1986)

John Wheatley, one of the great low-temperature experimental physicists of the twentieth century, died suddenly this spring while bicycling to his lab at UCLA. His premature death left unfinished a large number of fascinating projects both in Los Angeles and in Los Alamos, among them the one on natural heat engines that he was writing about for this issue of *Los Alamos Science.*



As a tribute to John and his brilliant contributions to science and technology, a group of close associates shared with us their insights about the man and his achievements. What made him a great scientist? How did he succeed in carrying out high-precision experiments at such low temperatures? Why did other experimentalists frequently aim to prove him wrong? Why is research at a few thousandths of a degree above absolute zero so tricky? How did John Wheatley interact with theorists, graduate students, administrators? These are some of the topics addressed in the following round table. The participants included both theorists and experimentalists. Theorists David Pines (the current Bernd Matthias Visiting Scholar at the Los Alamos Center for Materials Science) and Gordon Baym from the University of Illinois had worked with John at Urbana in the sixties on liquid helium-3 and dilute solutions of helium-3 in helium-4; theorist Al Clogston, long-time member of Bell Laboratories and now a member of the Center for Materials Science, worked with John during the last three or four years on new ideas about nonlinear excitations in molecular systems. The theorists trusted John's physical intuition and knew they could count on his results.

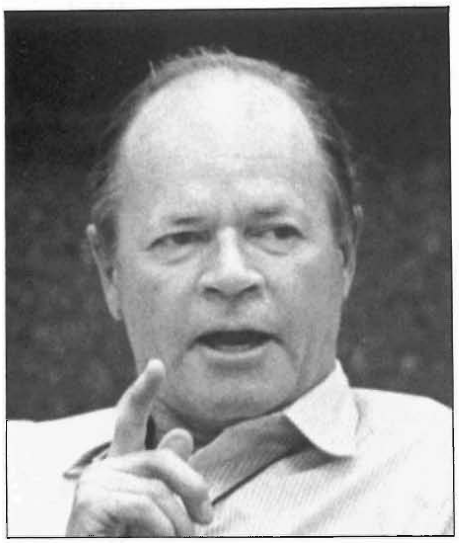
Three experimentalists at the round table got down to the nitty-gritty, giving a vivid picture of John's genius in the laboratory. Matti Krusius from Otaniemi, Finland, had worked with John during the seventies at the

University of California, San Diego, on the superfluid phases of helium-3 and at Los Alamos with John on spin-polarized hydrogen. Greg Swift worked with John at Los Alamos on superfluid helium-3 and natural engines, and Al Migliori worked with him here on natural engines and nonlinear excitations in molecular systems.

Sig Hecker wanted very much to be a participant but was unavoidably traveling. We interviewed him later, inserting his comments where appropriate. He gives a moving description of how his not-so-easy collaboration with John on establishing the Center for Materials Science grew into a strong friendship.

The result is a portrait of a man who inspired those around him by his extraordinary drive for excellence, his intense interest in science, and his joy at being in the lab doing the best experiment that could be done. Although his insistence on perfection, his intolerance of incompetence, and his confidence that he was right could often be a source of friction, he will primarily be remembered for his contagious enthusiasm and ingenious skill in pushing the limits of science.

Pines: John was the pre-eminent low-temperature physicist of his generation. He made absolutely major contributions both to low-temperature technology and to understanding the physics of the helium liquids. Between the late fifties and the mid seventies, he carried out most of the key experiments on liquid helium-3 and the dilute solutions of helium-3 in helium-4 that either provided a basis for a theoretical understanding or else confirmed theoretical predictions. He also



carried out a number of the key experiments on the superfluidity of helium-3 and missed, by really a shadow, identifying the superfluid phases.

John was not just the outstanding experimentalist in the low-temperature community; he was also its conscience. He paid attention to what other people were doing and was willing to take the time to sort out why their results were different from his own. In that respect he was unique. And he did it all with great style, verve, honesty, and a sense of humor.

John as the conscience of the low-temperature physics community is epitomized by the following anecdote. In 1964, at the Eighth International Low-Temperature Physics Meeting in Columbus, Ohio, one of the leading Soviet low-temperature experimentalists, V.P. Peshkov, presented the details of his previously announced "discovery" of the long sought-after superfluid phase of helium-3. Following Peshkov's presentation, John got up and, in the most careful, honest, objective way, pointed out what he thought were the fatal flaws in the experiment. John demolished Peshkov but not in any personal sense. He just demolished the way in which Peshkov had arrived at his temperature scale—one of the problems in low-temperature physics is knowing what temperature you're at—and then pointed out the effects that may have led Peshkov to erroneously conclude that he was dealing with a superfluid phase of helium-3. John had done experiments down to lower temperatures, he was sure of his temperature scale, and he knew he hadn't seen superfluidity.

Migliori: Later, when John in fact had superfluid helium-3 in his own lab, he didn't know it, although he was right about his temperature scale.

Krusius: Unfortunately, that one mistake probably cost him the Nobel Prize. He did most of the pioneering experiments on liquid helium-3, both before and after the discovery of superfluidity by Osheroff, Lee, and Richardson in 1972.

Pines: Another measure of John the scientist concerns the debate during the years 1980 to 1983 over the correct low-temperature specific heat of liquid helium-3. The results obtained in 1980 by a group working in Helsinki differed by some 40 per cent from the results that John and his collaborators had found in their classic work in the mid sixties. This discrepancy was of great concern because helium-3 is the benchmark liquid in all ultralow-temperature work. After the announcement of the Helsinki group's results, John wrote a long letter to the experimentalists in the field discussing all the possible things that could go wrong and all the consistency checks that were needed to do an accurate measurement. Within about a year and a half of that letter, Dennis Greywall at Bell Laboratories carried out what is likely to remain the definitive experiment. It led to results that differed from John's by about

10 per cent. No one understands to this day why the Helsinki experiments were so far off. But certainly John led the way in suggesting what could have caused an erroneous result.

Clogston: John was definitely a driving force in the low-temperature community, always stretching things to the limit. Take, for example, the Argentinean adventure.

Pines: Yes, John spent two years [1962 and 1963] founding the low-temperature group in Bariloche, a city off in the Argentinean countryside not far from the Chilean border.

