Selected Papers of Jule Gregory Charney

Dynamics of Large-Scale Atmospheric and Oceanic Processes

Edited by J. Shukla

DYNAMICS OF LARGE-SCALE ATMOSPHERIC AND OCEANIC PROCESSES

Selected Papers of Jule Gregory Charney

edited by

Jagadish Shukla

Center for Ocean-Land-Atmosphere Studies
Institute of Global Environment and Society
and
School of Computational Sciences
George Mason University

A. DEEPAK Publishing 2001
A Division of Science and Technology Corporation
Hampton, Virginia USA

CONTENTS

SELECTED PAPERS OF JULE G. CHARNEY

PREFACE

Jule Charney was one of the greatest leaders of the 20th century in the fields of meteorology and oceanography. He started his glorious journey as a research scientist by providing a theoretical explanation for large-scale waves in the atmosphere, advancing the theory of baroclinic instability, and developing the quasi-geostrophic system of equations which became the basis for numerical weather prediction. He was a brilliant theoretician; however, he used his deep physical insights and his creative genius to understand and predict observed phenomena that have direct applications to the welfare of society. Charney provided the scientific justification and the overall intellectual leadership to launch the Global Weather Experiment which some have called the Charney Experiment. His scientific contributions lead to the creation of several new areas of research: assimilation of satellite data, tropical dynamics, geostrophic turbulence, dynamics of deserts, multiple equilibria and predictability of monsoons. Not only a great scientist, Charney was a renaissance man with deep interests in music, history and world events. He encouraged research collaboration, and he was a coauthor of numerous papers. To give the readers a glimpse of his humanity, I asked his doctoral students to write reminiscences. Their responses are both illuminating and amusing.

During May 1981, I asked Charney if he would be willing to have his scientific writings published in a single collection. He was emphatic that he would not be in favor of publishing 'collected works' that included all his papers; however, he was agreeable to a publication of selected papers. We intended to have some further discussions on the actual selection of the papers at our next meeting, which, alas, never took place because Charney died on 16 June 1981. This, of course, left open the difficult question of which papers to include, or, harder still, which to omit. In the present volume, the choice has been to include as many of Charney's papers as possible while omitting long chapters from books and works not published in journals. A complete bibliography of his works is also included. According to Jay Fein, the titles of Charney's proposals to the National Science Foundation were consistently "Dynamics of Large-Scale Atmospheric and Oceanic Processes." I considered this title to be appropriate for this volume.

This collection serves as a companion second volume to *The Atmosphere—A Challenge: The Science of Jule Gregory Charney,* edited by R. S. Lindzen, E. N. Lorenz, and G. W. Platzman published by the American Meteorological Society in 1990. More information about Charney's life and his works, can be found in "Conversations with Jule Charney" published as National Center for Atmosphere Research Technical Note 298 (1987) based on interviews by G. Platzman, and Biographical Memoirs of the National Academy of Sciences (1995) by N. Phillips. A brief essay on Charney's contributions during the last decade of his life is included in this volume.

J. Shukla
Editor

ACKNOWLEDGMENTS

Acknowledgments are gratefully extended to the large number of people who have helped in the production of this volume. Friends and colleagues of Charney met on 17 December 1981 and agreed that a volume of Jule's collected works would be prepared with Mark Cane as editor and that I would assist him. Mark and I worked sporadically towards the preparation of this volume and we solicited the help of many of our colleagues in identifying corrigenda for Charney's papers. I would like to express my deep gratitude for Mark's indispensable efforts during the early phase of this endeavor. Due to other commitments, Mark Cane was unable to remain a co-editor.

I extend thanks to all those preeminent scientists who took their all-too-scarce time to peruse Charney's papers, give comments, and suggest corrections. The future generation of readers of these papers will be thankful to these volunteer reviewers. The names of the reviewers and the titles of the papers they reviewed are listed below.

Sharon Bushing and Mark Suskin were instrumental in taking care of all the logistical problems associated with publishing a compilation of papers. They handled correspondence with the many parties involved, and made sure that we got all of the permissions needed. Thanks go to numerous organizations which granted permission to reproduce Charney's papers. Furthermore, Mark Suskin collected and proofread most of the material other than Charney's papers themselves.

Many thanks to all of Charney's doctoral students (their names and years of Ph.D. are listed on the contents page) who kindly agreed to contribute and to share their personal impressions and memorable stories.

Finally, thanks to Nora Charney Rosenbaum for letting me borrow photos of her father, and to Diana McQuestion and A. Deepak Publishing, for seeing the merit in this project.

Names of the Reviewers, and the Title and Year of the Paper:

J. Pedlosky: *The Dynamics of Long Waves in a Baroclinic Westerly Current* (1947)

J. Holton: *On the Scale of Atmospheric Motions* (1948)

E. Kalnay & N. Phillips: *On a Physical Basis for Numerical Prediction of Large-Scale Motions in the Atmosphere* (1949)

A. Eliassen: *A Numerical Method for Predicting the Perturbations of the Middle Latitude Westerlies* (1949)

N. Phillips: *Dynamic Forecasting by Numerical Process* (1951)

N. Phillips: *Numerical Integration of the Quasi-Geostrophic Equations for Barotropic and Simple Baroclinic Flows* (1953)

N. Phillips: *Numerical Prediction of Cyclogenesis* (1954)

G. Veronis: *The Gulf Stream as an Inertial Boundary Layer* (1955)

K. Emanuel: *Integration of the Primitive and Balance Equations* (1960)

Y. Ogura: *A Numerical Model of Thermal Convection in the Atmosphere* (1960)

P. Drazin: *Propagation of Planetary-Scale Disturbances from the Lower into the Upper Atmosphere* (1961)

M. McIntyre: *On the Stability of Internal Baroclinic Jets in a Rotating Atmosphere* (1962)

D. Duffy: *Numerical Experiments in Atmospheric Hydrodynamics* (1963)

C. Leovy: *The dynamics of Large- and Medium-Scale Atmospheric Motions* (1963)

A. Eliassen: *On the Growth of the Hurricane Depression* (1964)

T. Delsole: *What Determines the Depth of the Planetary Boundary Layer in a Neutral Atmosphere* (1969)

J. Bates & J. Shukla : *Tropical Cyclogenesis and the Formation of the Intertropical Convergence Zone* (1971)

J. Kinter: *Impact of Computers on Meteorology* (1972)

J. Holton: *Movable CISK* (1974)

P. Stone: *Dynamics of Deserts and Drought in the Sahel* (1975)

D. Straus: *Form-drag Instability, Multiple Equilibria and Propagating Planetary Waves in Baroclinic, Orographically Forced, Planetary Wave Systems (1980)*

K. Mo: *Comparison of a Barotropic Blocking Theory with Observation* (1981)

REMINISCENCES OF CHARNEY'S DOCTORAL STUDENTS

Francis W. Murray

My first acquaintance with Jule Charney came in 1942 when I was assigned as an Aviation Cadet to the wartime Meteorology course at U.C.L.A. Charney had gone through the course a couple of classes ahead of me as a civilian and had stayed on as a synoptic lab instructor. As an added duty, he gave lectures on atmospheric radiation during the frequent absences of Professor Joseph Kaplan. Of necessity, our interaction was rather brief and distant, and after I had finished the course and gone to Air Force duty elsewhere, our paths did not cross for several years. By the time that I returned to U.C.L.A. for another year of study, he was finishing up the work on his thesis, and again I saw little of him. In the ensuing years, I followed with interest his pioneering work in numerical weather prediction, but our real relationship did not start until 1957, when the Air Force sent me to M.I.T. for doctoral studies. Charney had just come to M.I.T. from Princeton, and somehow it was a foregone conclusion that I would work under him, which I considered to be an honor.

