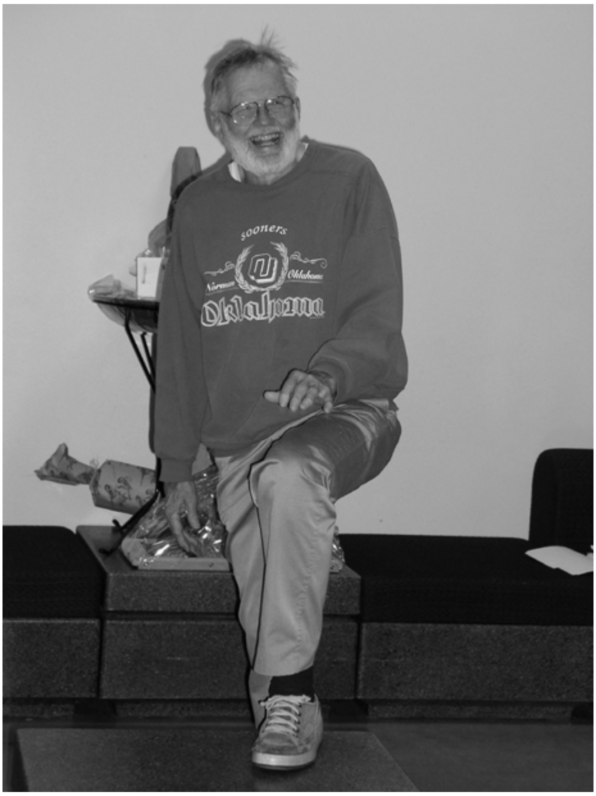


Douglas K. Lilly: a biography

Katharine M. Kanak

with recollections from
K. Bryan, J. Deardorff, K. Droegemeier, J. Kimpel, P. Lamb,
D. Lenschow, and J. Smagorinsky

Douglas (Doug) Lilly was born on June 16, 1929 in San Francisco, California. He grew up on the San Francisco peninsula where, as he describes it, "there is not much weather!" He states that he was interested in weather and the atmosphere starting from his years in high school in California. The predominant cloud features there were stratus decks that would come into the bay area, stay for a while, and eventually break up. He used to borrow the family car to drive up hills to observe these stratus decks. One might say this was his high school hobby.

Doug attended Stanford University and completed a Bachelor of Science degree in Physics in 1950. At Stanford, he was a member of the rowing crew and of the Naval Reserve Officers Training Corps (ROTC) program. From 1950 to 1953, during the Korean War, he was on active duty in the Navy. He was stationed for a while in Hawaii, and then later on a minesweeper off the coast of Korea.

After completing his military service Doug decided to pursue a graduate degree in Meteorology. It was early in his graduate studies at Florida State University (FSU) that Doug first met Judith (Judy) Anne Schuh, who would later become his wife. She was pursuing a degree in Education with a minor in German. They dated for one year and married on August 12, 1954 (the year Judy graduated) in her home town Jacksonville, Florida. Their first child, Kathryn Elizabeth, was born July 19, 1955 in Tallahassee, Florida. In this same year, Doug completed his Master of Science degree in Meteorology at FSU. During their time living in Florida, and driving back and forth between Jacksonville and Tallahassee, Doug was fascinated with the tropical convection and spent a good deal of the rides with his head out of the car window!

In 1956, Doug took a job with Radio Free Europe and the family moved to Munich, Germany. His responsibilities there included prediction of wind directions and weather conditions for the purposes of launching balloons with news pamphlets into Eastern Europe during the early years of the cold war. This was a nice opportunity for Judy to perfect her German in which she had earned a minor in college. Doug also learned German there and later also some French. During this year they lived in the apartment of a retired opera singer.

In the summer of 1957, the family returned to Redwood City, California, where their second child, Donald Roger, was born on July 31, 1957. The following year, they returned to Tallahassee for one more year in order for Doug to complete his Ph.D. in 1958 with Seymour L. Hess as his major advisor. The title of his dissertation was "On the Theory of Disturbances in a Conditionally Unstable Atmosphere." This novel theoretical work was later published under the same title in 1960 in *Monthly Weather Review* (Lilly, 1960).