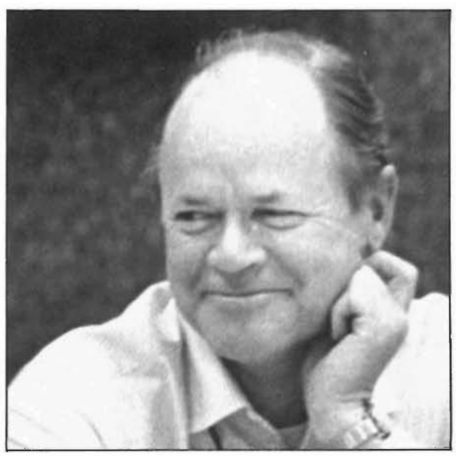
Krusius: At first the conditions at Bariloche were very primitive with hardly any electricity or water. Everything had to be started from scratch. They even had to build the liquefiers to make liquid helium and liquid hydrogen.

Pines: He went there because he liked the people, liked the adventure, and liked the opportunity afforded by the research atmosphere of Argentina at that time. Before he left, he set up a two-way radio on the roof of the Urbana physics building so he could talk regularly to his colleagues there and order much-needed equipment for the lab in Bariloche. On his return he used the set to stay in touch with the people in Bariloche. John was thoroughly successful in his Bariloche enterprise. A number of his former students continue to work there, and the lab is recognized as a center of excellence in the South American experimental physics scene.

Krusius: Even for a scientist of John's stature, a highly recognized international standing does not come automatically. John won international recognition and had many, many friends abroad because he cultivated and worked with his foreign colleagues. In 1975 he received the Fritz London Memorial Award, the highest recognition of the international low-temperature physics community, and an honorary degree of Doctor of Science from the University of Leiden. In 1980 he was appointed to the Academy of Finland. John really valued those recognitions. The Academy of Finland, which consists of only some thirty members, is small enough that if one member says he's interested in studying the use of hydrogen gas as fuel for the diesel cycle, he'll be whisked off in a helicopter to an icebreaker to see large-scale diesel engines in action.

Science: *When did John begin his work in low-temperature physics?*

Pines: He came to the University of Illinois in 1952 as a nuclear physicist, working on paramagnetic resonance and nuclear magnetic moments.



Krusius: John decided that the proper way to study the interactions of magnetic moments in condensed matter was to polarize the moments using low temperatures and high magnetic fields. So he had an immediate need to get acquainted with low-temperature techniques. In 1954 he spent a year in Leiden at the Kamerlingh Onnes Cryogenics Laboratory, the renowned birthplace of low-temperature physics. During his stay he undoubtedly became acquainted with the concoction of myths and cookbook recipes that made up low-temperature technology. When he returned to Illinois, he set out to correct this situation by methodically establishing the basic techniques of present-day low-temperature refrigeration and thermometry.

At first he used the adiabatic demagnetization of cerium magnesium nitrate as a cooling method for all his low-temperature work. When helium-3 became available in the late fifties, he added helium-3 evaporation as a precooling step and was able to extend the low-temperature limit by a factor of 20, down to a few millikelvins. At the same time he developed the whole technology needed to work at these low temperatures. For example, he compiled a list of materials according to their magnetic susceptibility at low temperatures so one could use materials for the apparatus that would not interfere with the measurement of very small magnetic moments.

Science: *Tell us about the atmosphere at Illinois in the fifties and sixties.*

Pines: John and I both came to the University of Illinois in 1952—as did Hans Frauenfelder and Francis Low. John Bardeen, Fred Seitz, and Charlie Slichter were already there. Later, in the sixties, Gordon Baym, Tony Leggett, and Chris Pethick came. It was a remarkable group; we very much enjoyed talking and working together. At first, during his nuclear physics phase, John Wheatley sat somewhat apart, but soon his work on the helium liquids made him a central figure in our discussions.

Science: *Why was helium so interesting?*

Pines: Helium-4 was always of great interest to both the experimental and theoretical low-temperature community because it remains a liquid down to the lowest temperatures—as long as you don't squeeze it—and because helium-4 becomes a superfluid, with all sorts of fascinating properties, below 2.19 kelvins. When helium-3 became available in large quantities in the late fifties, the attention of both theorists and experimentalists turned to the properties of this new quantum liquid. Because helium-4 is a spinless particle, a boson, it condenses to a superfluid in which all the atoms are in the same lowest energy state. Helium-3 with a spin of ½ is a fermion and can have only one particle at a time in a given state; hence it was expected that the quantum liquid properties exhibited at very low temperatures would be quite different from those of helium-4.

In the early days Los Alamos had more

helium-3 than anybody else because of the work done with tritium for the Super [the first H-bomb design], and the Lab also had a very active low-temperature group. It is this group that John eventually joined.

Baym: Of course it was no accident that helium-3 research began in Los Alamos in the late fifties. The half-life of tritium, the parent of helium-3, is 12.5 years, so it was natural that by the mid fifties substantial stocks of helium-3 would have begun to build up. Landau's paper on his Fermi-liquid theory of helium-3 was published in 1957, just one tritium half-life after the end of the Second World War.

Pines: The theoretical challenge was to understand why Landau's theory of Fermi liquids worked as well as it did for helium-3. The experimental challenge was to achieve temperatures low enough to cause the strongly interacting system of fermions, helium-3, to behave like a collection of weakly interacting elementary excitations. This is the not-so-obvious prediction of Landau's model. The elementary excitations in the model are helium-3 quasiparticles and quasiholes near the Fermi surface, analogous to the particle and hole excitations of electrons in a metal. For helium-3 at a low enough temperature that quasiparticle and quasihole excitations govern its properties, the model predicted that the specific heat would vary linearly with temperature, the spin susceptibility would be independent of temperature, and a collective mode, called zero sound, would arise. Zero sound is a density wave that propagates by means of the forces between the atoms, rather than by collisions maintaining local equilibrium, as in ordinary sound.

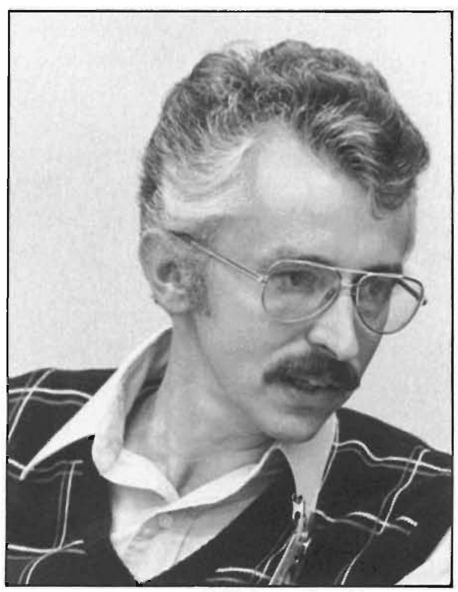
Science: *What were John Wheatley's contributions to the study of helium-3?*