For me it was a somewhat difficult experience to return to academic life after ten years in the field, and for him it was also a great change to assume the teaching responsibilities of a professor after full time research work. His courses were exciting and challenging because he was not going over ultra-familiar material that he had taught repeatedly before. Rather, some new idea might come into his head during the night, and he would come to class the next day eager to present it even though it was not thoroughly worked out. That sometimes led to a bit of confusion and backtracking, but it was always stimulating.

His habit of working late was well known. He never scheduled morning classes, but preferred to start at 2 p.m. He was one of the leaders of a joint seminar with Woods Hole that met every two weeks, alternating between Cambridge and Woods Hole. So every month a group of us would pile into carpools in the early afternoon for the rather long drive, followed by a late afternoon seminar and then dinner. The first year that Charney offered his course in planetary fluid dynamics, starting at 2:00, as usual, he found that it was too hard to get to Woods Hole on time after class. He asked if the class would be willing to change to another day of the week, but that interfered with the schedules of several class members. Someone suggested starting the class at 1:00, but Charney demurred. Finally mathematics professor C. C. Lin, who was sitting in on the course, broke the impasse by saying, "Come on, Jule. You will only have to get up an hour earlier."

The thesis topic Charney suggested for me to work on was an extension into the baroclinic stratosphere of his earlier work on dynamic stability, and I needed plenty of guidance on it. Communication was a bit inhibited by his working hours but more so by his frequent absence on projects such as setting up N.C.A.R. To top it off, the Air Force transferred me to Omaha before the work was finished, but through it all, Charney was most helpful, even arranging to bend the rules at M.I.T. to allow me to do some thesis work in absentia. To top it off, when the thesis was finished, he cleared the way for rapid publication.

Since writing a doctoral thesis was obviously a new experience for me, and advising his first doctoral candidate was a new experience for him, we were both feeling our way. I am sure that others of his students were closer to him and had a better insight into his qualities and his work, but perhaps my rather early experiences with Charney will add a little to the overall picture.

Conway Leovy

It was my extraordinary good luck to be involved peripherally in two of Jule Charney's most influential contributions, with Phil Drazin on vertical wave propagation and with Melvin Stern on the stability of quasi-geostrophic flow. The problem of vertical propagation, in particular, had bothered Charney for some time, and the simplicity and beauty of the approach that he and Drazin hit upon, clearly delighted him. Details were worked out quickly, some of them over several late night working sessions driven as well as guided by Charney's quick insights and clear ideas.

But Charney was broad as well as deep. He believed passionately that science and technology should benefit the whole public, and he understood clearly that new technologies, thoughtlessly deployed, could have unexpected negative consequences. By the autumn of 1970, I had convinced myself that a large commercial supersonic transport fleet could release enough water vapor into the stratosphere to impact the ozone layer. I was invited to discuss my ideas at a press briefing that preceded key congressional votes on SST funding. To my surprise, Jule Charney was also there, the only other scientist present, and I think he was as pleased as I to meet in those circumstances. He had quickly grasped the scientific issues as they were understood at the time and also came to believe that the environmental risks were too great to commit to deployment of a large SST fleet without better understanding of the environmental consequences. In the end, of course, the chemistry of the ozone layer turned out to be far more complex than either Charney or I anticipated. But the incident reveals the breadth of his interests and his willingness to dive into the messy terrain at the intersection of science and public policy when he felt the urgency of the issue demanded it. I sometimes wonder how Charney's influence would have shaped the debates over global warming and other great environmental policy issues of our time.

Joseph Pedlosky

Jule was an inspiring thesis advisor. He was kind and direct. He treated me as a colleague rather than as a student almost from the beginning, and this seemed natural to him. Rank seemed to matter very little. He was interested in the person and what the person could do in science. I think all of his students were struck by the respect with which he treated them and the warmth of his personality.

I remember two moments particularly keenly.

We were once, the two of us, driving down to Woods Hole from Boston for one of the biweekly GFD seminars that were such a focus of our lives in those days in the early '60s. As we turned onto the Southeast Expressway in Boston, Jule, while driving, started to talk about his connection to the great chain of researchers in 19th century European science, He considered his own advisor and mentor really to have been Rossby, who had been guided by V. Bjerknes, who had been taught by his father Christian Bjerknes, who in turn had worked with Hertz, who had been following in Maxwell's path. He spoke about this very feelingly, and it was inspiring to sense the direct connection he felt with his heroes of 19th century physics. Of course, it was natural and inspirational for me then to feel a little bit included in that chain.

The chain might easily have ended right there for both of us, for while Jule was deeply involved in this rumination he had also (I think automatically so, given his peripatetic life) taken the expressway exit for the tunnel to Logan airport. Not wanting to get trapped in the chaos of Boston traffic, and already being somewhat behind schedule for reaching Woods Hole, Jule decided the best way to solve the problem was to back up the ramp and back onto the expressway around 3:00 p.m. on a Friday afternoon. I wonder if Hertz ever had such a close call!

The other moment was in the early phase of my thesis work. I was still grappling with identifying a problem and trying to find a problem at once big enough and tractable enough to serve as a good thesis problem. Jule, as I said, was content to let a student think out this important part of the Ph.D. process independently. When I though I had a problem that seemed interesting, and that I thought I could do, I went to him to describe it and to ask him if he thought it would do for a thesis problem.

"Yes," he replied, "If it turns out to be fascinating."

That seems to me the best answer anyone could have given and explains pretty clearly the quality of his mind, his standards, his work, and what he expected of others. I still hope I keep that standard now.

James Holton

My most vivid memories of Jule are of his wonderful lectures in his famous course, number 19.67, "Planetary Fluid Dynamics." The published version of these lectures, although it remains a valuable resource for dynamicists, does not really capture the spirit of Jule's classroom style. Owing to Jule's practice of developing his lecture material in real time as the lecture unfolded, these lectures not only introduced the student to the intriguing realm of stratified rotating fluids, but provided a fascinating window into the workings of a great scientific mind. Occasionally this lecture technique resulted in long pauses and unusual digressions. (I recall one particularly delightful digression on the migration habits of the lemmings.) But even the digressions and dead ends were a valuable part of the learning process. One can only wonder whether Jule would have been able to retain his unique style in this era of formal "objective" student ratings of teaching.

