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JULE GREGORY CHARNEY

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A Biographical Memoir by

NORMAN A. PHILLIPS

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BY NORMAN A. PHILLIPS

JULE CHARNEY WAS one of the dominant figures in atmospheric science in the three decades following World War II. Much of the change in meteorology from an art to a science is due to his scientific vision and his thorough commitment to people and programs in this field.

In 1946 he married Elinor Kesting Frye, a student of logic and semantics with H. Reichenbach at the University of California at Los Angeles. They had two children, Nora and Peter. Nicolas, Elinor's son from her previous marriage, assumed the last name of Charney. Their marriage lasted almost twenty-one years. In 1967 Jule married Lois Swirnoff. Lois is a painter and color theorist and was a professor at UCLA and Harvard. Their marriage lasted almost ten years. Jule shared the last years of his life with Patricia Peck, a photographic artist with roots in New York City and Venice. His last illness was lung cancer, from which he died in Boston on June 16, 1981.

THE BUDDING MATHEMATICIAN

Jule was born on New Year's Day 1917 in San Francisco. His parents, Stella and Ely Charney, had immigrated early in the century from White Russia, where the lot of Jewish citizens was difficult. Each of them had taken up work in the New York garment industry, but later met and married in St. Louis. After a brief stop in Denver, they moved to Los Angeles in 1914. Employment difficulties forced a temporary move for several years to San Francisco, where Jule was born. He spent most of his youth in Los Angeles with one important exception. This happened at the age of fourteen, when his mother, temporarily estranged from his father, moved back to New York. Jule later recalled that he did not like New York, but he also remembered that it was here at a relative's home that he came upon Osgood's book on calculus. Calculus was not taught in any of the usual high schools in the country, but exposure to this book and the realization that he could solve the problems excited his interest in science.

Mother and father were fervent socialists, especially Ely, who took an active role in union affairs. Stella favored a more leftist position than that held by her husband. Home political discussions were frequent. Along with this stimulating background, Jule read widely and voraciously in the public library during grade school. He was exposed to music in his early years through a small family collection of records (Caruso, Galli-Curci, Tchaikovsky, etc.), but he never received any musical training. Nevertheless, music was a source of enjoyment throughout his life. One of his amusing recollections in later years was of having played games with the young prodigy Yehudi Menuhin on top of Yehudi's apartment building, and in using this fact many years later to establish an element of mutual recognition with the world famous violinist.

His last three high school years were spent at Hollywood High School after the family moved from Boyle Heights in east-central Los Angeles. By graduation in January 1934 he had already familiarized himself through independent reading with most of the standard material on the differential and integral calculus, and it seemed that he was already on the way to a career in mathematics or theoretical physics. He attended the Los Angeles campus of the University of California instead of the scientifically well-established campus at Berkeley, because of UCLA's nearness and the absence of any advice about the senior campus to the north. His undergraduate years emphasized both mathematics and physics (although Jule later complained about the lack of theoretical physicists at UCLA), and he began to be recognized as a likely candidate for the first doctorate in mathematics from the Los Angeles campus. He became a member of Phi Beta Kappa and a University Fellow in 1939 shortly after he started his graduate work under T. Y. Thomas. A master's degree followed in 1940 and he soon completed a paper, "Metric Curve Spaces." Thomas considered this suitable material for a doctoral thesis, but Jule had a lower opinion of its merit; he never began the final write-up for submission as a thesis.

Thomas led a seminar that included treatment of fluid turbulence and one day invited J. Holmboe from the newly formed meteorology group in the Physics Department to talk. Having introduced Jule to the idea of meteorology as a field with some scientific possibility, Holmboe invited Jule in the spring of 1941 to be his assistant and to participate in the meteorology training program taking shape at UCLA and other universities under sponsorship of the army and navy. At this time the war in Europe and tensions in the Pacific had progressed far enough that university students began to consider various options for useful service. Seeking advice, Jule visited T. von Karman and was counseled to pursue meteorology over work in the aeronautics industry since the latter was becoming too much of an engineering subject for a person of Jule's theoretical inclination. Since this option had also been made easy by Holmboe's offer, it was the logical choice; in 1941 Jule became a teaching assistant and student in the meteorology program at UCLA.

A NEW LIGHT IN METEOROLOGY

In 1941 only a few U.S. universities offered meteorology as an academic discipline, although greater interest in the field was being stimulated by an expanding military's need for weather forecasters. The leader of the small meteorology group at UCLA (then a part of the Physics Department) was J. Bjerknes, who had recently arrived from Norway. He was very well known in the meteorological world for the description of cold and warm fronts he had put forth in Bergen about the time of Jule's birth. J. Holmboe was a younger Norwegian who was at ease with these concepts and had somewhat more familiarity with fluid dynamics.

M. Neiburger, on the other hand, had been educated under C.-G. Rossby at the Massachusetts Institute of Technology. Rossby preferred a more analytic approach to atmospheric and oceanic motions, in which fluid dynamics was applied to simplified models of the atmosphere and ocean. In 1939, for example, he had pursued a recent idea of Bjerknes that the variation with latitude of the Coriolis parameter (twice the angular velocity of the earth times the sine of the latitude) played an important role in the eastward migration of the large-scale circulation systems. Rossby used a simple model of a purely horizontally moving homogeneous atmosphere to arrive at a quantitative formula for the speed at which these systems (now called Rossby waves) would move from west to east in such an idealized atmosphere. Although these flow patterns were correlated with weather systems, weather forecasting throughout the world was still done by extrapolating the day-to-day behavior of pressure systems as they were depicted on daily weather maps of surface weather observations. Even Rossby's formula—at the few places it was known—had only a limited role because there was no evidence for deciding the level in the atmosphere at which his model should be applied. Furthermore, in 1941 measurements in the troposphere were too few to define the flow pattern over the hemisphere at one instant. (The novel *Storm*, published by G. Steward in 1943, gives a necessarily romantic, but otherwise realistic picture of meteorological practice at that time.)