Pines: First he developed the technology to cool to temperatures below 100 millikelvins, as Matti described. Once he had the technology, his work on helium-3 took off. John and his collaborators demonstrated that the transport properties—thermal conductivity, viscosity, ultrasonic attenuation, and spin diffusion—of liquid

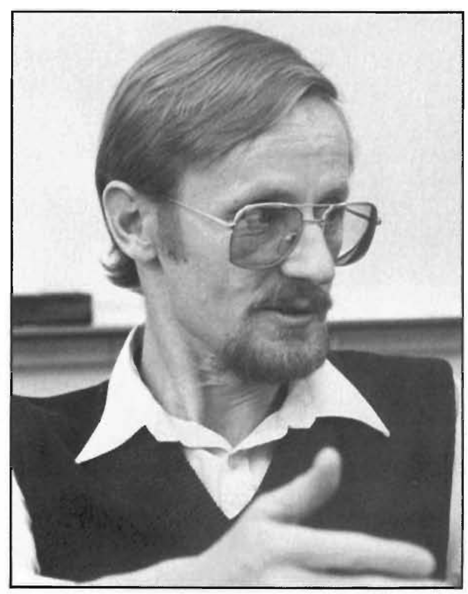
Round Table Participants

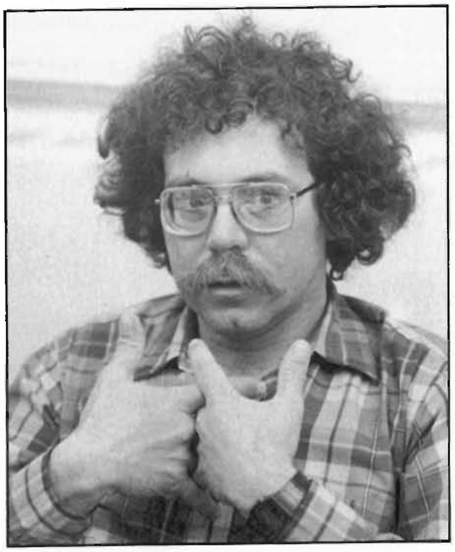
John's character was dominated by a systematic drive for excellence in all things.
—Greg Swift

John was always the first to perform a new type of experiment. He would quickly set the benchmark, and it would be a correct one.
—Matti Krusius



When any technical idea was brought up, John wouldn't let us continue until he understood every aspect of it. That's what made him such a pain and so beautiful too.
—Sig Hecker

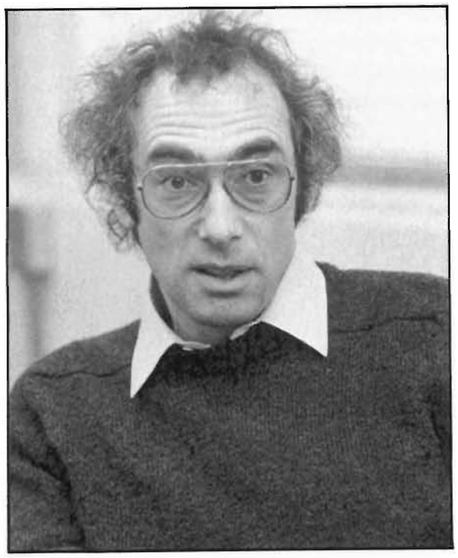


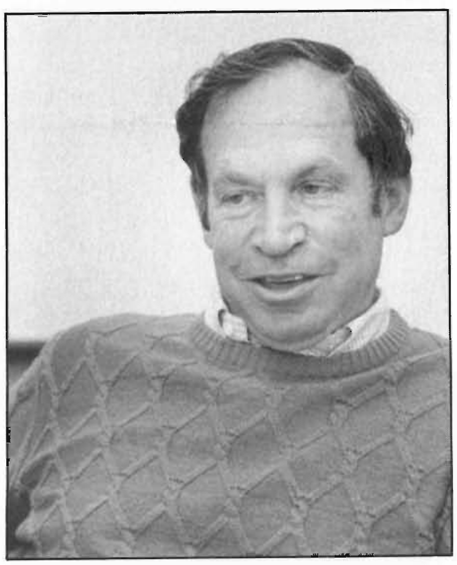


". . . The irreversibility will be the thing that makes the engine work." *He liked that idea because then the engine, the natural engine as he called it, wouldn't have a single extra thing wrong with the technology.*
—Al Migliori

John Wheatley developed his profound understanding of his experiments through mechanical models and simple theories that captured the essence of the physics involved.
—Al Clogston

By understanding the numbers, he was always way ahead of the theorists; he could tell quickly whether a theoretical guess was right or wrong.
—Gordon Baym





John was not just the outstanding experimentalist in the low-temperature community; he was also its conscience. . . . In that respect he was unique.
—David Pines

helium-3 agreed with the predictions for a Fermi liquid. John was also the person who directly found zero sound in helium-3. Wilks had done an indirect measurement based on acoustic mismatch, but Wheatley, Abel, and Anderson made the direct observation in 1966.

Krusius: John was always the first to perform a new type of experiment. He would quickly set the benchmark, and it would be a correct one. This happened so consistently you often heard at conferences that the driving motivation for a later experiment had been a feeling that the early Wheatley experiment could not possibly have been right.

Swift: The basic principle here is that John was always right. Low-temperature experiments have many pitfalls, the most important being the determination of an accurate temperature scale. John was way ahead of everyone: he consistently worked sixty-hour weeks, had a fantastic memory for everything he did in the lab, and outlined his experiments in meticulous detail.

Krusius: An illustration is his work on the temperature scale in the millikelvin range. All through the years John had collected his own measurements on the absolute temperature scale, which were done with cerium magnesium nitrate. He then used them to finally summarize the helium-3 superfluid transition curve as a function of the externally applied pressure and absolute temperature. Many groups have since employed different techniques, but his initial tabulation has resisted challenges remarkably well.

Clogston: I think of John Wheatley as one of the world's premier experimentalists. He certainly wasn't a theorist; I'm not even sure he had a very high regard for theory. But he certainly interacted, David, with you, Gordon, and John Bardeen very deeply and intensively. What was the nature of that interaction?

Pines: Whenever John observed anything new and puzzling about helium-3, he'd climb up to the lair of quantum-liquid theorists two floors above his lab to ask questions. John always had a strong feeling for the essence of what was happening and a real interest in finding the simplest theoretical description. On the other hand he never got involved with theoretical technology. For instance, he understood the theoretical construct of a Fermi liquid but never learned the mathematical details at the heart of the theory.