Isidoro Orlanski

I arrived in the summer of 1965 from Argentina, with a small stipend from the University of Buenos Aires. Those were very tumultuous years in Argentina's life. Within a few months after my arrival, the military took control of the Argentine government in a very repressive way. The military government was very harsh, in particular toward the academic population. We, some of the Argentineans in Boston, felt we should protest this barbarity by resigning our university positions and fellowships in Argentina. Needless to say, since I was married with a child, my position in the United States was very precarious, to the point that I was planning to drop my studies and return to Buenos Aires. Charney, who by that time was already in California, found out about my intentions, and called me at home to convince me to give up my plans. In less than a week's time, he arranged for M.I.T. to extend me a loan that allowed me to complete my studies.

This incident, though small, was very moving for me, and reflects the human care of which Jule was capable. At the time, I also witnessed his immense respect for human rights that manifested itself in his deep preoccupation with the repression of Argentinean scientists. This was one of the many issues that Jule had participated in during his active life.

John R. Bates

I first met Jule Charney as a student in his Dynamic Meteorology class at MIT in the spring of 1965. I had come to MIT the previous fall after spending a period as a weather forecaster at Shannon Airport, Ireland. Being already very familiar with the name Charney from my time in meteorological training school, I expected that this famous scientist would be a grey-haired Eminence. To my great surprise, he turned out to be a very young looking man of forty-seven, with his hair black and a bound in his gait.

Though Charney's enthusiasm was infectious, his teaching style was not a model of organization. He set out to derive the quasi-geostrophic potential vorticity equation on the blackboard as if he were doing it for the first time, thinking his way through each step, erasing and rewriting. The result conveyed the excitement of fresh discovery, but was not designed to encourage the faint-hearted. We asked if he would allow us to tape his lectures. He demurred, saying that this would completely inhibit him. Later in the course a set of typed notes was handed out, which compensated in clarity for the near-confusion of what we had been able to copy from the blackboard. I did well enough in Charney's course to be accepted as his Ph.D. student, which was what I had hoped for when first applying to MIT.

To be Jule's student was to be carried along on a grand adventure. To him, meteorological research was clearly the most exciting enterprise that one could be engaged in. By then, his early work on numerical weather prediction had flourished to the point where countries all around the world were adopting operational models. Dynamic meteorology was in a stage of rapid growth. The Global Atmospheric Research Program was in the planning stages. The National Center for Atmospheric Research Program was being established. In all of this, Jule played a leading role. His scientific eminence, combined with the magnetism of his personality, placed him at center stage. It was clear that he enjoyed his fame.

He was enormously generous to his students, treating them as his peers, involving them in his projects, taking them along on some of his travels. Students whose spirits were flagging or whose theses were on the doldrums had only to talk to him to have their energies renewed.

In 1967, I accompanied him on his six-month sabbatical to UCLA. Here he was at home and obviously relaxed, enjoying the sunshine and the memories. He took a deep interest in the work of Arakawa and Mintz, who were then producing their early climate simulations on the UCLA computer.

In the summertime of 1969, I went with him to Barbados to take part in the BOMEX experiment. Unlike most of his contemporaries who had entered meteorology as forecasters during World War II, Jule had never been a forecaster, having transferred to meteorology from being a graduate student in mathematics. This was probably the first time he had been deeply involved with observations. In his role as Chief Scientist, he eagerly examined the data brought back from flights, carefully studied the geostationary satellite photographs, and himself spent long periods in the air, flying in hot and humid surroundings at low levels over the sea. Observation would preferably fit theory, but if not, theory would have to give way to observation.

Back at MIT, the lights in his fourteenth floor office in the Green Building were often to be seen burning late into the night. He was not one to rest on his laurels. Nevertheless, he would always accept invitations to student parties and get-togethers, and would often be the last to leave. He liked to reminisce about his student days, and the days in Norway and Princeton. On one of these occasions, I asked him if it had ever occurred to him when working on his Ph.D. thesis that a large number of people would still be working out the details of it twenty years later. He admitted that, in fact, it had. On another occasion, he related that when he visited L. F. Richardson in England to tell him of the successful integration of the barotropic vorticity equation on the ENIAC computer at Princeton, Richardson had displayed only the mildest interest.

I had the pleasure in later years of welcoming him to Ireland, about which he knew a surprising amount through his reading of Irish literature. I also saw him on some of his trips to Europe when he was involved in the Save Venice Campaign. It seemed fitting that he should be running a model of the Venice floods on a computer housed in a palace on the Grand Canal, whose walls were adorned with frescos and tapestries.

All who were his students will always be thankful for their good fortune to have known him, to have shared the excitement he generated and the sense of optimism he conveyed. He was a true scientist. I shall always remember him with admiration, fondness, and gratitude.

Eugenia Kalnay

Charney was a feminist ahead of his time. He had a male secretary in a world in which careers were stereotyped as male or female, and he chose secretaries for their intelligence. There was a rumor that he interviewed and selected one, who turned out to be not very bright, because she had been carrying a book by Schopenhauer (which she was returning to the library as a favor to her roommate). His office door was hard to close, and he used to slam it. My office was across from his, and every time the door was slammed, I had the sinking feeling that Charney was angry with me because of my lack of progress with my thesis. In reality, he was always very supportive and encouraging.

I did my thesis without much interaction with Jule. I saw him once about every three months, but each time we got together he was an incredible source of stimulation and inspiration. I remember particularly one session. I had started my thesis trying to test, using a numerical model, the validity of Goody and Robinson's theory to explain the high surface temperature of the atmosphere of Venus. Their idea was that the radiative heating and cooling at the top of the atmosphere drove a deep circulation that maintained the atmosphere almost neutrally stable down to the surface. Goody and Robinson's idea seemed to hold for a Boussinesq model, but with a quasi-Boussinesq fluid the results were not clear, and I deluded myself into thinking that they were still "qualitatively right." Charney looked at my results and immediately said, "Maybe Goody and Robinson are wrong!" And this was indeed one of the main results of my thesis. The shock of realizing that I had not been looking at my results with an open, critical mind was a tremendous lesson. Although we did not spend much time together during the development of my thesis, he went over my draft with utmost care, and I learned an awful lot from that reviewing process alone. One piece of advice that has helped me ever since is: "If you want to say something in your thesis, say it clearly, don't just imply it or give a hint."

Charney was not just a great scientist, but also a great human being. He became a leader in the anti-war movement at MIT in the mid-60s, which was a source of pride for me, and a consolation at a time in which I felt guilty to be in the US because of the war in Vietnam. Later, at the beginning of the "dirty war" in Argentina, a friend of mine, Carlos Cardelino, who had been jailed and tortured for a year in Uruguay in a case of mistaken identity, went to teach in an Argentinean university. He was expelled during a right wing purge, and could not return to Uruguay, while in Argentina death squads were making hundreds of "suspicious" people disappear every day. I told Charney of Cardelino's truly desperate situation, and he immediately accepted him as a student in the Department of Meteorology, even though his background was in computer sciences. Cardelino got an M.S. in Meteorology, his wife got a Ph.D. in Chemistry, and both they and their three children now have extremely productive careers in the U.S. I have no doubt that Charney saved his life.

