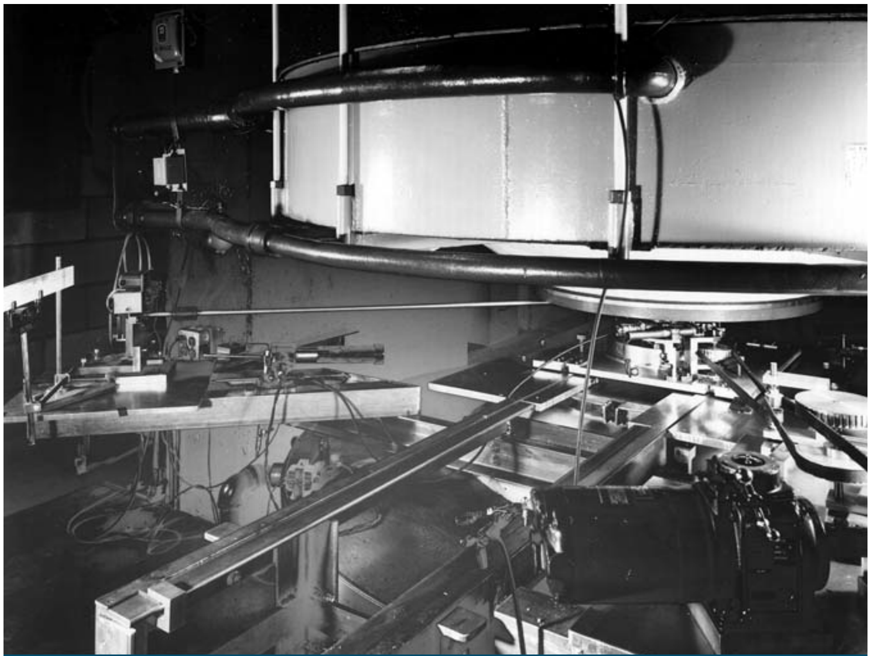
Recollections of Chandra



This rotating cylinder viscometer, which used mercury as a conducting fluid, was located in an old cyclotron magnet that provided fields to 1 tesla. The initial goal was to confirm Subrahmanyan Chandrasekhar's stability calculations. The cyclotron magnet now sits next to the entrance driveway at Fermilab. (Photo supplied by Russell J. Donnelly.)

Subrahmanyan Chandrasekhar is the subject of a splendid remembrance in the December 2010 issue of PHYSICS TODAY. From 1956 to 1966, I was in the physics department at the University of Chicago, and Chandra was my colleague and best friend. I would like to add some recollections of his work on hydrodynamics, which has unfortunately been neglected in almost all celebrations of his life.

I was a graduate student at Yale University from 1952 to 1956, working with Cecil Lane and Lars Onsager on the superfluidity of helium-4. Those of us studying quantum fluids found we had to learn a lot about classical fluid mechanics, because most probes of superfluids are hydrodynamical in nature. Onsager introduced me to the subject of hydrodynamic stability and mentioned the names of the leading investigators, whom he called a "rare crew": C. C. Lin at MIT, G. I. Taylor at Cambridge University, and Chandra. Onsager introduced me to them all at an American Physical Society (APS) meeting in New York City. Once Chandra got to Chicago, he and I tried to calculate the hydrodynamic stability of superfluid helium between rotating cylinders[1]—the Taylor–Couette flow—but our paper was based on still incomplete equations of motion for the two-fluid theory. It took another 34 years to finally get theory and experiment complete.[2]

Intellectually, Onsager and Chandra were very different men. Onsager had unmatched insight into the nature of many fields but found it difficult to convey his insights to others. Chandra had less physical insight into experiments, but once he had "done the math" he could explain it to anyone on the street.

Chandra's work in fluid dynamics is essentially forgotten in mainstream physics. I think that is because astrophysics, especially general relativity, is much more popular in the physics community than fluid dynamics; I am not aware of an undergraduate course in fluid dynamics being taught in any physics department. Also, Chandra had the bad luck of turning to fluid dynamics just before computers became generally available, and his numerical methods are not used today. His calculations were done on a Marchant mechanical calculator by his incredibly hard-working assistant, Donna Elbert. We used to call her "Miss Canna Helpit."