Baym: The most intense interaction was on dilute solutions of helium-3 in superfluid helium-4; we were back and forth almost daily. John was always trying to force us as theorists to explain in his terms how the mixture was working. He insisted on very detailed explanations. By understanding the numbers, he was always way ahead of the theorists; he could tell quickly whether a theoretical guess was right or wrong.

Pines: The helium-3/helium-4 mixtures were a good system to work on because we understood helium-4 completely, and we could add helium-3 a little at a time, really checking theory against experiment as we went along. You could study a range of densities for helium-3 that's impossible to obtain in pure gaseous helium-3. Because of John Wheatley, Gordon, John Bardeen, and I attacked a fascinating theoretical puzzle, the nature of the effective interaction between helium-3 atoms in the helium-4 background at temperatures so low that the helium-4 behaves like a mechanical vacuum.

Science: *What do you mean by a mechanical vacuum?*

Clogston: Helium-3 binds more strongly to helium-4 than to itself, making it energetically more favorable under some circumstances to be in a mixture than to separate into different phases.

Migliori: As a result, the amount of helium-3 that will dissolve in helium-4 at atmospheric pressure can be as large as 6 per cent down to absolute zero temperature. At the same time, superfluid helium-4 has no viscosity, so helium-3 moves nearly as if nothing is there. It acts like particles in a vacuum, except, since you're in the liquid phase, you have a lot more atoms present per unit volume.

Baym: An example of John's influence on theory arose from his measurements of the thermal conductivity and spin diffusion of dilute solutions of helium-3 in helium-4. It appeared impossible to construct any theory of the effective interaction that would explain both of those experiments. From the discrepancy with the experiments—and John stood by his results—we discovered that the solutions to the Landau kinetic equation we were using were in fact not very accurate. John's measurements really inspired the subsequent work that eventually led to the exact solution. If John had not done the experiments and pushed on the theorists, that theoretical advance would likely not have been made for quite a while.

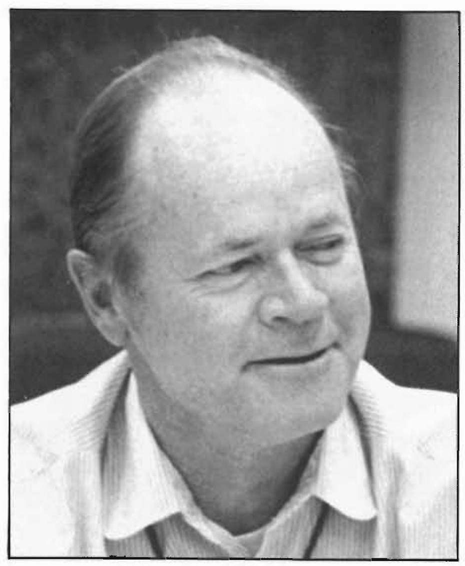
Migliori: John really understood what it meant for helium-4 to act as a mechanical vacuum for the helium-3 particles.

Baym: He realized that if you cooled by evaporating helium-3, the binding of helium-3 to helium-4 allowed you to achieve a higher vapor pressure at a given temperature than you could in the ordinary vacuum. If you just evaporated helium-3, you could cool down to about a third of a kelvin, but if you evaporated helium-3 into helium-4, you could go to much lower temperatures. That's the principle of the dilution refrigerator, which John developed to a practical device.

Swift: John was able to get a factor of a hundred lower in steady-state low temperature with the dilution refrigerator. Prior to this development work, the lowest continuously available temperature was about 0.3 kelvin, produced by evaporating helium-3 into a vacuum. London suggested the principle of cooling by diluting concentrated helium-3 in helium-4, but John engineered it into a practical reality, using heat exchangers, distillation units, and pumps to circulate the helium-3 continuously. This was an extremely important technological development. A factor of 100 reduction in temperature is as important in condensed-matter physics as a factor of 100 increase in energy is in particle physics.

Pines: In some ways John was more excited about his work on dilution refrigerators than he was about having sorted out the low-temperature experimental properties of helium-3. He worked very hard on the theoretical papers he wrote in connection with the dilution refrigerator. Right before he left Urbana in 1966, he had a refrigerator running, and he, of course, developed them further after he left. In 1970 during a symposium talk on experiments of the future, he spoke about ultralow temperatures of 30 millikelvins. Now, thanks to John's dilution refrigerators, one can reach those temperatures fairly easily.

Krusius: His dilution refrigerator chopped the continuous-cooling frontier down to 4.5 millikelvins. Dilution refrigerators



now go down to 2 millikelvins, but they require much larger pumps than John had in the early work.

Pines: When you combine the dilution refrigerator with demagnetization techniques, you can get down another factor of 10 to about 0.2 millikelvin.

Swift: But remember, there's a big difference between dilution refrigeration, which is continuous, and demagnetization, which is one-shot.

Baym: Dilution refrigerators are now being used at the high-energy laboratories, including LAMPF and Brookhaven, to cool targets to very low temperatures for polarized target experiments.

Getting back to John, when he left Urbana he had a very strong urge to get into a different field; he wanted to work on geophysics. However, superfluid helium-3 was discovered in 1972, and then John couldn't break away.

Science: *How did John happen to miss the discovery of the superfluid phase?*

Krusius: Ironically, John had made measurements below the superfluid transition temperature earlier but missed identifying it because he had his own inimitable and sometimes stubborn way of doing things. He was a very organized and meticulous worker, but he was sometimes reluctant to resort to the most modern type of equipment. Doug Osheroff discovered the transition early in 1972 during his compressional adiabatic cooling experiments at Cornell. Osheroff found a glitch in the pressure as a function of time—a very small glitch, only a few per cent of the total. This tiny glitch was the superfluid transition everyone had been looking for. About a year and a half before, John had also been developing adiabatic compressional cooling to obtain low temperatures and to look for the transition. During those experiments, rather than reading the pressure as a function of time from a strip-chart recorder attached to a pressure transducer, John had Rich Johnson, the graduate student doing his thesis work on this experiment, sit on a stool and shout out numbers from a pressure gauge with a needle. These discrete points did not show an obvious glitch. Afterwards, knowing where the transition occurred, they went back and plotted their data above and below the glitch and saw the transition.