Paul Janota

I took my general exams in the fall of 1963 after about a year of course work. In those days, the process took about two weeks and consisted of several hours of open and closed book exams, two 24-hour take-homes, and a final set of orals by several faculty members. I was so distracted during this period that, on at least two occasions, I drove my car in from Watertown and took the bus home. Dr. Charney's take-home question was (in retrospect) a simple application of his scale theory to flow on a rotating plane wherein the essence of the solution was in the Ekman layer transport. I was so intimidated by his reputation and stature that my knee-jerk reaction was to duck his question and work on the alternate question from another professor. Thus, it should have come as no surprise when Jule showed up on my orals list, and as my final questioner, no less. The word was that I should prepare for him by boning up on various boundary layer hypotheses, which is what I did. As our session began, he stretched out full length on his office couch and asked me to go to the board and discuss classical boundary layer models (hurrah!). Unfortunately, I was too well prepared. After about 10 minutes, he conceded that I was an expert in the field and gently suggested that I use the remaining time to work out his take-home problem instead (doom!). I can still clearly remember the absolute feeling of dismay as my legs turned to jello and my mind emptied of all coherent thought. He suggested that I might start the analysis by writing the equations of motion in cylindrical coordinates; I could not do it.

He suggested Cartesian coordinates; still no luck. The chalk hung limply in my worthless hand. Then, he did the most wonderful thing. He walked over and led me through each basic step with patience and gentle humor until my gears began to mesh again, and I could finish the analysis myself. This simple, caring act salvaged my dignity, restored some measure of self confidence, and may have taught me a little fluid dynamics in the bargain. But the enduring lesson will always be his humanity and genuine concern for my academic future when it would have been perfectly understandable if he had simply let me fail.

It was probably 1963, and Jule was hosting an informal beer party for our Planetary Fluid Dynamics class. At some point, I told him about the following encounter I had had a few days earlier. I was heading home on the Mass Avenue bus reading Charney and Stern's paper on the stability of internal baroclinic jets. Suddenly, a rumpled and unlikely looking little man sitting next to me looked at what I was reading and went into a minor tirade about Charney's work. In particular, he accused scale theory of throwing out all the important terms in the equations of motion, and went so far as to call Jule a "bandit." In the few minutes we sat together, I was busy patronizing him with the many advances the theory had produced, while he tried to convince me to give it up as a bad job. As we parted at Harvard Square and I finally asked who he was, he suggested that I read his book, *I Prodigy*, and then shuffled away toward the T-stop. When I finished the story, Jule roared with laughter saying something to the effect, "Norbert Wiener called me a 'bandit,' that may be the greatest compliment I've ever received."

Arthur Bass

Although he may not have subscribed overtly to the ethos of his forebears, I know that Jule Charney understood it and lived it in a profound way. What I treasure most about Jule, and remain most deeply grateful to him for, is how spontaneously and intuitively Jule animated the ancient Jewish maxim that 'He who saves one life saves the World.'

Case in point: Late in March, 1969, heartsick about having been hiding out successfully for some years in a meaningless Defense Industry job to avoid the Vietnam War draft, I came to MIT looking for a job as a scientific programmer. (In hindsight, I was really looking to reclaim my self-respect.) It was my great luck to be introduced to Jule, who understood immediately and with great compassion, how desperately I wanted out of the Military-Industrial complex. Jule said, "I'm very sorry I can't offer you a job—I already *have* a programmer— but if you were a *Graduate Student* in the Meteorology Department we could give you a little income: an Assistanceship, maybe a Fellowship, something in any event."

He held out this possibility to me knowing full well that, except for an MS in Physics and some rudiments of atmospheric transport modeling, I had *no* background, *no* academic qualifications, and (until that very moment) *no prior inclination*, to pursue a

Graduate program in Meteorology—at the world's strongest Meteorology Department, no less. Although thrilled at the possibility I thought the whole idea absurd, not the least because applications for admission to the Course 19 Ph.D. program had been due three months earlier; because the Departmental Admissions Committee was only days from announcing its decisions for the coming academic year; and because I had been candid with him about my less-than-sterling Graduate School career six years earlier. But Jule said, "Never mind, just get your application in and I'll see what I can do." Within what in retrospect seems like only days, I was admitted to the Ph.D. Graduate Program with a fellowship. Jule had cared enough to 'save one life...'

Mark Cane

In the late '60s I was working as a programmer at GISS on a meteorological satellite project. I knew virtually nothing about meteorology, and it quickly became apparent that no one else involved did either. Since these were smart and knowledgeable scientists, I concluded that the atmosphere must be an unmitigated mystery. At some point this consultant, Jule Charney, came down from MIT. It was magic: I couldn't conceive how anyone could have such a feel for the workings of the atmosphere.

Life went on, I had a child and left GISS to think things over in the woods of New Hampshire. The decision I reached was to apprentice myself to the magical scientist, so I entered the MIT Meteorology Department.

I hope someone else has done better than I in conveying the special atmosphere of the 14th floor. It was an intellectual boot camp; we spent most of our waking hours (and some of the sleeping ones) there, and the common experience created a lifelong bond among us. There was graduate student pain and suffering, but intellectual excitement was paramount. Ideas were shared very freely. It stemmed from Jule: he was more interested in learning something new than in being right. (I realize this is saying a lot since he had such a strong need to be right—and to be recognized for it.) He was impatient with fuzzy thinking, but could get very excited by new ideas—whether they came from others or himself.

The atmosphere held up in the not infrequent times Jule was off somewhere. Much of the 14th floor cohort was collected in his global wanderings. There were frequent visitors, and every advisee got used to having his appointment displaced to accommodate them. We would later come back in the other role, but not without sympathy for the graduate students now kept waiting because of us.

I learned more from Charney's course than any other I ever took. Which I still think of as odd, since by any conventional standard the course was taught terribly. He was obviously bored by having to lecture on this stuff yet one more time. So a few of us studied the notes thoroughly enough to be able to ask questions for the whole hour and a half—whatever it took to spare him from lecturing. Obviously I had to learn a lot to do that. But the real bonus was that this way his mind was engaged, and it was exhilarating to go along for the ride.

Jule invariably started a seminar on his latest work with a data picture or two to set the problem—he always had some phenomena in mind. Very often, he would start explaining the picture and pause ... something he hadn't noticed before caught his eye. Sometimes the pause could go on for quite a while, with the audience wondering what was in store for us.

Whenever I do a piece of work, it still comes into my head to consider whether Jule would be pleased. I have done a few things I think he would really like, and that pleases me.

Dean Duffy

It's difficult to express how Jule Charney affected my life. The best people do it so naturally that you don't realize what's happening. Of course, I could list his demanding planetary dynamics course, the elegance of his scientific thought, his ability to draw the very best students and post-docs to the 14th floor. But, for me, the best times with Charney were after I left graduate school.

During my tour with the Air Force, I was fortunate enough to get away for two weeks at MIT during each January and May. I always looked forward to these return trips so that I could discuss with Jule (of course, I would never say "Jule" to him) what I was doing. These discussions occurred during the daily 9th floor teas or late in the day when he left his doors wide open to let you know that he was available. Some times someone notable would drop by. This was Jule at his best, just you and he.

It's been many years since those meetings. Of course, many of his papers are still studied by the next generation of meteorologist. However, I know that he would view with equal pride his legacy as teacher and human being.