When I arrived at Chicago, Chandra had obtained substantial grant support to have stability experiments done in collaboration with Dave Fultz, a geophysical fluid dynamics expert. The grant included funds to convert an old cyclotron into a hydromagnetic stability facility that used mercury as a test fluid (see photo, above).

Chandra was passionate about the experiments and was often in the lab urging us on. He brought to the lab all his visiting friends, including his mentor, Paul Dirac. Chandra was intimately involved in each experiment. In the planning stages he, our instrument maker Jim Radostitz, and I would meet in Chandra's office and discuss the design. Sometimes we would have a race to see whether calculations or experiments would be the first to provide data on a new problem. Although Chandra's focus was on the onset of instability—the linear stability problem—our data provided insight into nonlinear stability, such as the torque beyond the onset of instability in Taylor–Couette flow as studied with a rotating cylinder viscometer.

Eventually Chandra gathered all his calculations and experiments and put together a book entitled *Hydrodynamic and Hydromagnetic Stability*,[3] by far the most cited of all his books. The day the first copy was received in 1961, Dave Fultz put Chandra in front of our Taylor–Couette apparatus and got the picture shown on page 10.

Chandra's theoretical and experi-

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mental work in fluid dynamics was very productive and influential. Chandra was an early supporter, with John von Neumann and others, of forming APS's division of fluid dynamics, and he served as division chairman in 1955. Today the division is one of the most rapidly growing in APS.

References

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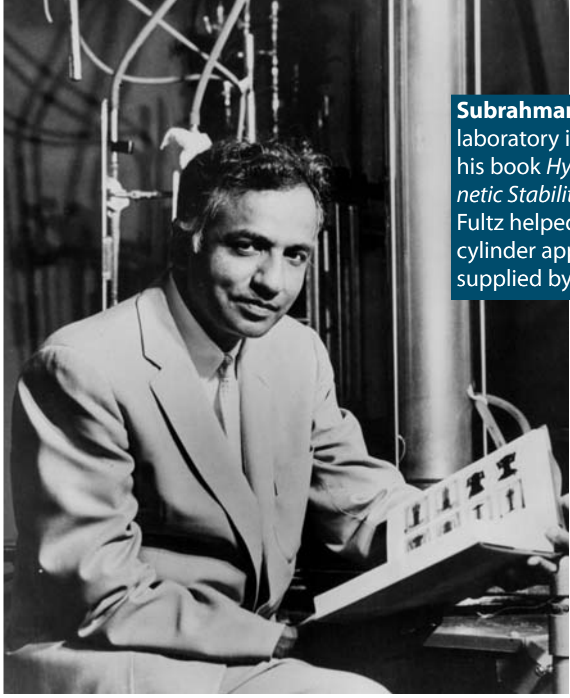
The interesting articles on Subrahmanyan Chandrasekhar dealt primarily with his accomplishments in astrophysics. Less known, perhaps, is his pivotal role in bringing the significance of the Stokes polarization parameters[1] to the attention of the optics community in 1946. Those parameters are now considered a cornerstone of modern optics.

In the late 1960s at the Catholic University of America, my doctoral thesis adviser, Peter Livingston, suggested that I express my final results in terms of the Stokes polarization parameters, which I had never heard of. After a little searching I found a paper by Ugo Fano[2] that discussed the Stokes parameters. The first two references in Fano's paper were to two French scientists,[3] and the next was to Chandrasekhar's 1946 paper.[4] By 1940 the Stokes parameters were nearly forgotten in the English-language scientific literature, although they continued to be taught in France.

In 1942 one of those two French papers (reference 3, Perrin) suddenly and quietly brought the Stokes parameters back to light in the English-speaking world. However, few optical physicists regularly read the *Journal of Chemical Physics*. Furthermore, allied efforts during World War II meant that few US and UK scientists had time to read the literature.

I was especially interested in knowing how Chandrasekhar, working in astrophysics, had found out about the Stokes parameters. He was to receive an astrophysics award at the 1969 annual meeting of the American Physical Society, and I decided to attend. Arriving early at the ceremony, I found him sitting by himself. I introduced myself, told him briefly about my research, and asked how he had learned about the



Subrahmanyan Chandrasekhar in the laboratory in 1961, with the first copy of his book *Hydrodynamic and Hydromagnetic Stability* (Clarendon, 1961). Dave Fultz helped build the Taylor rotating-cylinder apparatus behind him. (Photo supplied by Russell J. Donnelly.)