Swift: After Osheroff's discovery John became very serious about making measurements on these new superfluid phases. He brought Matti from Helsinki to La Jolla as a postdoc, and, together with a couple of students, they built a cryostat that used a dilution refrigerator as the precooler and

JOHN WHEATLEY—CAREER HIGHLIGHTS

EDUCATION

1947	B.S. in electrical engineering, University of Colorado
1952	Ph.D in physics, University of Pittsburgh

THE URBANA YEARS—1952-1966
(University of Illinois)

Research

Paramagnetic resonance and nuclear magnetic moments, low-temperature refrigeration and thermometry, development of the helium-3 dilution refrigerator, transport and physical properties of liquid helium-3

Honors and Special Appointments

1954-55	Guggenheim Fellow and Fulbright Research Scholar, the Netherlands
1962-63	Fulbright Research Scholar, Argentina
1965-66	Member, University of Illinois Center for Advanced Study
1966	Simon Memorial Prize

THE LA JOLLA YEARS—1966-1981
(University of California, San Diego)

Research

Low-temperature phases of helium-3, dilution refrigeration, magnetic properties of dilute alloys, development of the point-contact SQUID, properties of superfluid helium-3, liquid working fluids in heat engines

Honors and Special Appointments

1968	William Pyle Philips Lecturer, Haverford College
1969	Loeb Lecturer, Harvard University
1975	Member, U.S. National Academy of Sciences
	Doctor of Science, honoris causa, University of Leiden
	Ninth Fritz London Memorial Award
1980	Academician, Academy of Finland

THE LOS ALAMOS YEARS—1981-1985
(Los Alamos National Laboratory)

Research

Conventional and natural heat engines, superfluid helium-3, Rayleigh-Bénard convection in mixtures of helium-3 and helium-4, spin-polarized hydrogen

Honors and Special Appointments

1983	Fellow, American Academy of Arts and Sciences
1984	Fellow, Acoustical Society of America
	Distinguished Graduate Award, University of Pittsburgh

THE LOS ANGELES YEAR—1986
(University of California, Los Angeles)

Research

Nonlinear localization of vibrational energy

Honors and Special Appointments

1986	First Joint Fellow, UCLA and Los Alamos
1986	Chosen to be the first holder of the President's Chair at UCLA, a new position recognizing the most outstanding faculty member on campus

adiabatic demagnetization as the final cooling step. It was basically the Helsinki design, but it's remarkable that it worked just fine the first time they tried it. At 1 millikelvin a heat leak of only a billionth of a watt can be disastrous, so this is difficult work. Their first experimental run produced about ten publications, half of them in *Physical Review Letters*. This at a time when most experimentalists in the field were struggling just to get cold.

Krusius: John studied superfluid helium-3 systematically all through the seventies, producing one of his most important contributions to physics.

Baym: The superfluid phases of helium-3 are interesting in that they resemble a superconductor, except that the Cooper pairs have one unit of angular momentum instead of zero, which makes the description of the ordering much more complicated. There are two phases, called A and B. The A phase has an anisotropy axis, making it like a quantum liquid-crystal. A lot of John's work was on the dynamics of this anisotropy axis—orienting it with magnetic fields and such and using the anisotropy of zero-sound attenuation to see what was going on.

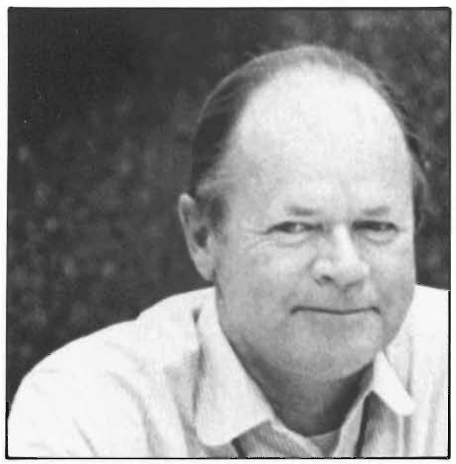
Pines: It seems that John began his study of superfluid helium-3 by, once again, improving the experimental technology.

Krusius: In particular, John was one of the key persons to develop the point-contact SQUID, a superconducting quantum-interference device that can measure very small magnetic fields and voltages.

Migliori: John used the SQUID to measure magnetic fields as well as temperature in his superfluid helium-3 experiments. A SQUID voltmeter was a good way of making a low-power, precise measurement of the magnetization curve of a dilute magnetic salt, and if you understand the physics of the salt reasonably well, its magnetic susceptibility defines the temperature scale. So the point-contact SQUID became a very important tool for determining temperature.

Krusius: John was probably the first person to use the SQUID for that. He also, I think, was the first person to use the SQUID to measure thermal noise in a resistor, letting that define a temperature scale also.

Migliori: The gold-iron SQUID thermometer is also a nice tool, sufficiently well developed by John's graduate students that it was written up in an instrument publication. That tool is used here at Los Alamos to study Rayleigh-Bénard convection in dilute helium-3/helium-4 solutions. These experiments yield signal-to-noise ratios equal to that of a compact disc, that is, 90 decibels. As a result, one can measure a long series of period doublings as the system undergoes a period-doubling transition to turbulence. These



are the finest measurements of the convecting state—in how it's initiated and how it evolves—and it's a real measurement, not a computer simulation.

Pines: The point-contact SQUID is now being used to measure electrical currents in the brain through the tiny magnetic fields that are generated. A number of people at Los Alamos and elsewhere are interested in trying to measure magnetic brain waves and to use a magnetic map of the brain for medical diagnosis.

When John went to San Diego after leaving Urbana, he not only devoted himself to research on superfluid helium-3 but founded the SHE [Superconductivity, Helium, and Electronics] Corporation. He did this, in part, to exploit some of his own inventions and also to make available excellent low-temperature equipment in a way he knew that no commercial company was going to do.

Swift: Two other low-temperature physicists were also founders of SHE, but it was John who guided it on a daily basis. Their main products have been dilution refrigerators and SQUIDs. John's technical expertise was indispensable in getting SHE started. I think the University faculty frowned on this involvement. It looked to them like John was developing these technologies at the University and at government expense and was trying to profit from them personally. Actually he never made a cent. A lot of his former students and postdocs work there now. That was one way he transferred his low-temperature technology from the lab to SHE.

Pines: At heart, John was a kind of missionary. Although he was a very private person, in another sense he was not at all secretive. He didn't develop these wonderful techniques to keep to himself and his immediate associates. He was always willing to share. He not only built the Argentinean low-temperature community but also played an important role in building a major low-temperature community in Finland.

Science: *What about John's work at Los Alamos these last five years?*

Krusius: Before he came to Los Alamos, he was interested in doing something new. He began thinking about thermal physics with heat engines, but he had a feeling such work could not be done at UCSD.