Jagadish Shukla

It was a privilege for me to know Charney for about ten years, at first as an awed graduate student and then as a colleague and friend. We spent many hours discussing meteorology; we cooked and ate meals together; we even played soccer with our friends and colleagues when Charney stayed with us in Maryland. He was a regular visitor to the NASA Goddard Space Flight Center, after M. Halem's group moved from New York to Greenbelt, Maryland. From those years I could tell a multitude of fascinating stories ranging from outrageous to not so funny. While many knew of Charney's somewhat erratic driving habits, my initiation came when he suggested that we make a U-turn on the Baltimore-Washington Parkway because we had missed an exit for the Baltimore airport and he was getting late to catch the flight to Boston. He did make a generous offer that if we got caught by the police, he would pay the ticket.

Charney was an animated conversationalist, especially after a few glasses of wine. On more than one occasion I heard him say: "I better not say anything further, I have had some wine and I might tell the truth." I particularly remember once Charney expressing some disappointment that once when he spoke with Einstein briefly, Einstein was not aware of Charney's research. Charney seemed to regret that, in spite of proddings by von Neumann, he never took the initiative to see Einstein to describe his work.

Of the many meetings over the years, I would like to describe both the very first encounter I had with Charney on 30 November 1968, and the last meeting on 13 May 1981.

30 November 1968:

Jule Charney was attending the international symposium on Numerical Weather Prediction in Tokyo. As an accident of the Indian bureaucracy, I was sent by the India Meteorological Department to attend this symposium and present my paper on vertical coupling in the tropics, a paper that had been prepared with the guidance of K. Gambo and T. Nitta of Japan. The theme of the paper was a criticism of Charney's earlier paper in which he had proposed that the tropical atmosphere has weak vertical coupling. Since my graduate education was not in meteorology, but in geophysical prospecting, I was not aware of Charney's contribution to meteorology. Since I did not recognize him, I needed to ask one of the local organizers to identify Charney for me. Something began to worry me when I noticed that every time Charney would get up to make a comment, there would be a complete silence in the room, and a battery of cameras would begin taking his picture. At the coffee break, he was completely surrounded by other participants.

By the time I had to present my paper, my worry had changed into complete fear for I began to realize that he was one of the most important persons at the symposium. Speaking nervously and so fast that the chairman reminded me a few times to speak more slowly,I somehow completed my presentation in less than 10 minutes and felt relieved when I saw no raised hands for questions. Then suddenly, my worst fear came true and Charney raised his hand. "I have four questions." Somehow I managed to argue back and forth with Charney, but he had the last word, stating that "Your paper proves my hypothesis."

What happened next is an unforgettable experience for me. At the end of the session, Charney took me to a blackboard and began to explain some more work he had done on the dynamics of tropical motions. I was in a daze. There were several quite important people who were waiting to talk to him, but he spent a good bit of time explaining his new work to me. He also asked me to come to his hotel so that he could give me a preprint of his new paper. His room was a real mess with piles of papers everywhere (something about GARP he said!). I was so impressed by Charney that when I returned to India, I applied for admission to MIT in Meteorology and wrote to Charney reminding him that we had met in Tokyo and that I would like to be his graduate student. Of course, he never replied. One day a postman delivered the MIT admission letter to my village from Phillips, who was then Chairman of the department.

13 May 1981:

On May 13, Charney called me at home at around 8:00 am and wanted to know if I could come to Boston to see him. I told him I was planning to see him the next day, but he indicated he would like me to come that same day. I told him that after I went to the office, I would check flights to Boston and let him know when I could come. He already had checked the flights to Boston and gave me the timings. By about noon, I was in his apartment. He looked very thin and weak and said his appetite was very bad. He prepared some lunch for himself and we ate some fruit. I volunteered to make his lunch, but he insisted on cooking it himself. At one point, one of the utensils fell in the soup, but he took it out and continued to cook. He insisted on carrying his own tray from the kitchen to the sofa and then to the round table, and in the process, he came close to stumbling and dropping the food, but he managed to hold on and to eat a part of his lunch. After a brief conversation about my one month old son, Chandran, we started discussing science.

Charney was very interested in the question of regional blocking. He was quite excited about his idea of variable zonal driving as being responsible for blocking to be regional and highly localized. He was no longer interested in the baroclinic extension of the barotropic blocking theory in which arbitrary topography and arbitrary diabatic heating were prescribed in the zonal direction. He was not in favor of prescribing the heating because he was very emphatic that the heating should be determined by the motion field. Then he interrupted our conversation saying that he had not called me today to discuss blocking, but some question about general circulation. He told me that he wanted me to be the thesis supervisor for Carlos Nobre and he was interested in discussing some problems of the dynamics of the tropical atmosphere. He began a discussion of possible thesis topics for Nobre by asking several questions: Why is the subtropical jet stream so sharp? He was not quite satisfied with explanations based on variations of temperature associated with the Hadley cell. He said he would understand the sharpness of the polar jet, because frontogenetic processes might be quite important in sharpening the polar jet, but those arguments do not hold for the subtropical jet. He stated that we must understand this phenomena. He alluded that non-linear effects must be important. Then he asked: How is the excess heat in the tropics transported poleward? Again, he was not satisfied with the Hadley cell explanation, and he said that these calculations were misleading because the circulation is not symmetric. It only meant the transports are poleward in isolated areas and therefore zonal averages show us more clearly how the transfers are accomplished and, in particular, what the role of low frequency planetary waves is in transporting the heat poleward. Then he asked: Why was the Hadley circulation so intense during the winter compared to summer? He was interested in Yale Mintz's idea of trans-tropical jets associated with the isolated heat sources in the tropics and was willing to consider the possibility that the zonal asymmetries of the sub-tropical jet are, at least in part, related to the asymmetric heat sources in the tropics. (Nobre would later investigate this question in more detail in his thesis.)

While we were waiting for Nobre to come, I told Charney about some recent work on the calculation of the circulation with prescribed tropical heat sources. He was silent for several minutes and then he said, "This is the kind of thing I do not want Nobre to do for his thesis." He wanted Nobre to understand the mean tropical circulation, and for that he was quite convinced one has to study the non-linear problem with planetary scale heating.

Later, Carlos Nobre came and joined us. We had ice-cream. Again, Charney insisted on serving the ice-cream himself. Then he discussed Nobre's proposal and his plan of study. Nobre and I left his apartment in the evening around 5:00 pm; by that time we were also exhausted because this had been a long and intense session. Just before leaving, I asked Charney if the doctors had found any clue to the persistent fever which he was still having and he said they had not. He was still planning to attend a conference in Europe.

That was the last time I talked to him; the next time I saw him was on June 16, 1981 around 11:00 pm on his hospital bed. Dr. A. Eliassen, Dr. Y. Mintz and I rushed from Washington after hearing from his friend Pat Peck that his condition was deteriorating fast. He had died about a half-hour before we arrived. It was an impossible sight for us. He had become so thin within a short period of time. I could not believe for a moment that this man could be lying so powerlessly because he had always been a source of energy for us. But then the present status of dynamic meteorology is a vivid example that Jule Charney lives and will always live with us.

