about all the lines of evidence, so I quickly became one of the best informed experts around on continental drift.

It was a fascinating time to be researching the subject. In 1962, Harry Hess had published his model comparing ocean floors to conveyor belts spreading to either side of oceanic ridges; in 1963, Fred Vine and Drummond Matthews had described symmetrical patterns of magnetized stripes on ridge flanks; and by 1965, J. Tuzo Wilson had envisioned transform faults. The scientific journals were alive with new ideas and with arguments for and against drift.

In my seminar, I tried to present fair arguments for both sides of the question, but members of the audience detected a residual antitrdrift bias. That autumn, however, while I was writing the text for publication, several new and persuasive lines of evidence appeared. Among them, Patrick Hurley and his colleagues issued a map showing that if South America and Africa lay side by side, as shown on the computer reconstruction by Bullard, Everett, and Smith, a contact between rock provinces in Brazil would line up with one between formations of similar age and petrologic composition in Ghana. Alan Cox, Richard Doell, and Brent Dalrymple established the timing, by the K/Ar dating method, of nine pole reversals within the past four million years, and Fred Vine described magnetic stripes as frozen tape recordings of spreading oceanic ridges. By December 1966, I was a drifter.

As Robin has pointed out, my first paper led to others, and then one morning, the director of Smithsonian Institution Press walked into my office and asked for a book on continental drift. He said to make it technically correct but readable for students and interested nonexperts; he said to make it discursive. I looked up "discursive" and found the preferred definition to be a synonym for rambling—moving from topic to topic without order. I hope I made the book readable; I hope I did not make it, in that sense, discursive.

My "Continental drift" is a history book, and I enjoyed the research and the writing so much that after its publication, I began looking into the history of ideas on other subjects. I am particularly interested in the radical revolution in geologic thought that is implicit in our new awareness of the primacy of meteorite impact on the Earth, Moon, and other bodies. The very idea of impact as an important geologic process, modifying the Earth by random, externally generated events of great violence, turns inside out the old Huttonian-Lyellian views of gradual change by natural processes intrinsic to the globe. I presented papers on this subject at the Lyell Symposium of 1975, at the International Geological Congresses in Sydney in 1976 and in Paris in 1980, and at several other meetings. I incorporated some of this material into the article Robin referred to entitled "Meteorites, the Moon, and the history of geology" in the Journal of Geological Education. I am currently finishing a history of a meteorite fall that occurred in Alsace in 1492 and planning a revised edition of my long out-of-print book on continental drift.

So, in response to your final comment, Robin, I can assure you that I certainly shall publish more on the history of geology. I have come to love it. One day, not long ago, I looked up into the eternal gloom of level D, three stories below the ground in the vastness of Harvard's Widener Library and asked myself, very curiously, "Was Marty right? Am I happier doing history than geology?" The answer was a swift and unequivocal "No! I'm happiest because I did not have to choose!" Somehow, the two roads that diverged, in a yellow wood, early in my career have merged again; and I have been immensely fortunate in being able to combine a consuming interest in geology with one in history. I did not fully realize how fortunate, until I received notice of this honor you bestowed on me today.

Presentation of the O. E. Meinzer Award to
T. N. Narasimhan

CITATION BY S. P. NEUMAN

It is an honor and a pleasure to cite our colleague, T. N. Narasimhan, a personal friend of long standing, as the 1986 recipient of the Oscar E. Meinzer Award in hydrogeology. Nari (as he is affectionately called by many) was born in Madras, India, in 1935. After receiving his B.Sc., in geology with honors from the University of Madras in 1956, he joined the Geological Survey of India, for which he worked till 1969. During these first 13 years of his professional career, Nari served primarily as a field hydrogeologist in charge of aquifer testing for water supply to rural and urban communities, including the City of Madras. He was expected to do not much more than apply routine textbook methods to this task. A burning intellectual curiosity drove Nari away from professional stagnation, however, to which it was all too easy to descend in the climate that surrounded him, toward a persistent search for self-enlightenment and the discovery of exciting new ideas. As is often the case when such a search is coupled with true talent, Nari soon found himself treading the path of scientific innovation. His first published contribution was the development of "An alternative method for computing aquifer constants using the Theis nonequilibrium formula," which appeared in the Journal of the Indian Geophysical Union in 1966. His idea was to use "A ratio method as an alternative to the curve matching procedure for solving certain types of problems" (Current Science, 1968) such as "determining characteristics of ideal, leaky, and bounded aquifers" (Internat. Assoc. Sci. Hydrology Bull., 1967). Nari was among the first practitioners to apply the Papadopulos-
Cooper wellbore storage theory to large-diameter wells in India. He commented on the original paper by these authors (Water Resources Research, 1968) and further contributed to this topic through several additional articles and discussions. For his early work on well hydraulics and pumping-test analyses, Nari had been commended by no other than the late Mahdi Hantush. Additional early recognition came in the form of an invitation to prepare a guest editorial on “Methods of analysis of pumping test data,” which was carried by Groundwater in its second issue of 1969.