After completing his Ph.D., Doug took a position as a Research Meteorologist at the US Weather Bureau's General Circulation Laboratory (GCL), a division of the National Oceanic and Atmospheric Administration (NOAA) in Washington, DC (predecessor to the NOAA Geophysical Fluid Dynamics Laboratory, GFDL). In Washington, DC, Doug and Judy's third child, Carol Susan, was born on August 18, 1959.

It was at GCL that Doug met and worked with Joseph Smagorinsky. During that time, Doug contributed to some of the very earliest efforts towards numerical simulation of atmospheric convection. He developed a series of numerical techniques and methods that are still used today. He also worked on laboratory studies of vortices and convection, and, of course, on the theory of atmospheric convection. To this day, Doug considers Joe Smagorinsky as his most esteemed mentor and respected friend Joe writes:

I first met Doug Lilly in the late 1950s. I was a very young laboratory director looking for talent. Bob Simpson and Werner Baum talked to me about a brilliant, though brash, graduate student at Florida State University who had not yet finished his thesis work. Doug Lilly's interests were in tropical meteorology and convection. This coincided with my intentions to begin a modeling and simulation activity at the GCL.

I went to Florida to talk to Doug. It was one of the hottest spells of the year and I can remember my introduction, the following morning, to grits. The scientific precedents to my objectives were the work of Joanne Simpson of Woods Hole Oceanographic Institution and Georg Witt of the University of Stockholm. The modeling of atmospheric convection at that time was virtually non-existent, as it was in other domains of geophysical fluid dynamics, such as climate, oceanography, hurricanes, mesoscale meteorology, and extended-range weather prediction.

Doug agreed to take the job and came to GCL in 1959. He and Syukuru (Suki) Manabe arrived within a few days of each other: two nascent superstars. Doug started by modeling the boundary layer in today's Large Eddy Simulation (LES) sense and then derived appropriate parameterizations – something that hasn't yet been done properly 40 years later. But Doug did invent the essence of LES along the way!

Considering that Doug did his undergraduate work at Stanford, it was not too surprising that he got a hankering to return to the West. Doug applied for a sabbatical to NCAR. The country, replete with horses, was irresistible to Doug and his family. This was the beginning of the end of the GCL phase of his career. Doug decided to make his stay at Boulder more permanent. We missed his presence at GCL.

I can't help thinking "this lanky kid came a long way, and some of it started here."

(*Joseph Smagorinsky*, GFDL, Princeton)

Another colleague of Doug's from his time at the GCL, Kirk Bryan, remembers:

I was one of a very small group of scientists who worked in GCL from 1950 to 1965. For a brief period, Doug Lilly, Syukuru Manabe, and I all shared the same office. I remember those years as one of the most intellectually stimulating periods of my life. At that time Doug was working on the simulation of convection, but his interests extended far beyond that. He had completely mastered what was then known about numerical methods and worked out many original approaches to numerical simulation on his own. What little I have learned about the subject is through him.

Lengthy scientific discussions during the day left little time for actual work at the office. That was done after hours. Almost every day Doug would come in the office, looking somewhat haggard, and we would spend two or three hours at the blackboard going over the problem he had "solved" or "almost solved" the previous night.

We would spend hours dissecting each lecture given at the Laboratory. Doug was very quick to penetrate to the essential ideas. This was not easy to do in those days, because geophysical fluid dynamics was still in a very rudimentary state. This is what has made him an inspiring teacher and mentor to many coming into the field. In a sense, I consider myself one of his first students.

(*Kirk Bryan*, GFDL, Princeton)

In 1964, Doug took a position as a Senior Scientist at the National Center for Atmospheric Research (NCAR) in Boulder, Colorado. Not long after the family moved to Boulder, construction was started on the NCAR Mesa Laboratory, which was completed in 1966. Doug and his colleagues referred to the Mesa Laboratory as "Mount Olympus: Home of the Gods," which seemed appropriate due to the building's impressive nature.