During the next ten years Jule Charney brought about a profound change in this primitive procedure. In collaboration with J. von Neumann he was to show how the newly developed electronic computer could be used to make forecasts by numerical integration of the hydrodynamical equations of motion, beginning with the observed picture of those motions that had then become available from a greatly expanded network of daily radiosonde stations. The basic premise of this physically based procedure was not new, having been stated by V. Bjerknes in the early years of the century and even attempted partially by L. Richardson during World War I. It had, however, lain dormant for twenty-five years.

Part of Jule's assignment was to teach a course in synoptic meteorology—the construction of weather maps based on surface observations of pressure, temperature, wind, and weather. In his 1980 conversations with G. Platzman, Jule recalled his distaste for this subjective procedure with its emphasis on elegant drawing of isobars and fronts. He admitted, though, that it was in 1941 the only way for students to become familiar with atmospheric motions and behavior. (His performance as a teaching assistant was evidently acceptable; his small class of students in this subject successfully manipulated his campus-wide election as King of the Mardi Gras—a precursor of many academic honors to come!) Jule also taught a course in atmospheric radiation as a substitute for J. Kaplan (where he recalled being just one lecture ahead of the class) and assisted in preparing notes for Holmboe's lectures on basic principles of fluid dynamics of the atmosphere.

Jule's university social life was happy. M. Wurtele, a fellow meteorology student, recalls that Jule shared a house on Kelton Avenue with several other students and enjoyed a lively social life. Fortunately, a mistaken diagnosis in childhood that he had a heart problem had been corrected in his teens. Jule had since learned to ski and play tennis, sports that he was to enjoy until the last several years of his life. Somewhere along the way he acquired experience in games of chance, a skill that was exercised much later on night watches during one of the two Indian Ocean ship expeditions in which he participated. (After Jule's death B. Taft recalled that Jule was the only scientist he knew who could play poker nightly with the ship's crew, win their money consistently, and never engender the slightest ill will.)

With his mathematical background Jule was not attracted by the descriptive reasoning used by Bjerknes and Holmboe. Fortunately, however, Neiburger exposed him to Rossby's papers early in Jule's assistantship. This is not to say that Rossby used completely deductive reasoning—the simple models that he constructed to describe the atmosphere and ocean were based on intuition instead of rational simplification (and were often resisted by fellow meteorologists on that ground). Rossby and Charney exchanged many letters in the ten years preceding Rossby's death in 1957. (In the Charney files at the Massachusetts Institute of Technology there are forty-two letters from Rossby and twenty-three from Charney.) In one of them Rossby described his own teaching method: "Perhaps I occasionally sought to give, or inadvertently gave, to the student a sense of battle on the intellectual battlefield. If all you do is to give them a faultless and complete and uninhabited architectural masterpiece, then you do not help them to become builders of their own." This philosophy also characterized Rossby's papers and seems to have had a permanent effect on Charney's thinking.

Around 1944 or 1945 Charney began to view himself as qualified to consider a thesis in meteorology. He gradually formulated his goal to be a theory of the instability of the average west-to-east flow in middle latitudes of the atmosphere. These zonal westerlies increase with speed from ground to around ten kilometers because the average air temperature below that level typically increases from pole to equator. This choice of topic was influenced by his exposure to Bjerknes' semi-quantitative description of the wave-like patterns in the upper atmosphere, three or four of Rossby's papers, and his exposure in the lecture series by Thomas to the idea of instability in fluid flows as a mathematical problem. This choice was his, with no guidance from the faculty.

The perturbation equations for atmospheric flow are intricate when allowance is made for a basic state containing a non-uniform current. Furthermore, even a resting atmosphere can sustain propagation of sound waves and of gravity waves, as well as the more recently recognized Rossby waves. To arrive at a tractable mathematical problem, Jule found it necessary to make a set of consistent approximations in his derivation of the final governing differential equation. In his 1980 recorded conversations with G. Platzman, Jule recalled with fresh enthusiasm the occasion when this process had reached a tractable state in his mind, with a recognizable standard second-order differential equation. It is easy now to forget that this type of reasoning was not then common in any branch of science. That Charney accomplished this, and without help from any established fluid dynamicist, is early evidence of his insight.

After much hand calculation Jule was able to find a curve of zero growth rate that separated unstable waves of short horizontal wavelengths from longer stable waves. He also calculated how the wind, temperatures, and pressure fields were organized in an unstable wave, and this picture agreed well with observed features of the upper waves. The thesis was quickly published and accepted as an explanation for this phenomenon even though few meteorologists were then familiar with this level of mathematics. Later studies have shown that the complete solution is more complicated than Jule thought in 1946, but his solution did contain the most important aspects. Most significantly, his thesis satisfied Jule's high critical standards and convinced him that he was indeed capable of original research of high caliber in meteorology. His ensuing commitment to meteorology as a permanent career was of major importance to the development of atmospheric science.