Inez Fung

When Charney returned from a trip, he would go along the corridor, usually around dinner time and usually with a coffee cup in his hand, looking for someone to talk to about the latest development. As a new student, I was excited, but intimidated by these encounters. Once he said to me, "Why are you just nodding your head and agreeing with everything I say?"

Despite my efforts to learn Fortran, Charney insisted that I should solve my thesis problem, whatever it was going to be, analytically. I would get involved with numerical models soon enough. Solving the problem analytically required approximations. One of my first was to get a tractable representation of the Ekman spiral for instability analysis. Charney suggested to me, on his way out on another trip, that I should include a uniform mean flow in my layered model. While he was gone, I realized that what the problem required, of course, was the inflection point, and that the inflection point could not be captured with a uniform mean flow. When he returned, I said, "But what you told me to do was wrong!" Charney looked at me, smiled, sat down and said, "Now, tell me how I was wrong."

Later, I decided to use the two-timing approximation. "Why?" "Because it was used by several authors in their papers." "But what makes you think that they are correct? Why do YOU want to use it?"

I gave Charney a draft of my thesis before the Christmas holidays. On Saturday, I had left the office at 5 am, slipped and slid home on the thick treacherous ice. Charney called me at home at 10 am and asked if I wanted to go over the draft with him. I had had no breakfast. We went over the introduction. He said, "I just don't know what you are talking about." I struggled. I didn't think that I knew how to explain it even if I had been in a better condition. I re-arranged words again. Finally, Charney exclaimed, "Why didn't you say so in the first place!" We spent the entire day on the draft, and he very patiently pointed out to me not just the scientific implications I had missed, but also subtleties of writing so that scientific conclusions are not obscured by jargon or by the intricacies of the solution.

Charney taught me to question, to formulate problems, and to focus on the finding. More importantly he made me believe in myself, that I could do it, if I tried hard enough.

When I was a post-doc at Goddard Space Flight Center, my husband was a post-doc at Lamont. I had a very difficult time commuting between Washington and New York, and considered quitting science altogether. In one of Charney's visits to Goddard, he said, "Your work is only part of your life; you should never confuse the two." So he suggested that I should return to New York and learn chemistry, "since you are married to a chemist" —in particular chemistry of carbon and other tracers. Once I could teach him the chemistry, we could work on something together.

I prepared study notes on the carbon cycle for Charney. But alas, he never saw them.

I still write as though Charney will be reading my manuscripts and will be asking questions I cannot answer till he returns from his next trip.

Kerry A. Emanuel

"You don't want Charney as a thesis advisor," one of my fellow graduate students told me, "he expects you to be independent." That sounded good, though it took quite a while before I could arrange a meeting with the man. (They used to say that the difference between Charney and God is that, whereas God is everywhere, Charney is everywhere except M.I.T.) But that first meeting is indelibly imprinted in my memory. The broad grin with a little hint of naughtiness, and the overwhelming feeling that he really liked you. When he started to talk science, the rest of the world dropped far into the background. Phones went unanswered, appointments unheeded, meals postponed. And he would stand at the blackboard for what seemed like hours, chalk poised over an unfinished equation that just might not be right. He was his best and most engaging self when you got him into an argument, whether about Kelvin-Helmholtz instability or the Vietnam war. I once disagreed with him over some point having to do with the Howard semi-circle theorem. Charney rings up Howard: "Lou, Emanuel says so-and-so. Yeah, I thought so. Bye." End of argument.

The scientist of television and film is an abnormal person, socially ill at ease and probably rejected by his peers as a child. Part of Jule Charney's magic was that he made you feel that being a scientist was not just acceptable, it was wonderful; by his example, he freed us from the Hollywood stereotype. Jule loved life. At an Armenian restaurant in Los Angeles, he got up on the table and danced, to the cheers of the clientele, and the utter befuddlement of his former student. His greatest pleasures outside his work were the company of his friends, long walks in the Santa Monica mountains, and Schubert.

My last memory of Jule dates to December 1980, when he and his friend Pat visited me at my family home on the coast of Maine. The temperature was well below zero, a stiff wind was blowing, and Jule was recovering from a bout of chemotherapy. No matter; Jule insisted on venturing out into the cold for a group picture. I remember him, with that broad grin, snapping happily away. We later discovered that there was no film in the camera. It didn't matter.

CHARNEY'S LAST DECADE: DESERTIFICATION, MONSOONS, ITCZ, AND MULTIPLE EQUILIBRIA

Jagadish Shukla

The present volume is the second volume of a memorial project undertaken after Charney's death in June of 1981. The first volume "The Atmosphere—A Challenge" (edited by R.S. Lindzen, E.N. Lorenz and G.W. Platzman, and published by the American Meteorological Society in 1990) was centered on the transcript of a lengthy interview with Charney conducted by G. Platzman in which Charney discussed at length his own scientific activities. There were also brief contributions illustrating Charney's influential role in major facets of meteorology and oceanography, critical appreciations of a few of Charney's classic papers and reprints of five classic papers. The descriptor, 'classic,' was not applied lightly. By the time Charney came to MIT, he was already recognized as the world's leading dynamic meteorologist, and he was not yet 40 years old. Several of his papers were recognized as establishing the foundations for modern meteorology. This included his thesis at UCLA which occupied a complete issue of the Journal of Meteorology (now the Journal of the Atmospheric Sciences). For the next 20 years, Charney remained the intellectual leader of the field. He would be a leading influence in the major program of the period, GARP and its components, GATE and FGGE. He even chaired the NRC committee which, in 1979, produced the first NRC assessment of the possible role of CO_2 in global warming. However, through all of this, Charney's first love was always science itself. His primary ambition remained scientific achievement and leadership. Nothing makes this clearer than the many new directions he initiated in the last decade of his life.

What follows is based on my personal recollections and my conversations with some of Charney's collaborators. The four sections describe four major topics initiated by Charney in the period 1975-81. Some of these are also discussed by Charney in the preceding volume.

Dynamics of Deserts

Charney was quite fascinated by the barren beauty of the deserts and he particularly loved the Mojave Desert in California. He was invited by Perkeris to spend some time at the Weizman Institute in Israel, and he felt that he should work on something of interest to the Weizman Institute. In the early 1970s, he had been on some oceanographic expeditions in the Indian Ocean, and, during the return flight, he was fascinated to see some of the Arabian and African deserts. During the same time, the Sahel drought was at its peak.

He was not quite comfortable with the prevailing explanation of deserts, that they result from the descending branch of the tropical Hadley circulation, and he had conjectured

Revised summary of a contribution to the Charney memorial symposium at MIT, March 1983.

that the higher albedo of the desert surface could lead the desert to perpetuate itself. Charney thought that the albedo effect could be one of the primary reasons for the net radiative sink over the deserts that had been observed from the satellite data. (W. Bandeen of NASA, told me that one day his boss, W. Nordberg, told him to send some satellite-derived net radiation budget data to Charney who had hypothesized that there should be a net radiation loss over the deserts.) Since the albedo over the deserts is high, and since the ground temperature is hotter in the desert than in the surroundings, the atmosphere above the desert loses more radiative energy than it receives; therefore, it becomes a radiative sink. The consequence of this counter-intuitive process is that atmospheric descent is required to maintain thermal equilibrium, which in turn increases dryness, inhibits rainfall and cloudiness, and perpetuates deserts.