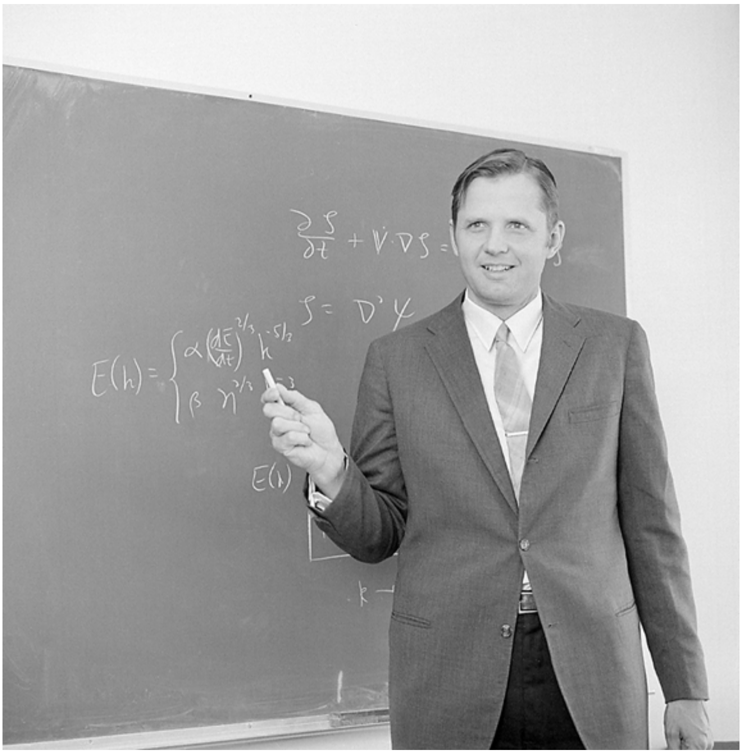
In 1966, the Lilly family moved to a home east of Boulder. A few years later they had an anemometer mounted outside the house with the recorder indoors in order to measure the wind speeds at the house. Doug's daughter Carol recounts that the instrument had its own graph and red ink, and that three kids loved to watch it recording wind speeds during the severe wind storms, which were common in the lee of the Rocky Mountains.

At some point, the kids became aware that their father was becoming renowned as a meteorologist. He would appear on television occasionally, but he was always "low key," just as he is today – modest about his achievements. At one time the interviewer asked Doug what the wind was going to do this year, and his simple chuckling response was: "Blow!"

Doug loved his work and he worked all the time. After coming home from the office, he would eat dinner and then sit in an easy chair, writing on his lap from about eight until midnight. Even today, this is his preferred method of working. Anyone who knows him can visualize him with his simple thin spiral notebook (nothing fancy for elegant equations!) and a pencil. His constant working was a source of amusement to the children and their friends. They would come in the house, all pass by him and say "Hello, Dr. Lilly" one after another, and then giggle upstairs because he never even looked up! Carol says he was also a wonderful role model in finding work that he loved so much.

Doug's career is distinguished by his broad range of interests and abilities, but it might be said that stratocumulus clouds were, and still are, his favorite topic. While at NCAR in 1968, he published a seminal paper on stratocumulus clouds in the *Quarterly Journal of the Royal Meteorological Society* (Lilly, 1968). It is remarkable that much later, in the 2000s, Doug has again returned to stratocumulus (Lilly, 2002a, b; Stevens *et al.*, 2003) and is writing a book on the topic.

Whilst at NCAR, Doug also began to conduct laboratory and observational studies with Jim Deardorff and Don Lenschow. He did work on turbulence in the atmospheric boundary layer and in the stratosphere, and also became



Doug Lilly in the mid 1960s at NCAR speaking about two-dimensional turbulence.

interested in predictability and numerical simulation of turbulence. Jim Deardorff writes:

Around the time that Doug joined NCAR, in the early 1960s, I was getting started in the numerical simulation of two-dimensional thermal convection. Doug showed me how to finite-difference the vorticity and thermal-diffusion equations so as to conserve kinetic energy and temperature variance in the absence of sources and sinks, and helped direct me to references on the subject, which had already started to appear in the literature. His papers in *Tellus* (Lilly, 1962) and in *Journal of the Atmospheric Sciences* (Lilly, 1964) were of great help to me.