NUMERICAL WEATHER PREDICTION AND PRINCETON

In the months before his thesis defense in the spring of 1946 Charney explored several avenues for a postgraduate fellowship, having in mind that he was, in spite of his thesis, a newcomer to fluid mechanics. He was awarded a National Research Council fellowship, tenable in Europe, and he made plans to visit H. Solberg in Oslo (who had been the leading mathematician in the Norwegian school) and G. I. Taylor in Cambridge, England. Fortunately, Jule and Elinor called on Rossby at the University of Chicago en route. Rossby was leading the department into its heyday with field investigations of thunderstorms (under H. Byers), discovery of the jet stream (under E. Palmen and H. Riehl), application of group velocity to meteorological and oceanic wave propagation (under Rossby), and simulation of atmospheric motion in experiments with rotating differentially heated "dishpans" (under D. Fultz).

The two men hit it off at once and Rossby, with his extraordinarily persuasive powers, had no difficulty in persuading Jule to postpone his fellowship and stay at the university for almost a year. The two men had many discussions both together and with other faculty and the many foreign visitors Rossby brought to Chicago to open the channels of communication that had been interrupted by the war. Jule later viewed this year as the most formative experience in his professional life.

A major event soon occurred when Rossby arranged for Jule to attend a meeting that J. von Neumann was to hold in August 1946 at the Institute for Advanced Study in Princeton. The subject was the application of electronic computers to weather forecasting. Von Neumann had recently recognized weather prediction as a prime candidate for application of electronic computers, in particular the new computer that was being built to his specification at the Institute. (In his 1980 interview with Platzman Jule suggested that von Neumann's interest in weather prediction originated from von Neumann's acquaintance with V. Zworykin at nearby RCA. F. Nebeker, however, points out in his Princeton University thesis that it was Rossby who suggested to von Neumann that the Institute for Advanced Study should submit a proposal for meteorological funding to the navy's Office of Research and Invention, and that this had been done by May 1946.)

About a dozen of the leading dynamical meteorologists in the United States attended, including Rossby. Most of them knew that L. Richardson had attempted during World War I to integrate the hydrodynamical equations for the atmosphere with finite-difference methods for a single time step, but had obtained an absurdly large value for the rate of change of surface pressure. Neither the official minutes nor Jule's notes of the meeting record anything of material or even inspirational value at that time. But from the vantage of hindsight it is possible to see that the presentation by navy Lieutenant R. Elliott consisted of approximated equations that had some similarity to the quasi-geostrophic theory that Jule was to formulate in the next several years. The similarity is clear, however, only to someone who knows what to look for, because Elliott's derivation was ad hoc and his computation scheme was involved and ill-posed. It is not surprising that Elliott's work was not pursued by the small meteorological group that von Neumann collected.

Thus, the only important result of this meeting was to acquaint Jule Charney with the fact that John von Neumann was a man with considerable feeling for physical problems and that a rational theory for the large-scale motions of the atmosphere would receive a strong welcome at Princeton, with a good likelihood of being applied on the new computer. Jule's files show that shortly after returning to Chicago he went so far as to write a letter to von Neumann exploring the possibility of coming to Princeton, but he never mailed it.

Jule and Elinor sailed for Norway in the spring of 1947. Their first stop was at Bergen, the intellectual home of the Norwegian frontal concept since World War I. Here Jule met the English theoretical meteorologist E. Eady. Eady had independently derived a theory of the instability of the west wind belt containing the same physical mechanism as that in Jule's thesis, but in a simpler form. They became good friends and Eady later spent a part of a year with Jule at Princeton.

Upon arrival in Oslo, Jule found a long letter from J.

Bjerknes containing many practical suggestions on travel and other aspects of living in postwar Norway. Bjerknes also arranged for Elinor to receive some money by helping C. Godske with the language for his contribution to the new English edition of the *Physikalische Hydrodynamik* that had been published in 1933 by the Bergen meteorological group. This must have been a welcome addition to the fellowship stipend. The solicitude continued when Bjerknes wrote in late November to discuss not only the possibility of coming back to a faculty position at UCLA, but of faculty appointments at other universities as well! It is easy to understand Jule's long, deep respect for J. Bjerknes.

Jule did not take long to discover how to modify the hydrodynamical equations for separating the meteorologically relevant large-scale motions from the faster acoustic and gravity waves that were also contained in the equations and which were demonstrably at the root of Richardson's difficulty; the year of gestation at Chicago had done its job well. As he wrote in his 1948 paper, "On the Scale of Atmospheric Motions":

> The motion of large-scale atmospheric disturbances is governed by the laws of conservation of potential temperature and absolute potential vorticity, and by the conditions that the horizontal velocity be quasi-geostrophic and the pressure quasi-hydrostatic.

This formulation, which Jule first stated in a letter to P. Thompson in November 1947, was justified by a careful scale analysis of the terms in each of the hydrodynamic equations for momentum, for mass, and for entropy. This scale analysis was similar in principle to the consistent approximation steps Jule had been led to in arriving at the governing differential equation for his thesis. The set of prediction equations that results from the above prescription is nowadays called the quasi-geostrophic theory. It allows only "slow" advective-type motions without acoustic or gravity waves (i.e., it acts as a low-pass filter).