In the Symons Memorial Lecture, Charney presented the results of a simple linear model in which he calculated the dynamical circulation induced by a radiative perturbation over a desert, and he showed that radiative perturbations caused by high albedo will have a tendency to perpetuate dynamical descent over the desert. Since he considered a linear model, these effects could be linearly combined with the (linear) Hadley circulation to obtain the complete flow. In order to maintain thermal equilibrium, radiative cooling over the desert has to be compensated by importing heat from the boundaries, especially from the Intertropical Convergence Zone (ITCZ) on the south side of the Sahara desert. Since the desert is sufficiently far from the equator, the meridional gradients of temperature (due to differential radiative forcing) and pressure produce zonal velocities and zonal frictional forces which are balanced by Coriolis forces associated with meridional circulation. In Charney's model, the frictionally controlled sinking motion in the middle troposphere increases by a factor of two when the albedo is increased from 14 to 35 percent. He also advanced the hypothesis that the reason for very low rainfall during winter between the Mediterranean and the Libyan and Egyptian deserts could also be due to strong sinking caused by radiative cooling. The key point in these calculations was a demonstration of the fact that the radiative time constants were comparable to the advective time constants, and in the regions of weak advective effects, radiative perturbations can lead to frictionally controlled sinking.

In this simple model, the temperature at the southern boundary (near the ITCZ) was prescribed; however the radius of influence of any perturbation on the boundary was about 1000 km. He conjectured that changes in the albedo of desert margin regions can produce instabilities or metastabilities that might produce prolonged periods of drought (or floods) or might help maintain a perturbation. This suggestion was perhaps motivated by the ongoing Sahel drought.

He then asked what could cause the change of albedo in the desert regions, and he proposed that it could be anthropogenic. I learned from my conversation with J. Otterman of Israel that when he met Charney in Israel for dinner, Otterman took with him a satellite picture of the Sinai-Negev Desert region which showed a marked discontinuity across the fence between the Sinai and the Negev, which was erected in 1948 when Israel was born. No cattle were allowed to go to the fenced regions of the Negev, but they were roaming freely in the Sinai. When he came back from his sabbatical to Israel, Charney suggested to R. Jastrow and M. Halem at NASA that a General Circulation Model (GCM) sensitivity experiment be conducted to investigate the influence of an increase in albedo in the desert margin regions. He carried out several numerical experiments and the model results strongly supported the proposed hypothesis. However, a close examination of the model results showed that the reasons for the GCM results were quite different than had been proposed in the Symons lecture. In particular, Charney found out that the evaporation from the land surface, and therefore the soil moisture, is equally if not more important than albedo. I was asked to review and edit the manuscript by Charney, Quirk, Chow and Kornfield which was later published in JAS in 1977. Y. Mintz's reaction to this manuscript is worth noting. Mintz told me that he was always impressed with Charney's writing because he had such a powerful and clear exposition, but he found Charney to be an incomprehensible speaker. His speeches were sometimes incoherent and disjointed. Mintz felt that this albedo paper was the first paper that was written in the way Charney used to talk. The main result of this paper was that the increase in the albedo decreases the solar energy available to the ground, but it also decreases the cloudiness which increases the solar energy at the ground. The albedo effect on solar radiation could thus have been compensated by the cloudiness effect if it were not for the fact that the reduction in the cloudiness also reduces the long-wave radiation from the base of the cloud to the ground, and therefore the net radiative energy at the ground is reduced, causing less evaporation and less rainfall.

This work of Charney generated tremendous interest in the role of land surface processes in climate change and climate variability. Although the role of vegetation or irrigation on climate has always been of interest, Charney was the first one to give quantitative estimates of these effects and to point out important interactions between the land surface processes and the dynamical circulation.

Predictability of Monsoons

Charney was invited to visit India and deliver lectures at several research centers. He really wanted to visit India, but he felt that he really did not have any scientific results on monsoons worth a trip. He was tired of giving his desertification lecture. We started talking about the physics and dynamics of monsoons and especially recent work on the Indian monsoon. I told Charney about two of my recent results: one using the Geophysical Fluid Dynamics Laboratory (GFDL) model showing the effects of sea surface temperature (SST) over the Arabian Sea on Indian monsoon rainfall; and the other on the empirical relationship between Eurasian snow cover and Indian monsoon rainfall. Charney was especially intrigued by the GCM result, because F. Bretherton had recently told him that the National Center for Atmospheric Research (NCAR) model had no sensitivity to large SST changes. When I told Charney that we seemed to get large responses when we changed the boundary conditions in the tropics (Rowntree had shown large effects of the tropical SST using the GFDL model, and I had shown the effects of Arabian Sea SST and Eurasian snow cover), he became cheerful and excited and started pacing back and forth. He recalled that when he had done the albedo sensitivity experiments with the NASA model, he had also noticed little change in the Indian monsoon by perturbing the atmospheric initial condition. He had noticed that changes in the simulated July field were quite large for the midlatitudes, almost as large as observed in nature; however, the changes in the monsoon circulation were very small.

We began to put together some of these previous results so that he had something to present during his India trip if he chose to go. As we proceeded to do so, it became increasingly clear that we had a hypothesis that took care of Charney's impression based on the numerical integrations of the NASA model which had shown little change in the monsoon circulation with the fixed boundary conditions, and my observational and numerical results with the GFDL model that changes in the boundary conditions did produce changes in the monsoon circulation. We therefore put forward the hypothesis that the monsoon circulation is predictable, because its interannual variability is influenced largely by the boundary conditions which change slowly. Charney decided that he would go to India. We were concerned with the influence of the mid-latitude variability on the monsoon variability but Charney argued that the zero-wind line could prevent the propagation of mid-latitude influences. We were aware, especially from the observational studies of Indian monsoon, that on certain occasions, mid-latitude perturbations penetrate deep into the monsoon region and influence the monsoon circulation, but we considered these to be exceptions rather than the rule. (Retrospectively, I now believe that the influence of the mid-latitude disturbances on the Indian summer monsoon, especially its intraseasonal variability, could be larger than what we had assumed. Likewise, small changes in the monsoon rainfall in the NASA model were, at least in part, a model defect.)

This was not a complete study because the conclusions on the stability of the monsoon were based on one set of experiments with one model, and the conclusions about the influence of the boundary conditions were based on another set of experiments with another model, and we basically patched them together. One could argue that model deficiencies of either of the models could account for part or all of the discrepancies between the simulated variability and the observed variability. More systematic numerical experiments have since been carried out and it is now well established that interannual variability of the tropical circulation (but not necessarily the regional Indian rainfall) is significantly influenced by the interannual variability of the boundary conditions, which gives hope for the predictability of time averages in the tropics, and in the extratropics, through the influence of the tropics on mid-latitude circulation which is otherwise unpredictable. This work was helpful in providing a scientific basis and justification for the international program entitled Tropical Ocean Global Atmosphere (TOGA).