After I started to realize the advantages of utilizing the equations of motion directly, along with the thermal-diffusion equation, it was Doug who showed me how to use a "staggered" grid system for the velocity components in a manner that would conserve kinetic energy and avoid non-linear computational instability. We in Doug's group (I think NCAR had developed a small-scale group by then which he had agreed to head), could count on him to keep up on the latest developments in our fields of interest, and he had acquired an understanding of Arakawa's (1963) pioneering work along these lines at an early stage.

In the 1960s, Glen Willis and I were busy pursuing turbulent thermal convection in the laboratory, in which Doug was quite interested, and we were making frequent observations of the rate of increase in the height of the turbulent layer through the process of penetrative convection and entrainment. Doug already had a decent understanding of this process, through his studies of the work of Turner and others, and could explain it to us along with the expectation that the magnitude of the downward heat flux in the entrainment region would be some fraction of the upward heat flux at the surface. His input here was too valuable for him to avoid being a co-author of our paper on laboratory investigation of non-steady penetrative convection (Deardorff *et al*., 1969).

After computing power had increased to the point where it was conceivable to study turbulence in three dimensions, in the late 1960s, the problem arose of how to simulate the dissipation of turbulent kinetic energy cascaded to scales too small to represent explicitly. Doug was very well acquainted with J. Smagorinsky's work on this subject, and was very helpful in advising me on how to apply Smagorinsky's method to small-scale turbulence. Doug had already done his own research on this problem, and so could recommend a coefficient of proportionality between the magnitude of the subgrid-scale eddy coefficient and the resolvable strain rate.

By the 1970s, my interests had turned towards the atmospheric boundary layer and its turbulence. In so doing, it soon became clear that results from numerical integrations would be of greater value if the physical quantities involved were made non-dimensional. Here Doug's input was quite helpful to me for pointing out the use of the boundary-layer height to scale all lengths, even though that height could increase with time within the calculations. His interest in, and knowledge of, Ekman instability alerted me on what to look for in the roll-like structures that emerged from the numerical calculations for neutral and slightly unstable planetary boundary layers.

In the mid 1970s my interests turned towards the stratocumulus-topped planetary boundary layer, partly as a result of Doug's earlier study on this topic (Lilly, 1968). It had taken quite a while for his work on this to sink into my consciousness, but with Doug on hand to explain, from time to time, how radiative cooling from the tops of stratocumulus clouds helps drive the turbulence, I was able to make my own contribution to this topic in 1976 (Deardorff, 1976).

Throughout the time period that Doug and I were both at NCAR, our group frequently benefited from the visiting scientists that he invited there to give a talk and discuss mutual interests. Needless to say, Doug's influence within various fields of atmospheric science has continued to be contributory in all respects.

(*James Deardorff*, Oregon State University)

Doug's observational work of that time was focused on mountain waves and downslope windstorms. His paper with Joe Klemp on wave-induced downslope winds (Klemp and Lilly, 1975) was awarded an "Outstanding Publication Award" from NCAR. Don Lenschow recalls:

I first met Doug when I applied for a position at NCAR as a student nearing graduation, in 1965. He had heard from Jim Telford, who was then at Commonwealth Industrial and Research Organisation (CSIRO) in Australia, but making plans to move to the US, that it now seemed possible for the meteorological community to take advantage of new technology that would allow direct measurements of mesoscale air motions from aircraft. He saw in my 'résumé' that I had worked on the development of an air-motion sensing system on an aircraft, and hired me to help bring this technology to NCAR. He supported the development of INS-based (inertial navigation systems) air-motion sensing platforms on aircraft at NCAR, well before others in the community recognized their capabilities and applications (e.g. Lilly and Lenschow, 1974).