The quasi-geostrophic theory is probably the most rewarding development in meteorology and oceanography since World War I. Numerical weather forecasting is now based on a more complete set of dynamic equations, but Jule's quasi-geostrophic system was necessary for the first several years of computer work; it is still used for theoretical studies of atmospheric motion. It is possible to detect procedures in earlier literature that bear some resemblance to parts of Jule's system, but they are scattered and have little intellectual continuity. Jule was not familiar with most of them in 1947, but this was probably an advantage. It is also true that in 1947 other meteorologists were close to formulating the quasi-geostrophic system (A. Eliassen in Oslo had already done so), but Jule had in effect proved with his scale analysis that this system was a consistent approximation for large-scale atmospheric motions.

In early 1948 von Neumann invited Jule to head the meteorology group in his Electronic Computer Project, whose financial support came from the Office of Naval Research. Arrangements were also made for A. Eliassen to come from Oslo for a year, to be followed by R. Fjørtoft. These Norwegian meteorologists were well trained in both hydrodynamics and descriptive meteorology, while earlier members of the group, J. Freeman, G. Hunt, P. Queney, and Thompson as well, were primarily theoreticians and left Princeton for other commitments.

For three years Jule and Elinor lived in the Institute compound—a collection of wooden rowhouse barracks that had been moved to the Institute grounds from use elsewhere. These were occupied by the many one-year temporary members of the Institute, mostly young mathematicians and physicists, with a sprinkling of more established professors in the humanities. Located within a brief walk of the computer building and the main Institute building, this was a stimulating place for Jule and Elinor to live and for Jule to begin work on what was clearly going to be a milestone in atmospheric science. Von Neumann, although often away from the Institute, was an eager listener and willing participant in Jule's thinking. John and Klari von Neumann were gracious hosts and the Charneys soon met the Oppenheimers and other permanent members at the Institute, as well as faculty at Princeton University. It seems reasonable that this cosmopolitan milieu and the earlier year at Chicago did much to equip Jule for the powerful domestic and international advocacy roles he was to play later in the creation of the National Center for Atmospheric Research and the Global Weather Experiment.

During the first year Jule took several major steps in preparation for predicting flow patterns with a computer. First, with the quasi-geostrophic system he investigated the important question of how large a volume of atmosphere surrounding a forecast point must be considered for a twenty-four-hour forecast. This was answered by appeal to the three-dimensional group velocity of Rossby waves in a uniform current from the west. But the full three-dimensional geostrophic system, straightforward as it was, was still too demanding for von Neumann's computer. Jule showed how, by an intelligent system of vertical averaging, the full system could be reduced to a simple approximate statement that vorticity was advected horizontally at a certain objectively defined level in the atmosphere. This was denoted as the equivalent barotropic level, located about 5 kilometers above sea level. Jule put forth this greatly simplified system as the first nonlinear system for numerical weather prediction, to be followed by future systems with more vertical detail as experience and computer development would justify.

Since the computer was not available, Charney, with the collaboration of Eliassen, took the simplification process one step further. They linearized the barotropic equation to treat perturbations on a uniform flow in a narrow west-east channel and expanded Rossby's frequency formula into a Green's function that would give a twenty-four-hour forecast of the initial flow pattern by simple weighted longitudinal integration of the initial distribution of the isobaric height at the barotropic level. Tests gave very promising results, indicative of success to come when the new computer could be applied to the full nonlinear vorticity equation. (These linear results were so striking that when Jule sent them to Rossby in Stockholm, J. Namias, then chief of the long-range forecast section of the U. S. Weather Service on visit to Rossby's institute, wrote immediately to H. Wexler in Washington, urging that the Weather Bureau contact Charney at once to start operational testing of this linear method. J. Smagorinsky became involved in this effort.)

Charney and Eliassen exploited this linear model further by inserting the effect of flow over mountains and the effect of turbulent friction in the air near the ground. These effects were not of major importance in a forecast for one day, but when the enhanced equations were solved for a stationary perturbation field the resulting pattern was amazingly similar to the time-averaged perturbation field at the five-kilometer level. (The manner in which friction was represented in the equations was conceived by Eliassen and has become known as Ekman pumping, referring to the Swedish oceanographer who first presented the mathematics for the effect of the earth's rotation of frictionally driven currents near the surface of the ocean.) Many later studies with more detail in latitude and height—including one by

Charney—have shown that the effect of mountains on large-scale atmospheric flow is not as straightforward as was assumed here by Charney and Eliassen. (For example, they used a somewhat narrow zonal channel, which reduces considerably the dispersive aspect of Rossby waves.) But the gods smiled!

Since work on von Neumann's computer had progressed more slowly than hoped, Weather Bureau chief F. Reichelderfer, at von Neumann's request, wrote in September 1949 to General Hughes, Chief of Army Ordnance, for permission to use the ENIAC computer at Aberdeen Proving Grounds. Von Neumann had developed a technique for using Fourier sums with cyclic input and output of punched cards that allowed the nonlinear vorticity equation to be integrated on the ENIAC, whose internal storage was small.

The first one-day, nonlinear prediction was made in April 1950. It required the round-the-clock services of Charney, Fjørtoft, J. Freeman, G. Platzman, and J. Smagorinsky, and, largely because of ENIAC breakdowns, more than twenty-four hours to execute. The results of several such forecasts were quickly published in Rossby's journal *Tellus*. Their overall character was good. Jule sent copies of the forecasts to L. Richardson in the United Kingdom. Richardson was a committed pacifist who had abandoned his early numerical work on forecasting and, since 1920, had worked on devising mathematical models to understand and prevent war. Richardson asked his wife to judge whether initial (*a*) or forecast (*d*) maps best resembled the verification maps (*b*). He reported her verdict to Charney:

Thus (*d*) has it on average, but only slightly. This, although not a great success of a popular sort is anyway an enormous scientific advance on the single, and quite wrong, result in which Richardson (1922) ended. So far I have only had time to glance at your five papers. To comment on them now would be rash; but to defer comment would be to risk never making any; for I have other urgent duties.