Migliori: When I first talked to John in San Diego about his decision to come to Los Alamos, he expressed this love of technology that has been a theme throughout his life. His interest in heat engines has a similar quality to his work on the dilution refrigerator. He wanted to do technology, to make things that did something. For that reason the acoustic engine work here at Los Alamos was one of the high points in his life.

Pines: His recent work on heat engines was an outgrowth of his profound working knowledge of thermodynamics and heat cycles and what you could do with them. Having solved essentially all those problems in low-temperature physics, he wanted a new major challenge. He went back to the work of the great thermodynamicists and engine creators of the nineteenth century, such as Carnot and Kelvin, and took a fresh look at that technology to see what you could do.

Clogston: I had the impression, working with him these last three or four years, that he always based his thinking on some kind of classical model. Perhaps he was one of the last great classical physicists.

Migliori: He loved these engines, especially the acoustic engines, more than anything else because the physics could be understood on the basis of classical thermodynamics. A normal person could understand it. Although there was nothing quantum mechanical about acoustic or natural engines, they were, nevertheless, a completely new development, and they were sufficiently rich and complex that they challenged everyone's understanding.

Science: *What was the new idea behind natural engines?*

Swift: In traditional heat engine designs, the idea is to minimize irreversible processes because they lead to inefficiencies.

Migliori: Anything you build is going to be irreversible, so from the traditional point of view, there'll always be some process that messes you up. John said to himself, "I'm going to make that irreversibility work for me. The irreversibility will be the thing that *makes the engine work*." He liked that idea because then the engine, the natural engine as he called it, wouldn't have a single extra thing wrong with the technology.

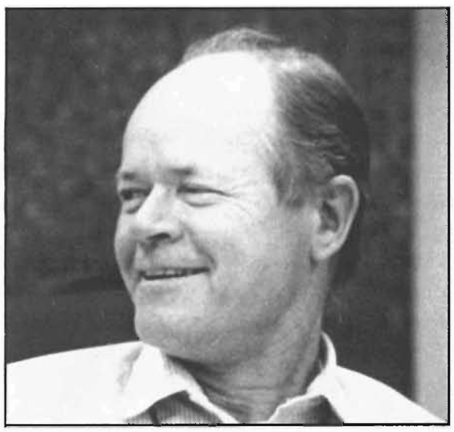
Swift: It's like taking a liability and turning it into an asset.

Migliori: The acoustic engine was the first natural engine John developed. Merkli and Thomann and then Nikolaus Rott had discovered the important acoustic engine principles, but John was trying out his new principle to see if it had absolute global importance.

Pines: In the summer of eighty-three, he organized a meeting to see whether people in various parts of physics would agree with him that this was a whole new approach to understanding engines.

Baym: I had pointed out to John the relation of his idea to instabilities in stars, and eventually he and Art Cox wrote a paper for *Physics Today* on the connection.

Pines: There are also a certain number of natural engines in your body, and that fascinated John. He wanted to see if he could make engines that operate at a molecular level.



Migliori: I think one of his chief motivations in all this work was his desire to make engines work well.

Swift: He liked the promise that something practical would come out of the research. Eventually he started using a liquid instead of a gas as the working substance in a heat engine—an idea that had lain dormant for fifty or sixty years. This idea led us to the liquid sodium acoustic engine, which we're working on now. John recognized all along that liquids were good things to work with and kept his eyes open for opportunities.

Migliori: The sodium acoustic engine is an example of taking an idea and implementing it with exactly the right working fluid. But it takes about fifty years to get an engine working properly in the economic sense—and you have to compete with existing technology. So even though the sodium engine has no moving parts, it's going to take time to yield a big payoff.

Clogston: The need for efficient engines in space is so great that the payoff may come sooner.

Pines: Well, SDI may push it up a little.

Science: *So the liquid sodium engine is truly a capstone on a prolific experimental career. What influence did John's experimental style—his way of doing things—have on other people?*

Swift: We need to talk about John's graduate students, because that's what his experimental career was all about.

Migliori: John had great skill getting students interested in topics he wanted to pursue. He displayed excellent taste, picking out topics that were important, and then, through force of personality or charm or just brute force, got people to work on them.

Swift: All the measurements in the fifties, sixties, and seventies on the properties of liquid helium-3 were made with John tightly controlling a handful of graduate students. That was the key to his great productivity. He was in the laboratory with them day and night, calling all the shots. They were reading the meters, and he was writing the numbers in the lab notebook. When he came to Los Alamos, he brought a number of students with him from the University of California. We still have a handful here. In the last few years, though, he started to let them take more initiative. He was mellowing.

Pines: You know, John had a killer instinct when he worked on something. He really wanted to get at it and get there fast. But it's a delicate point. When you want to get something done, the very best thing is to do it yourself. Sometimes, though, you need help, and you enlist a graduate student or a postdoc. If you want the answer in a hurry, you are on that person's neck every moment of the day. On the other hand it's not a very good way for a student or a postdoc to learn. The mellowing that Greg referred to was John's willingness to wait another day or two for the answer and let people make their own mistakes.

Krusius: I think John had a very positive interaction with his students. In low-temperature work you can lose an enormous amount of time if you do things wrong. John would have the student think about the measurement and come up with a proposal. Then they'd go through the plans together, and he'd press on the things he thought wouldn't work. The student would see that his proposal might not be a secure way of starting the experiment. That guidance was very useful. Otherwise, the student might waste a year or two.

Migliori: A simple example is using too much current through a thermometer so that it heats itself. That's a subtlety that might be missed by a new student.

Krusius: John's approach with his graduate students changed after his heart by-pass operation three and one-half years ago, because time became immensely valuable to him. He really wanted to do physics on his own terms and not have too many people involved. John was the most organized person I know about doing work in the physics laboratory. When he came to the lab at seven or eight, he had a list in his mind of what he was going to do that day, and he really wanted to carry out all the things on that list. He didn't have much time for discussions. One graduate student in La Jolla solved this problem by bicycling home with John in the evening. During that bicycle ride he'd talk about his experiment and get advice for it.

Swift: In later years John would respond to a student's question by giving him as much time as he needed, but John would never seek out students to make sure they were doing the right thing that day.

Science: *What about his family?*

Swift: There was his wife, Martha, and two sons. His career would not have been possible without Martha because she devoted herself entirely to making his life easy. She was the foundation that gave him the freedom to do all the great things we remember him for.

Science: *Was his whole life devoted to work?*

Swift: No. Although he did spend some of his weekend time at work, a lot was spent with Martha riding bicycles on longer trips, or sometimes hiking or skiing. He had a passion for bicycling and, years ago, a passion for his motorcycle.