He also recognized limitations in other observational capabilities at that time that needed to be addressed in order to make definitive tests of hypotheses for explaining the behavior of the atmosphere. He encouraged the development of new instruments, for example, to measure temperature in clouds, and high-rate temperature and humidity for eddy fluxes in the boundary layer. He worked with NCAR and NOAA scientists on the development and deployment of Doppler radars for studying motions in convective clouds. He experimented with constant-level balloons for following air motions over the Rocky Mountains. He worked on deploying chaff from aircraft for use as radar targets to study air motions. He had a keen appreciation of the importance of technological developments in providing the tools needed for model verification and improvement, and helped to implement them at NCAR. His support led to systems that were at the cutting edge of the field and to many important research results from observational programs.

The first field program that I recall in which I participated with Doug was a marine stratocumulus study. In the late 1960s, after completing his seminal cloud-topped mixed-layer paper, he carried out a small-scale Queen-Air based field program flying out of Coronado, California to take measurements in the marine stratocumulus. Although I do not recall any published results from this study on stratocumulus, it did provide the basis for later observational studies, in terms of gaining an understanding of what this regime looked like, and how to deploy an airplane instrumented for turbulence measurements (and also ozone). Later, he worked with Wayne Schubert in deploying the NCAR Electra in the first large-aircraft deployment in the marine stratus regime. That program led to several publications, for example by Wakefield and Schubert (1976), and Brost *et al*. (1982a, b), that helped to understand this regime.

Doug had a long-term interest in studying flow over mountains – in particular the weather regimes over the Front Range that led to the wintertime Boulder wind storms. He carried out a series of airborne observational studies, including the 1970 Colorado Lee Wave Observational Study (Lilly *et al*., 1971), and played a central role in organizing and implementing the Wave Momentum Flux Experiment (WAMFLEX). Some of the events were quite remarkable and intense (e.g. Lilly and Zipser, 1972). Over the years, he employed the NCAR Sabreliner, Buffalo, and Queen Air aircraft, as well as a WB57-F, a high-altitude military aircraft. These studies took advantage of the research platforms that he played a role in developing at NCAR and provided the observational basis for many modeling studies of mountain waves and downslope winds.

(*Don Lenschow*, NCAR, Boulder)

In the early 1980s, Doug became interested in severe supercell convective storms. In this area, his main collaborators were Joseph Klemp, Rich Rotunno, and Tzvi Gal-Chen. Along with Tzvi Gal-Chen, Doug served as editor for a book on mesoscale meteorology (Lilly and Gal-Chen, 1983).

About this same time, Doug started thinking it might be time for a change in career. In 1982, Doug and Judy visited the School of Meteorology of the University of Oklahoma (OU) and decided to move to Norman to experience university life. They did so along with Doug's long-time friend and collaborator, Tzvi Gal-Chen, and his family. I recollect Doug's mentoring during my years as a student at OU:

In 1984 I was an undergraduate and had the privilege of being in Doug's first Physical Meteorology class. I always enjoyed and appreciated his comments on my papers. He had an ability to look beyond standard measures to find the unique strengths of each of his students.

I graduated and went to the University of Wisconsin–Madison for graduate school. However, I did return to Oklahoma for my Ph.D. work and to my great honor, Dr. Lilly accepted me as his graduate student. He was an outstanding mentor, and he was never condescending. He challenged me to reach for more from myself. Sometimes he would ask me questions quite casually about our research and I would answer (smiling to myself) knowing that he knew the answer and was just testing me. Doug never made an ordeal of evaluating me.