Richardson died several years later.

The first computations on the Institute computer were made in the summer of 1952, followed closely by models that contained two and then three levels of information in the vertical. The latter successfully predicted a case of intense winter storm development. This development was an example of the instability that Jule had described in his thesis and was convincing vindication of his graduate work seven years earlier. This was the peak of Jule's interest in personally pursuing numerical weather prediction, although he was to make several theoretical contributions in later years.

The meteorology group (in particular, the writer) used a simple quasi-geostrophic model to simulate the so-called general circulation—the manner in which latitudinal variation of solar heating generates zonal winds and how this, through Jule's instability process, gives rise to cyclones and anticyclones, which in turn modify the zonal winds into belts of westerlies and trade winds. Besides predicting westerlies and trades, this experiment indicated that the fronts described by Bjerknes were not the source of large-scale storm development, but were created by the developing unstable wave. This showed that Bjerknes' recent emphasis on the wave in the free atmosphere and Jule's fixing upon this wave instead of fronts as the basic instability element was correct. This type of numerical experiment has blossomed nowadays into elaborate computer simulations of the atmosphere (and the oceans), which include detailed radiation calculations, modeling of the hydrological cycle and, in some instances, chemical interactions. These are used to estimate anthropogenic effects on mean temperature from changes in carbon dioxide or dust from thermonuclear explosions and on ozone from changes in hydrofluorocarbons and nitrogen oxides.

At its maximum, Jule's meteorology group consisted of Jule plus four meteorologists, several programmers, and a secretary. In addition there was a constant stream of short-term visitors from the United States and abroad, some Jule had invited, and others who requested to see firsthand what these new developments were like. Jule also took seriously the responsibility to report frequently the group's progress at scientific meetings.

The new prediction method spread rapidly, with assistance from the Princeton group. In August 1952 von Neumann organized a meeting to consider operational use of the new method by the weather services of the United States, and by February 1954 the computer had been selected for what was known as the Joint Numerical Weather Prediction Unit, representing the Weather Bureau, Air Force, and Navy. The Princeton group assisted in teaching the new methods and computer usage to representatives from the three services. By 1954 groups also had started at the Air Force's Cambridge research laboratory, at Rossby's international institute in Stockholm, and independently, at the British Meteorological Office. In late 1955, shortly after the general circulation numerical experiments had been digested, von Neumann and Charney encouraged the establishment of a special unit in the Weather Bureau to exploit this technique. J. Smagorinsky led this effort, which has culminated in the Geophysical Fluid Dynamics Laboratory of the National Oceanic and Atmospheric Administration at Princeton University.

Jule's creative interests continued apace. He had some familiarity with the existing theory of large-scale motions in the ocean from his reading of Rossby's papers from the 1930s and from more recent acquaintance with H. Stommel and W. Munk. (H. Sverdrup was also on Jule's thesis committee.) Jule first applied Eliassen's development of the Ekman theory to show how the effect of wind stress at the top of the ocean should be used as a boundary condition on the quasi-geostrophic interior motions of the ocean. This step brought to fruition the original observations of ice drift that F. Nansen had made at the turn of the century, which had led to Ekman's theory. An even more dramatic step was an inertia theory for the Gulf Stream. H. Stommel and W. Munk had presented theories in which the width of the stream—much narrower than the ocean—depended on a poorly known artificial friction parameter. After much discussion with Stommel, Charney showed how the conservation of potential vorticity in the water mass moving slowly westward in the ocean interior should lead to a narrow boundary current at the western coast of the ocean with geostrophic balance in the streamwise velocity. (A similar theory was published almost simultaneously by G. Morgan.)

Jule had now been at the Institute for seven years and was thirty-eight years old. He received a five-year appointment as a member of the Institute at the end of 1951, but was not a permanent member. Von Neumann had become an atomic energy commissioner with heavy duties in Washington and it was clear that the Electronic Computer Project, with its applied flavor, was not to be a permanent feature of the Institute; pure mathematics was the preferred science there. Oppenheimer was unable to promise a permanent membership to Jule, although both men respected each other and the mutual benefit that the Institute and Jule had on one another. The situation became more urgent when von Neumann developed cancer. By this time Jule realized the importance to him of contact with experimental and observational work and began inquiries about a university appointment. The universities responded favorably, some with remarkable alacrity. The Massachusetts Institute of Technology was the winner and offered a package deal including the writer and the retention at MIT of E. Lorenz. It was characteristic of Jule that he recommended to the MIT administration that V. Starr and Lorenz should be promoted as part of this enhancement of the meteorology department. The move was made in the summer of 1956, with a special personal grant of $5,000 to Jule from the Institute at Oppenheimer's urging.