Clogston: That brings to mind the time in 1976 when John drove all the way from La Jolla to Urbana on his motorcycle and arrived just in time for a party honoring John Bardeen.

Pines: It was quite a dramatic event. We were holding a symposium on new directions in condensed-matter physics to honor Bardeen's retirement. John had allowed himself just enough time to make the trip by motorcycle. He appeared at the University about an hour and a half before the meeting was to begin, having driven through not one but two blizzards. But he loved it; he was so excited to have brought it off and to have arrived on time. That exhilaration of being out on the edge with the unknown is what attracts most of us to physics and keeps us there. John loved getting to lower temperatures than anyone else and to sort out tricky experimental aspects that might cause someone else to slip up. He loved living on the edge with his motorcycle, and he loved pushing himself on his bicycle.

Photograph courtesy of the Wheatley family

John and Martha Wheatley congratulating their son Bill at his wedding.

Swift: The only thing that kept John from riding his bicycle every day was substantial snow or ice on the road. A couple of years ago, he took his bike with him to an Enrico Fermi Summer School in Italy and rode it from the Milan airport to Varenna—perhaps forty miles or so. During the two weeks of the school, he rode it around Varenna every day, and then he rode it back to the airport. That was pretty good for a fellow in a foreign country whose only fluent words in the native language were "more ice cream, please." At Los Alamos, John bicycled to work every morning, then home for lunch a little before noon, back in after lunch, and then back home again about six.

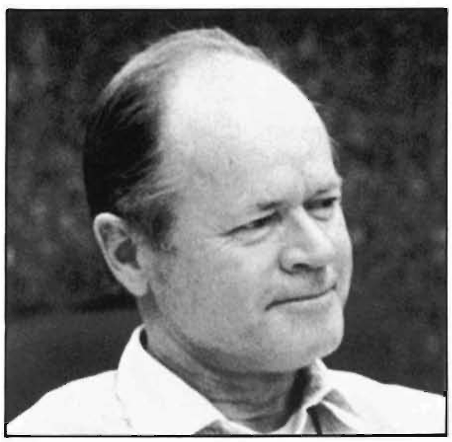
Pines: John never liked to feel hemmed in. When he was at Urbana, he and his family lived in St. Joseph, a rural village about eight miles away. When they went to La Jolla, they didn't live near the beach; instead, they lived inland about ten miles.

Science: *Let's talk about John's impact at Los Alamos. I understand Jay Keyworth was responsible for bringing him here.*

Pines: Jay certainly played a major role. John called Bill Keller (then head of the experimental low-temperature physics group at Los Alamos) to say that he might be interested in moving to Los Alamos because of its strength in low-temperature work and its possibilities for his developing technological interests. Bill told Jay, who then launched a major campaign to secure the funds and space needed to attract John to Los Alamos. John would never have come without this kind of all-out effort.

Hecker: I remember Jay saying Wheatley must be gotten at any price because he did

really high quality research.
Pines: John was pleased as punch with the space he had at Los Alamos and the possibilities of technical help. Every time I visited his lab, John would give me a guided tour and show me one more room and one more group of students doing another set of experiments. The move to Los Alamos was very liberating for him. He felt he could move out in a whole set of directions at once. This was simply not possible at La Jolla because of the lack of physical space as well as a lack of psychological space.
Hecker: John was incredibly protective of space. Part of his dowry in coming to Los Alamos was a good part of the old cryogenics building.
Swift: He had a dozen people—students, postdocs, staff members, technicians—working with him here on a whole set of problems that ranged from superfluid helium-3 to liquid Stirling-cycle heat engines. He spent most of his time managing, bringing his wisdom and good judgment to bear on the problems. It was incredible the way a little time with John could help point you in the right direction.
Clogston: John certainly brought an element of excellence and drive to Los Alamos that I think must have been unique. Also, John was one of the founding fathers of the Center for Materials Science. He worked closely with Sig and a few other people, and he maintained an enduring interest in the Center.
Swift: All John really wanted was to do physics. He'd do anything to get that to work, and, in the long run, he thought the Center would help him there.
Clogston: About two years ago he was made a member of the Center and that pleased him enormously, especially since, with his support coming from the Center, he could fund another postdoc. Again, that was exactly what he wanted.
Hecker: When we decided to start the Center for Materials Science, I wanted John on the internal advisory committee. His name would provide instant credibility with a large part of the solid-state, condensed-matter physics community. John somewhat reluctantly agreed, and then, for the next three months, I was sorry I'd asked. He just gave us hell. He made our meetings take twice as long as we anticipated because he was always a stickler for detail. When any technical idea was brought up, John wouldn't let us continue until he understood every aspect of it. That's what made him such a pain and so beautiful too.
Science: *Did he have definite ideas about the Center?*
Hecker: Absolutely! Early on, I gave what



I thought was an excellent seminar explaining my vision of the Center and emphasizing the sort of equipment we'd have, the type of building, and all kinds of other grand things. John thought it was a dreadful talk. The only thing that mattered to him was to get the best people; he assumed everything else would follow. In that sense he was quite an idealist. I, on the other hand, had to be a realist, because to build a center you have to know how to fit it into the existing structure. Eventually I realized just how valuable John was. I learned from him that you have to insist on excellence and insist on the quality of people. We had many disagreements, but gradually we learned from each other how to implement the Center, and John began to recognize that no one is going to just throw a million dollars at you.
Clogston: The Center was built adjacent to John's lab. Agreements were finally reached about how space was going to be shared, but I must say I detected no signs of mellowness whatsoever in John during those discussions.
Hecker: Even before that, in my eagerness to get the Center started, I tried to convince everyone we could start in a corner of the warehouse. That was the only space available at the time where we could build laboratories. Again, John thought that was a dreadful idea. He wanted the Center to be around him so he could interact closely with the people of quality that the Center was meant to attract. He knew he was being very selfish, and my first reaction was somewhat negative. I never expected to get space close to where he worked, but John helped make it happen. I remember his words well: the goal was "to build an intellectual community in materials at Los Alamos and have that community in a place where you get people rubbing elbows." The area close to his lab was clearly the right place since it's the site of a large share of the condensed-matter physics at the Laboratory.
Pines: I think John had no interest in exercising power for the sake of power. He had a very clear image of what the Laboratory could become—just very little opportunity to put those ideas to work. I'm not totally clear why John decided to return to an academic environment, but I've been told he felt he needed another army of graduate students.
Migliori: I think the key words are "army" and "graduate students."
Swift: Many people asked him why he was leaving, and I've collected seven or eight different answers. I think this is just part of his privacy. I don't think he wanted anybody to know why he wanted to leave.
Hecker: He *was* very unusual that way; he'd think things out totally beforehand. He came to me for advice only after he had made his decision to leave Los Alamos.
Migliori: Originally, John came to Los Alamos because he felt it was a good place to do technology. Eventually, though, he was disappointed that a few of the hoped-for services never materialized. When he

found that he had to go outside for such things as electron-beam welding and plastic molding that were supposedly readily available at the Laboratory, some of his enthusiasm diminished.