Inter-Tropical Convergence Zone (ITCZ)

During the last few years, Charney was once again interested in symmetric coupled ocean-atmosphere models to understand the location of the ITCZ. He was quite aware that a symmetric model is not an adequate tool to describe the tropical circulation; however, he thought that some of the regional features could be fruitfully studied with a symmetric model. For example, what determines the position of the ITCZ, why is the latitudinal position of the ITCZ different for different oceans, and what determines its position over the land? He had earlier proposed that over the oceans, the ITCZ cannot form over the equator because of the inefficiency of the Ekman pumping, and cannot be too far away from the equator because of the requirement of conditional instability. He had the intuitive feeling that the mechanisms for location of the ITCZ over the ocean are very different from those over the land and the location over land is perhaps largely determined by the declination of the sun. So, although he had done some earlier work on the ITCZ using a simple model, he wanted to study the problem with a symmetric version of a GCM. E. Schneider and Charney had made use of the symmetric version of the NASA model to study the ITCZ. That study was not conclusive, because it was very difficult to achieve steady states in the symmetric version of the full GCM. Subsequently, Charney and E. Kalnay developed a symmetric ocean model, which Kalnay did rather easily by modifying her model of the Venus atmosphere. Charney wanted to test his earlier proposition that the location of the ITCZ over the ocean is determined by the interaction between the ocean and atmosphere. The ITCZ is observed to be over the warm SST, but is it likely that the warm SST is away from the equator because the ITCZ is away from the equator? If the ITCZ is displaced away from the equator, the associated surface easterlies at the equator will produce upwelling and cool the ocean surface near the equator, thus preventing the ITCZ from returning to the equator. On the other hand, the convergence of higher angular momentum air from the equator and lower angular momentum air from further north or south will produce cyclonic shear (vorticity) and therefore enhance the frictional convergence.

To our regret, Kalnay and I both had gotten busy with other projects and this work never got published. It was later published as a NASA technical memorandum by Charney, Kalnay, Schneider and Shukla in 1988. I use the word regret simply because the lack of progress on this project made Charney get really upset—one of the very few times I observed him that way. However, the construction of a symmetric version of an atmospheric GCM was later carried out, perhaps a little more successfully, by B. Goswami, who was another post-doc of Charney.

Multiple Equilibria

It seems that some notion of multiple equilibria was on Charney's mind for many years. Charney was very much influenced by Lorenz's work on the mechanics of vacillation. When Kalnay returned to MIT, one of the things Charney was working on with her was a way to calculate the unstable limit cycles for Lorenz's 28 variable model. He was seeking a general theory of climate with the hypothesis that it is the weighted sum of several unstable stationary states where the weighting factor could be the residence time in each state and the residence time was a measure of the instability of that state. He had met with Scarf, an economist from Yale, and he was interested in his method of finding fixed points for a simple atmospheric model. Although Kalnay did develop a numerical scheme that was capable of getting unsteady states, they could not find unstable limit cycles. Charney had also attended a workshop on the computation of equilibria and stability regions at the International Institute for Applied Systems Analysis in Austria (July 21, 1975), where he had suggested that the average values and statistical moments derived from the unstable limit cycles will constitute a good approximation to those of the actual turbulent flow.

When D. Straus came to MIT in 1976, Charney wanted him to find a system with even fewer degrees of freedom than that in Lorenz's paper on the mechanisms of vacillation, but which exhibited multiple vacillation and aperiodicity to understand if the statistics of an aperiodic system are related to the statistics of unstable limits cycles and the nature of transitions. His ultimate motive, perhaps, was to explain the climate of the atmosphere as a weighted statistic of unstable limit cycles. They did not find a simpler system which had vacillation and was also aperiodic.

The Charney and deVore paper on multiple equilibria grew out of a seminar course while Charney was on sabbatical at UCLA in 1978. He was, at that time, interested in long-period large-scale motions in the atmosphere, among other things. They reviewed several observational papers in the seminar. One of the questions they were especially concerned with was: what produces the long period transient planetary Rossby waves? Charney could not accept them to be free waves because they could not exist for such a long period with any reasonable dissipation. They reviewed a paper by Hirota who had shown that the interaction of fluctuating zonal winds with topography can produce propagating planetary waves that would look like Rossby waves. Charney extended the idea by posing the question: what is the cause of the fluctuating zonal wind? He proposed that zonal flow could fluctuate due to its interaction with topographically forced disturbances. They decided to examine the properties of a highly truncated spectral model with topography, dissipation, and unspecified momentum driving.

According to deVore's recollection, he was asked by Charney to numerically integrate a low order spectral model with mountains, dissipation, and momentum driving to examine its time behavior. He made integrations with several random initial conditions, and to their surprise, they found that sometimes the waves stopped moving, as if they had reached some steady state. They were quite puzzled as to why the numerical simulations were stuck and Charney decided to calculate the steady states analytically, and when he found that there was more than one steady state solution, he quickly recognized what was happening. Kalnay told me that, as early as 1974, Charney used to comment that there must be multiple equilibria in the atmosphere (he knew that they exist in many other systems). I remember some of my own phone conversations with Charney when he was doing this work at UCLA. He would say things like, "You would not believe what is going on out here, it is simply out of this world, we have got multiple equilibria for the mid-latitude atmosphere." Without knowing what he had actually done, I mentioned the simple climate models in which we get different equilibrium climates, and he seemed to get a bit upset on the phone as if I had downgraded his work, and he repeated somewhat angrily that he had got multiple equilibria in a system which is a reasonably good approximation to atmospheric dynamics, with mountains, dissipation, forcing and waves and their interaction.

Charney and deVore had found that there were three equilibria, two of which—the super-resonant and subresonant solutions—were stable and the third intermediate one was unstable. The two stable solutions were found to be similar to the mean climatology and blocking situations, respectively. The question of blocking and the relevance of their work to blocking arose much later in the seminar. They were not trying to explain blocking, but they found something which they recognized would be shown to be relevant to the blocking phenomenon.

Charney and Straus extended the work of Charney and deVore for a two-layer baroclinic model and the results were numerous and more complicated. For large values of forcing they could find up to five equilibria. They examined the stability properties of the equilibria and found them to be largely unstable. They also showed a clear possibility that westward propagating planetary waves observed in the atmosphere could be due to the instability of orographically forced waves. Charney, Shukla and Mo extended the work of Charney and deVore using a suggestion from Hart to use very slow variation in the y-direction compared to x-direction, which gives rise to linear equations for perturbations and makes it possible to take arbitrary topography in the x-direction. It was found that more than half of the blocks observed in data were quite comparable to one of the stable (or quasi-stable) equilibria of the model. Part II of Charney, Shukla and Mo's paper, using a two-layer baroclinic model with arbitrary topography and arbitrary zonally asymmetric heating, was never written up although all calculations had been completed. This was simply because Charney became uneasy about prescribing the zonally asymmetric heating. He wanted the heating to be parameterized in terms of the motion field.

During the last few months, Charney was very interested in the question of regional blocking. He was quite excited about his new idea of variable zonal driving as being responsible for the blocking to be regional and highly localized. Charney and Mo had submitted an abstract of a paper that Charney was to present in Europe. The title of the paper was: "Localized blocking accounted for by slow variations of the zonal driving." Alas, he did not live long enough to present the paper.

To the present, the topic of multiple equilibria continues to generate substantial interest and research activity.