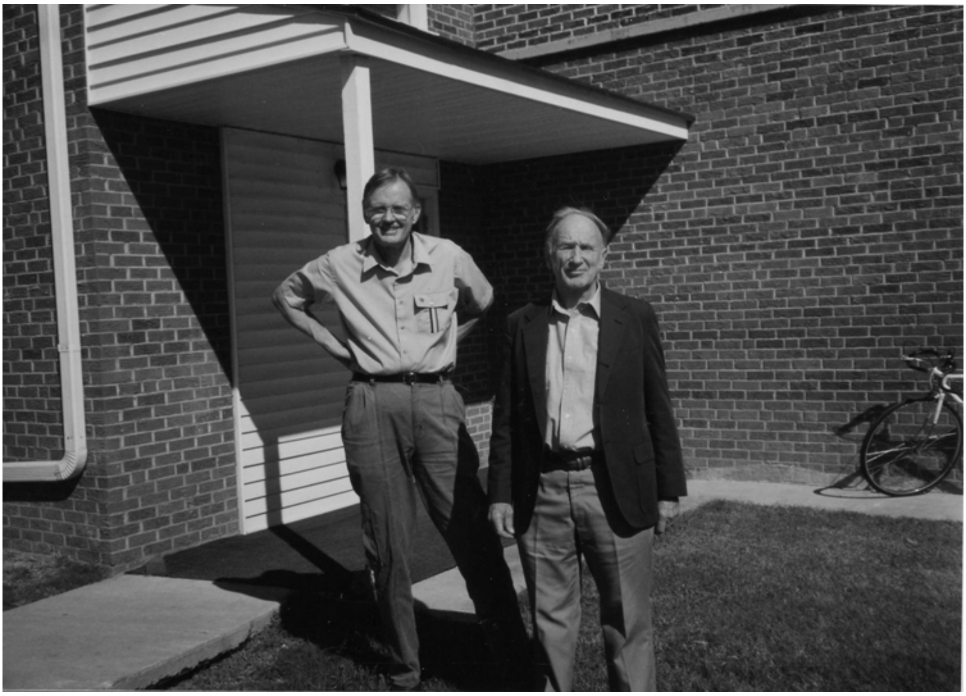
He taught me to be a scientist by example. I watched him think and watched carefully how he approached problems. I saw that he always attended seminars and spent significant time in the library reading the latest articles. He has an encyclopedic memory, an insatiable curiosity and never ever accepts anything as true just because someone says so.

He never pushed deadlines or hurried the work. In fact, he was meticulous and deliberate in research. This was very important; the most accurate answers in science cannot be forced to reveal themselves and he recognized this. He also directed me to uphold the highest possible standards of objectivity in science. He taught me how to write and to choose my words carefully. Through his guidance, I learned patience, persistence and determination. Articles we co-authored were never submitted until they were in the finest form we could achieve. I sometimes became impatient with this, but I later understood, when the review process was smooth, why he insisted on continuing to refine the manuscript prior to submittal. He used to call those peer reviews "softballs."

One other time, when I did not initially understand his reasoning, was when he wanted me to publish my dissertation paper before completing the dissertation. I did not fully realize at the time that I had stumbled onto something new and he helped me to get it published quickly. I look back over my doctoral research mentorship and I see he almost always was two–five (or more) steps ahead of me and I never knew it. In fact, Doug did not look at things in "steps," or in a linear sequential manner at all. I believe that he viewed things in full pictures and saw the full three dimensions of everything. He was excellent at gentle steering. His approach was to lead a student half-way across the river and wait on the other side for him/her to cross the other half, perhaps with a little coaching here and there. His scientific philosophy was that no scientific topic or question is without merit. He always supported "science for science sake" and topics without direct applications, that is, fundamental basic research. Doug was not one to hand out compliments easily. Therefore, when one received one, it really meant something and he was genuinely sincere.

(*Katharine Kanak*, CIMMS, Norman)

In 1986, Doug was awarded one of OU's highest honors, the George Lynn Cross Research Professorship. From 1992 to 1995, he held the Robert Lowry Endowed Chair in Meteorology at OU, which was at the time the first endowed chair in Meteorology/Atmospheric Science in the United States.



Doug Lilly (left) together with Edward Lorenz in front of the old CIMMS building in Norman.