Stokes parameters. His face lit up, he invited me to sit down, and he answered, “Because of a bet!”

In the early 1940s at the University of Chicago, Chandrasekhar explained, he had begun to write a series of papers on radiative transfer, and every Monday he would meet George Uhlenbeck and Gregory Breit for tea and coffee in the university lounge. At one of those meetings in 1945 he said that he had gone as far as he could with radiative transfer and that all that was left was to include the effects of optical polarization. Uhlenbeck said, “That might be more difficult than you think.” But Chandrasekhar was so confident he would have the complete solution by the following Monday that he made a $1 bet with Uhlenbeck.

The next day he set to work. By Friday evening, however, he was unable to find a means of introducing polarization into his radiative equations. He’d become frustrated, desperate, and, he said, “exhausted.” The next morning he went to the university library and searched every optics book he could find to see how polarized light was treated. No success! A bit frantic, he finally came across a 1904 English optics textbook[5] that described Stokes’s formulation of polarized light in terms of intensity—the first parameter—and three remaining polarization parameters. He immediately recognized the connection between the intensity formulation of the Stokes parameters and the radiative intensity equations. Despair turned to elation. He then found Stokes’s 1852 paper, quickly absorbed it, and proceeded to reformulate the radiative equations to include polarization.

He tidied up his paper on Monday morning and met his two colleagues at tea time. He proudly laid his radiative equation paper, with Stokes parameters, on the table. He told Uhlenbeck he had found the solution using polarization parameters he “discovered” that had been introduced by George Gabriel Stokes in 1852.[1] With those parameters he had been able to reformulate the radiative equations to include polarization and so had won the bet. Uhlenbeck casually replied, “Oh, *that* set of Stokes parameters. I know all about them, and here is your dollar.” Chandrasekhar was taken aback and thought to himself, “You could have told me that last week and made my life much simpler!” Probably, Uhlenbeck had not seen the connection between the radiative transfer equations and Stokes parameters. But, Chandrasekhar said, at least his reputation was intact!

I had one last question: Where had Uhlenbeck learned about the Stokes parameters? Chandrasekhar said, “I don’t know,” but pointed out Uhlenbeck on the stage. “Tell him I sent you, and ask him how he knew about the Stokes parameters.” I walked up to Uhlenbeck, and when I told him Chandrasekhar had sent me and asked him my question, he immediately said that he had learned the Stokes parameters from Paul Ehrenfest. I asked “Where did Ehrenfest learn about them?” Uhlenbeck replied, “I don’t know, but Ehrenfest knew everything about physics!”

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Anotating the history of rotation ideas

In his letter “Blackett Adopted Earlier Rotation Idea” (PHYSICS TODAY, January 2011, page 10), Thomas Ruedas complains about an error by implication in Greg Good’s article “Rutherford’s Geophysicists” (PHYSICS TODAY, July 2010, page 42). He says Good gave the impression that Patrick Blackett originated the idea that the magnetic field of a rotating body such as Earth or the Sun is produced by its rotation. True, Good could have used better phrasing, but Blackett himself made no such claim; his papers fully acknowledged the long previous history of the subject. Blackett’s 1947 paper includes a reference to Arthur Schuster’s paper of 1891, and his final, 1952, paper on this topic goes back to an obscure German work by Henry Rowland in 1885, presumably reporting the experiment Rowland did in 1875, an experiment that started the ball rolling.

Some years ago I prepared an annotated bibliography “Rotation Theories of the Production of the Magnetic Field of the Earth and Other Bodies”; it is available online at http://www.agu.org/history/mf/contrib/rotation.doc.

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A last word on scientists’ last word

In his letter, Brian Sutcliffe rightly calls attention to the long history of scientists speaking out on scientific matters of societal importance (PHYSICS TODAY, March 2011, page 8). Here is an especially flamboyant example. Arthur Shipley, an eminent zoologist and master of Christ’s College in Cambridge, UK, wrote about the Scopes “monkey” trial then under way in Tennessee, “The average American of the Middle and Southern States is a very naïve mammal. . . . The United States is a nation of adult children and some of the things they do seem to older and more mature countries decidedly childish.”[1]

Reference

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