ORGANIZATIONAL CONTRIBUTIONS

After his move to MIT Jule could shed much of his responsibility for progress in numerical weather prediction. Together with W. Malkus in the Mathematics Department he at once organized an informal seminar on geophysical fluid dynamics. This seminar was held fortnightly on late Friday afternoons and gradually involved people from the meteorology, oceanography, geophysics, and applied mathematics groups at MIT, Harvard, and Woods Hole, with frequent participation from Yale, Brown, and the University of Rhode Island. They were held at a different institution each time and the long automobile trip naturally demanded a social hour for decompression afterwards. This seminar lasted for twenty-two years and was the major means of informal communication between people in New England working on this subject.

The fame of his scientific work, however, quickly led to increasing demands for his service as an advisor and committee member. The most permanent of these was an appointment in 1957 to the National Academy of Sciences' Commission on Meteorology, a commitment that was to last, in one form or another, for fourteen years. This commission had been established a year earlier by D. Bronk, with Rossby and L. Berkner as co-chairmen. T. Malone recalls that Bronk did this in response to a request from F. Reichelderfer, who hoped for advice on strengthening research in the Weather Bureau, and because Bronk, as a sailor, had been dissatisfied with weather prediction since the 1938 hurricane! In November 1957 Jule made a report to the commission that emphasized the presence of three new factors in meteorology—satellites that observe the atmosphere on a global basis, instrumented aircraft and radar that scrutinize the details of small weather systems, and the electronic computer that helps digest the new data. Although not acted on at this time, this idea can be recognized in the later call by Jule for the global atmospheric research program.

At about this time, L. Berkner (Rossby had died in the summer of 1957) thought of creating a national research center for atmospheric science. Malone recalls that Berkner first charged him and Jule to give a prompt, but considered, reaction. After intense deliberation they returned with a favorable report and then other meteorologists (H. Houghton, for example) reviewed the concept. One of the early worries was whether the new center would weaken the university departments or whether the departments would stifle a new center. Malone and Charney were then charged to visit a significant sample of universities and established research centers for reaction and suggestions. They did so, with positive results.

Jule played no role in the organizational meetings that followed, but he was very active in assisting Malone in the more technical meetings that described the activity the new center would conduct above and beyond that done at universities. These initial steps, when supplemented by the organizational drive of the leading department chairmen, led to incorporation of the University Corporation for Atmospheric Research in March 1959 and the formation soon after of the National Center for Atmospheric Research (NCAR), with financial support from the National Science Foundation. Jule visited NCAR several times during its early years, but most of his extended visits away from MIT were to other centers.

He became a councillor of the American Meteorological Society in 1959 and was given responsibility for the scientific program at the fortieth anniversary meeting of the society in December 1960. He selected "motions of the upper atmosphere" as the central topic, partly in response, one may suppose, to his revived interest in the vertical propagation of large-scale wave energy. He also served as publications commissioner for several years.

Jule's outgoing nature and open mind quickly led to friendly acquaintance with many of the leading faculty members and administrators at the Institute and elsewhere in the Boston area. In early 1960 he was appointed to the Atmospheric Sciences Panel of the President's Science Advisory Committee. A year later he had discussions with B. Rossi from the Physics Department at MIT, who was advising J. Wiesner (science advisor to President Kennedy) on the possible peaceful uses of outer space. Jule arranged a meeting with several other meteorologists at which it was suggested that satellites could improve weather forecasting. This met with ready acceptance and was referred to by Kennedy in his State of the Union message and in his September 1961 speech to the United Nations. U.N. Resolutions 1721 and 1802 followed, asking first the World Meteorological Organization and then the International Council of Scientific Unions to develop plans to this end in operational practice and research.

Jule recalled that soon after, at a meeting of the American Meteorological Society dealing with international cooperation in meteorology, he was struck with the fact that, although meteorology was presumably a global science, the lack of global observations prevented examination of this important aspect, and that correction of this observational gap could be a very fruitful aspect of international cooperation in meteorology—even if it must be confined to a limited time as an observational experiment.

I recall that Jule pondered deeply on the commitment from him that a serious follow-through on this idea would entail. But having decided that it was a job that he should do, he pursued it with evangelical zeal, almost until the yearlong global observational experiment finally started in December 1979. He devoted considerable time in traveling, speaking, devising ways to arrive at meaningful specifications for an observing system, and helping to formulate the first set of plans.

In 1966 he became leader of the National Academy of Sciences' Committee on the Global Atmospheric Research Program—GARP, as it came to be called—and held this demanding position until 1971. He was active in several working groups, even to the extent of being scientific director for one month of the preliminary GARP tropical observing experiment in Barbados. It was at this time that he cemented a productive and long-lasting working arrangement with NASA's Goddard Laboratory, first in New York City, and then in Greenbelt, Maryland. In the meantime his other research continued at full intensity, as may be judged from his publications. He maintained his educational commitments at MIT in spite of this furious outside activity and even accepted non-trivial appointments to committees for NCAR, the American Geophysical Union, the American Meteorology Society, and several ad hoc committees for the National Academy of Sciences.

Jule and Lois took sabbatical leaves in the academic year 1972-73 and spent the first part at Cambridge, England.

During this period Jule gave considerable thought to how higher frequency gravity wave motions might be generated by nonlinear interactions among Rossby waves, but finally gave up.