Krusius: One reason he left was that, in the end, he realized he was a university teacher—an important part of his life that he missed.

Hecker: When we discussed his move to UCLA, he did say that maybe he was meant to be an academic person. Even here at Los Alamos, he ran a lot of his shop as if it were a university. He came, in part, because he felt he was a technologist and the Laboratory was a fantastic place to do technology. When he was ready to go back to the University, he said that maybe he was more physicist than technologist after all. He still felt Los Alamos was a fantastic place to do technology, but he wasn't quite clear what his role in that ought to be. Also, he missed the academic life and freedom. For instance, he always hated the fence around the site where his lab was located. He'd say, "Sig, tell me one thing. When are you going to get rid of this fence?" It turns out the fence went down about a year or so ago, not because of anything I did, but because of the process of fixing up the Center for Materials Science. When the fence went down, John was delighted. It's a pity that now the fence is gone, John is gone also.

Migliori: There were certainly many reasons for his leaving. Another was that he'd fought so long and hard over many issues that most of his blue chips at the Laboratory were gone. Of course, one of the fascinating things about John was that he argued a lot, but mostly he turned out to be right.

Swift: When John formed an opinion, it was very carefully thought out, and he knew he was right.

Hecker: After deciding to go to UCLA, John asked my advice on how to break the news and how to restructure his relationship with the Laboratory. I spent a lot of time with John carving out the idea that he proposed to Don Kerr, our Director, to become the first University of California-Los Alamos fellow. I wanted John to maintain his connection to Los Alamos. Not only was he doing very important work, but he provided a unique kind of leadership. There was just no substitute for having John Wheatley around.

Science: *Was the appointment successful?*

Hecker: The appointment is a very interesting one because it promotes a closer tie to the University of California and a better link to their students. The way John interacted with students was crucial to the way he did business. He was a natural teacher, and he recognized that the only way to get the best students is to be where the action is. He arranged to spend six months at UCLA and six months at Los Alamos. In fact his six months at UCLA were almost up when he died.

Clogston: We should list the experiments John was working on when he died.

Swift: Nucleation of the superfluid helium-3 B phase out of helium-3 A, measurements on sticking coefficients of spin-polarized hydrogen on superfluid helium, Rayleigh-Bénard convection in mixtures of helium-3 and helium-4, and a multitude of heat engines, including a liquid propylene Stirling engine, an acoustic cryocooler, a heat-driven acoustic cooler, and a liquid sodium acoustic prime mover. At UCLA he was doing nonlinear experiments on the localization of vibrational energy using the vibration of a thin cylindrical shell.

Migliori: In the last year and a half John, along with Scott Buchanan and me, became very interested in the localization of vibrational energy through nonlinear effects. At first the work was related to heat engine concepts, but it has since left those concepts far behind. Now, the idea is to establish whether such localized objects exist at the molecular level and whether they are as good an elementary excitation as anything else. To guide our thinking, John invented a classical model using the fact that a thin shell of stainless steel can exhibit some of the same effects that collections of tens or hundreds of molecules exhibit. Through this model one can make contact with the molecular system on an intuitive mechanical level by dealing with things you can hold in your hand. John, Scott, Seth Putterman at UCLA, and I planned to attack this problem in a major way. Of course, we're still going to do it.

Clogston: Within the last five years there's been a renaissance in classical physics as people developed tools to study nonlinear phenomena. This turn of events must have been very thrilling to John. As we discussed, classical physics is the area in which he really worked naturally, in which he could model things in his head. The surge of interest in nonlinear phenomena must have been very inspirational to him.

Migliori: But John felt, and I agree, that a lot of the principles are simple enough that one doesn't need the highest power theory to attack them. In fact, if you are going to do experiments, it's better to understand these things on a more fundamental and simpler level than merely to rely on buzz words and jargon.

Pines: Looking back on John's career, it seems that he'd concentrate on the technology for a period and suddenly there'd be a great outpouring of papers dealing with the physics made possible by that technology. His research at La Jolla was really based on the technology that he developed at Urbana with the dilution refrigerator. Toward the end of the time in La Jolla, he was beginning his work on heat engines. All the work in helium-3 was, as Matti said, based on solving the technical problems needed to do accurate experiments below a hundred millikelvins. He opened up new fields in science again and again either through his physics experiments or his interest in technology. One learned very soon that what John proposed, no matter how way out it might sound at first, had to be taken seriously.

Clogston: My experience was that John had enormous physical intuition, a really deep intuitive understanding of physics.

Hecker: I always respected John as a scientist, but I got to respect and love him as a human being. I learned later that he likewise had gained respect for me over a period of months and years. As a result, I was able to talk to him more frankly than to almost anybody else here at the Laboratory. He'd come to me for advice about his programs, his space, and his equipment because he knew I was a hardliner. There's no question he was difficult to deal with, but that was mostly because of his insistence on excellence. He wanted to be the best at everything he did, and he felt in order to do the best, he had to have the best. He insisted on it.

Pines: We don't miss just John the scientist; we miss John the person. He had an independent and special view about almost any topic. You couldn't anticipate it. He was never one to run with the crowd; he was just fun to talk to.

Clogston: Maybe that sums it up—he was fun to talk to. I'm going to miss him tremendously.

Krusius: Those of us who worked intimately with him over the years have lost a colleague, a mentor, and an example. He was a true experimentalist who found pleasure and inspiration in life from the search for new understanding. He was totally dedicated to this cause. I shall always keep in mind his disciplined and analytical thoughtfulness as he pursued a problem and his excitement and joy as he approached a solution. But beyond John's professional excellence, we have also lost a personal friend with whom we shared thoughts and countless ups and downs in both the laboratory and on bicycle rides. He was a friend who was always available for help and advice at difficult moments.

Swift: What John really liked most was to turn the screwdriver, to make the measurement, to do the whole scientific process himself. The vision of John that I'll always keep in mind is of him sitting on a hard wooden lab stool in front of a bunch of equipment, wearing a plaid shirt and khaki short pants—those great-looking legs of his on display—peering at instruments through his glasses, and writing numbers down in his lab book. ■