In 1987, Doug became the Director of the OU Cooperative Institute for Mesoscale Meteorological Studies (CIMMS). During that time he published a series of papers on the numerical simulation and predictability of thunderstorms. He worked on the novel applications of helicity concepts to modeling of severe thunderstorms and on cirrus outflow dynamics. He also maintained his work in atmospheric turbulence and two-dimensional turbulence as applied to atmospheric mesoscale flow motions. Additionally, he was involved in laboratory work on simulation of atmospheric vortices. Current Director of CIMMS, Pete Lamb recalls:

From my arrival at OU in August 1991 until Doug stepped down as CAPS Director in the summer of 1994, I had the good fortune to occupy an office relatively adjacent to Doug's office. The "outer office" between us housed support staff, some of whom we shared for a couple of years. So, I was fortunate to interact with Doug on an almost daily basis for three years. On two or three days each week, Doug would appear in my office around 5 p.m. to reflect for 10–20 minutes on the various states of CAPS, CIMMS, Meteorology in general, OU, NOAA, the American Meteorological Society (AMS), the United States, . . . , and the World. I am sure Doug's initial visits had two motivations – to make me feel welcome and (because of his interest in other people) to see what made me "tick." Because of Doug's encyclopedic interests, anything could be discussed during these visits. When Doug stopped by my office, it was part of a "one–two" Gal-Chen/Lilly "combination," since

Tzvi invariably stopped by about 45 minutes earlier for essentially the same purpose. These late-afternoon visits were entirely natural because high among Doug's and Tzvi's most important pastimes was a strong interest in their colleagues' well-being and work, free-ranging thought, conversation, and debate. Doug's relinquishing of the CAPS Directorship led to him moving to another office several floors up in our building, after which I saw much less of him, unfortunately.

Doug was always very supportive of his current colleagues and keenly assessed the accomplishments and potential of possible future colleagues. Doug's mode of operation during faculty searches was a model for younger colleagues. Looking over applicants' Curriculum Vitae was just a start, to be followed by reading of some of their papers, a trip to the main University Library to consult the Science Citation Index to assess the impact of the papers, asking questions at candidates' seminars even for presentations outside of his areas of specialization, and often interviewing them informally, like when he was driving them around Norman and vicinity. Several of us learned the value (and weaknesses) of the Science Citation Index from Doug.

In 1993, Doug received the prestigious Symons Gold Medal from the Royal Meteorological Society (RMS) at their end-of-year meeting in London. Before he left for London there were ruminations about what he would say in the short presentation he had been asked to give to the "Meteorological Dining Club" at a dinner following the RMS meeting. The Club, a group of especially accomplished RMS members, has a strict "anything but religion and politics" rule for these short presentations. Doug chose to speak on the development of scientific computing during his career, and when he returned he ventured that it had been well received.

An especially revealing incident occurred when he first came into the office after that trip. When I happened to cross paths with him in our outer office very soon after his arrival that day, I inquired somewhat cavalierly "well Doug, where is the medal?" Much to my surprise, he promptly obliged by producing it from his pants' pocket. Carrying it around there was not a trivial exercise, given that the medal was neither small nor light. Clearly, Doug was appreciative of, and comfortable with, the recognition signified by the medal, and suspected that his office staff and scientific colleagues would like to see it. Indeed we did!

(*Peter Lamb*, CIMMS, University of Oklahoma, Norman)

Doug became Director of the OU Center for the Analysis and Prediction of Storms (CAPS) in 1989. Along with Kelvin Droegemeier, Doug wrote one of 11 out of 330 proposals for a Science and Technology Center that was funded by the National Science Foundation. During his time at CAPS (1989–94), he was involved in many studies of severe storms and techniques to improve their simulation, including four-dimensional data assimilation and the impacts of convective storm helicity on its predictability. Kelvin Droegemeier writes:

I recall Doug Lilly casually walking into my office during spring, 1988, carrying a four-page request for proposals issued a few days earlier by the National Science Foundation. Describing it as perhaps the best such solicitation he had ever seen, Doug hinted that we should pursue the opportunity. In Doug's classic style, he never organized any sort of formal meeting regarding the NSF solicitation, but rather began writing a vision document, that he shared with me in hard copy, on retrieving the unobserved components of the radial