The Charneys then stayed at the Weizmann Institute in Tel Aviv, where Jule worked on a theory of desertification. (A loss of vegetation would increase the ground albedo and reflect more solar radiation back to space. The reduction in insolation absorbed by the ground would then decrease the local heating of the air by convection. This in turn would reduce the mean upward motion of air, resulting in reduced rainfall and a tendency toward further decrease in vegetation.) His interest in this topic was stimulated by the drought in the Sahel and by his fond recollection of spring trips to the Mojave Desert with his parents. This was his first visit to Israel, although he had received several invitations. His parents were not religious and Jule himself seems never to have taken up any part of his ancestors' faith. However, in several letters from Tel Aviv to friends back home, he described his trip as a "moving experience" and referred respectfully to "the toughness" of the Israeli.

The last months of this sabbatical were devoted to leading a summer workshop in Venice. This annual event had been started several years earlier by R. Frassetto of the *Institut per lo studio della dynamica delle grandi masse* and the oceanographer A. Robinson from Harvard. The drive behind this workshop was to help reduce flooding in Venice; the successful operation of a massive floodgate project would need accurate prediction of water level in the upper Adriatic. The fluid dynamical model developed by the Harvard group had treated the influence of tides and atmospheric wind and pressure as known forcing functions on the Adriatic. The success of this model shifted the emphasis to predicting the atmospheric wind and pressure. Jule's workshop was therefore on meso-scale meteorology, which, although somewhat different from the large-scale problems that had occupied him, was very important for the mountainous orography in the Italian area. In following years he continued to influence Italian science by sponsoring and working with several Italian students and postgraduates in his National Science Foundation project at MIT.

All the above activities illustrate Jule's sense of responsibility as a scientist in matters for which he had some unique insight and power, where he would be expected to lead and for which it was reasonable to expect success. His personal sense of responsibility was broader, however, as most of his friends can attest. The most ambitious of these efforts began in May 1970 after the invasion of Cambodia by U.S. forces and the tragedy at Kent State on May 4. Jule, Lois, and S. Luria conceived the idea of soliciting money from academic people to support antiwar candidates in the upcoming elections. With the help of A. Robinson and other Cambridge faculty members, they organized the Universities National Antiwar Fund. Chapters were organized at several hundred campuses and enabled UNAF successfully to solicit the equivalent of a day's salary from thousands of people. In this way about $250,000 was donated to dozens of carefully selected antiwar candidates in the primaries and the November election.

TWENTY-FIVE YEARS OF RESEARCH AND TEACHING

At MIT Jule continued to be a prolific creator of new ideas on the dynamics of atmospheric motion. Space here allows only a short description of the most significant.

During Jule's brief stay in Chicago in 1946-47 C.-G. Rossby had emphasized the existence of internal modes of oscillation for Rossby waves, and in 1948 Jule had used the vertical component of the group velocity in a resting atmosphere to estimate the rate at which influences from above the observed atmospheric volume would corrupt a forecast in the region below. This continued to occupy part of his thinking; for example, in a 1954 letter to D. Martin at Oxford Jule mentioned his interest in studying the upward propagation of Rossby wave energy. In 1961 this interest crystallized into a paper with P. Drazin. This time the important effect of a west-to-east flow in the undisturbed atmosphere was acknowledged. In this paper and in the Charney-Platzman conversations Jule states that the main goal was to show that this propagation is inhibited (i.e., little energy of this type reaches the very high atmosphere), so that a corona, or extremely high temperatures, would not be produced by viscosity. In 1982 a more direct proof of this was suggested by R. Lindzen and M. Schoeberl. The 1961 Charney-Drazin paper is, however, most important for two other less dramatic but more tangible results:

1. Subject to the limitations of WKB analysis, Rossby waves cannot propagate latitudinally or vertically if the wave moves either eastward or too rapidly westward relative to the basic zonal current.
2. Rossby waves will have no nonlinear effect on the basic state unless there is some non-conservative aspect to their motion.

The first of these gave an immediate qualitative explanation of the near absence of Rossby waves in the trade winds of low latitudes and in the westward flow that characterizes the summer stratosphere. Both results have since been extended and amplified in many ways by theoretical and observational scientists, although Jule's attention was quickly attracted again to another aspect of quasi-geostrophic motion.

In 1961 the traveling MIT-Woods Hole seminar heard a presentation by M. Stern on an extension of the Rayleigh condition for stability of a plane-parallel fluid flow (that the vorticity be monotonic). Stern had extended this to the case of a rotating homogeneous fluid with a free surface. Charney's interest was ignited by this; he and Stern then showed that an internal jet in a rotating atmosphere must be stable if the potential vorticity is monotonic and the temperature at the ground is uniform. The former condition is usually satisfied in our atmosphere; the latter is not and is therefore an important element for storm formation.

Hurricanes engaged Jule's attention ever since his car was damaged by a falling tree in 1954 as Hurricane Carol passed over Woods Hole. His first attempts at a numerical model for hurricane motion were unsatisfactory, however. In 1962 when A. Eliassen was a visitor to Jule's National Science Foundation project at MIT, he and Jule returned to the subject, this time considering the question of how hurricanes grow into strong vortices. In his conversations with Platzman Jule recalled that it was K. Ooyama (also a visitor at MIT) who first pointed out that the simple vertical stability considerations traditionally applied to explain individual cumulus clouds did not apply as a whole to the much larger hurricane cloud system. Jule and Eliassen then directed their approach to recognize that the storm was in a state of near dynamic balance and that it must be the frictionally induced indraft of air near the ocean surface that supplied water vapor and latent heat to the vortex. (As an example of Jule's intensity, I recall that much of the final work on this problem took place in the last part of Eliassen's visit, when Jule arranged for them to go off into the New England forests to avoid distraction.)

In 1971 Jule published a short paper on a subject that seems highly abstract, yet is of deep practical significance: the spectrum of large-scale quasi-geostrophic motion in the atmosphere. Kolmogorov and Obukhov in the Soviet Union showed many years earlier that the inertia spectrum of three-dimensional homogeneous isotropic turbulence varied as the minus 5/3 power of the wave number. This is the type of motion created in a wind tunnel and in more recent years has been found under windy conditions in the layer of air near the ground. Several theoreticians had in the meantime considered two-dimensional turbulence as a pure mathematical abstraction (there seemed to be no way to create it experimentally!) and had arrived at a steeper law—wave number to the minus 3 power. Such a flow would seem much smoother to an observer than would the conventional wind tunnel pattern.

Jule was able to present a convincing argument that a system governed by the dynamics of his quasi-geostrophic equations would have a spectrum like that of the hypothetical two-dimensional case, even though its motions are three-dimensional. The practical significance of this (although not emphasized by Jule) is that a weather map constructed from scattered observations makes more sense under the minus 3 law than it does under the minus 5/3 law and all of meteorology leading up to Jule's appearance in 1940 was based on such maps. Without these maps there would have been no meteorological group at UCLA!

Jule's last major research was performed in 1978, only a few years before his death. It had all the pathbreaking characteristics of his previous work. This time it was the result of a seminar he conducted at UCLA where he proposed that they jointly study dynamical models that might explain long-time variability in the atmosphere. The outcome was a joint paper with J. DeVore—a member of the class—on a dynamical theory of blocking. (This meteorological term refers to the sporadic occurrence of large high-pressure systems in preferred positions in middle and high latitudes. By lasting long enough and moving slowly enough they influence the overall hemispheric weather pattern for several weeks. There was no accepted theory for them in 1978.)

The basic dynamical model that Jule suggested to the class was the same as the one he and Eliassen had used thirty years earlier in 1948. But since nonlinearities were presumably important in this problem the class employed a Galerkin-like technique that E. Lorenz and B. Saltzman used in other nonlinear fluid problems—the use of severely truncated trigonometric series. Mountains were included. The results showed that under certain conditions the system would have two stable states, one corresponding to a strong west-to-east current with traveling waves and the other corresponding to a weaker zonal current with large-amplitude quasi-stationary waves. The latter resembled blocking. This research initiated much further exploration of this subject by other fluid dynamicists—an activity that always followed Jule's major papers.

Jule's teaching load was never more than one course and his lecture performance was often halting. But his stellar performance as a mentor for his thesis students more than made up for these defects, if such they were. At times considerable effort was required on his part to avoid interrupting their progress with his travels. His National Science Foundation project (typically funded for three years at a time) always included support for about five graduates in addition to a postdoctoral visitor. He shared supervision of some students with other faculty, but Jule was the sole supervisor of most of them, especially in later years when his GARP duties diminished.

He made a special effort to entrain into atmospheric science students and postdocs who had been educated in related fields, such as fluid mechanics and applied mathematics, and this often required as much personal attention as a beginning thesis student. In several instances the efforts he made to support and educate young people from abroad have had a major impact on the atmospheric sciences in their homeland. He worked hard, for example, to support several students affected adversely by the military takeovers in Argentina.

The housekeeping-type committee work associated with academic life was not to his taste and he successfully avoided it. However, he always took a keen interest and personal responsibility in the faculty selection process and served as department head from 1974 to 1977. His appointment as Sloan Professor in 1966 led ultimately to a modest stipend for his personal use. Some of this he dedicated to enhancing the computing facilities for the department, which at that time had no disposable money. In the isolation of Princeton he found it necessary to start a small reading collection in meteorology and related fields. The National Science Foundation continued some support for this in Cambridge and the "Charney reading room" across the corridor from his office became the main library for students, faculty, and visitors in meteorology and physical oceanography.

Jule will certainly be remembered for his research in atmospheric and oceanographic science and for his insight and initiative in the global atmospheric research program. But future chroniclers may well rank his students as an equally great contribution. R. Goody said it well at the memorial service for Jule held in 1982: "As a teacher Jule molded the thoughts of several generations of students. We shall be completing his thoughts and building upon them for a long time to come."

A. ELIASSEN, R. ELLIOTT, G. Golitsyn, T. Malone, G. Platzman, A. Robinson, and L. Swirnoff helped me on many points in correspondence and conversation.

Jule's personal papers are archived at MIT as "Manuscript Collection MC 184." They consist of about 27 cubic feet of records, fully archived. I thank Ms. H. W. Samuels and her staff at the Institute for help in examining this collection.

The National Center for Atmospheric Research published a verbatim transcript of an interview with Charney recorded in August 1980, *Conversations with Jule Charney* by George W. Platzman. NCAR/TN-298+Proc (1987).

The American Meteorological Society published a memorial volume titled *The Atmosphere—A Challenge* and subtitled "The Science of Jule Gregory Charney," edited by R. Lindzen, E. Lorenz, and G. Platzman (1990). Besides a full list of his honors, publications, and appointments it contains eleven essays on Charney and his work, reprints of five landmark papers, a series of photographs, and an edited version of the interview with Platzman. I made considerable use of the essay by M. Wurtele on Charney's youth.

The development of interest in numerical weather forecasting at Princeton is described by F. Nebeker in chapter 5 of his thesis, *The 20th Century Transformation of Meteorology*, Princeton University (1989).

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