Armed with this impressive record, a warm letter of recommendation from Hantush, and the promise of a research assistantship from the University of California at Berkeley, T. N. Narasimhan came with his family to the United States in 1970 to pursue graduate studies under the guidance of Paul A. Witherspoon. In 1971, he completed his M.S. and, in 1975, his Ph.D., both in engineering science within the Department of Civil Engineering. Nari's doctoral work involved the integration of concepts from porous-media theory, soil mechanics, and soil physics into a comprehensive computer tool for the study of transient fluid and soil behavior under saturated and unsaturated conditions in three-dimensional space. Much of what we know today about such behavior is an outgrowth of Nari's work. His computer code, Trust, is widely used by researchers and practitioners to investigate fluid flow in deformable, variably saturated soils. Some of the more universal features of this code have been incorporated by Nari, through collaboration with Dr. Christopher Preller and myself, into a finite element code, Flumps, that can handle a variety of flow conditions, including irreversible regional land subsidence. Through the development of these and related codes, their use as a research tool, and theoretical work on the mechanism of ground-water storage, Nari has made us keenly aware of the intimate link that exists between the mechanical behavior of soils and the flow of fluids through their pores under conditions as complex as those that arise during multiphase flow in geothermal reservoirs.

One of T. N. Narasimhan's most important recent contributions to ground-water science is his development, together with Dr. Karsten Pruess of Lawrence Berkeley Laboratory, of a computer modeling concept that makes it possible to investigate the interaction between fluid phases in the porous blocks and fractures of a rock subjected to large temperature variations. The concept, known as “MINC” or “Multiple Interacting Continua,” is based on a unique and powerful four-dimensional grid representation of the modeled medium at any given instant of time. The concept has already helped resolve some important issues relating to “fluid reserves and the production of superheated steam from fractured vapor-dominated geothermal reservoirs” (Jour. Geophys. Research, 1982), including observed pressure behavior that could not be explained otherwise. It has considerable promise as providing “A practical method for modeling fluid and heat flow in fractured porous media” (Soc. Petroleum Engineers Jour., 1985), such as those encountered in many instances in oil reservoirs. Another very important new contribution that resulted from a collaboration between Nari and Dr. Joseph Wang of Lawrence Berkeley Laboratory has been the elucidation of several hitherto not fully appreciated “Hydrologic mechanisms governing fluid flow in a partially saturated fractured porous medium” (Water Resources Research, 1985), such as the English chalk or tufts at the Nevada Test Site. For these and other accomplishments, the Hydrogeology Section of the GSA is recognizing Dr. T. N. Narasimhan today as one of its outstanding members. We extend to you, Nari, our congratulations and sincere wishes for a successful and fulfilling continuation of your intellectual and professional endeavors. We are proud to present you with the Oscar E. Meinzer Award and to claim you as a friend and colleague.

RESPONSE BY T. N. NARASIMHAN

Thank you very much, Shlomo. As always, you are very kind.

I feel very honored as I stand here before you as the latest recipient of a unique honor in the field of hydrogeology. Somehow the very success one longs for creates a feeling of humility. It will take me some time to convince myself that I have been elevated to the company of such distinguished hydrogeologists as Hantush, Poland, Stallman, Toth, and many others who have been honored by the Meinzer Award. Please permit me to reflect a little on this special occasion.

As a young geologist with the Geological Survey of India (GSI) I was assigned in 1957 to the investigation of ground-water resources. For one whose primary interest was in crystallography and mineralogy, ground water was hard to swallow at first. For many of my colleagues in the GSI, I was merely a water diviner! During the mid-1960s, when I did not find any possibility of moving away from ground-water studies, I told myself that if this is what I have to do I might as well try to do it well. My interest was then in the hydrology of the hard rocks of the Indian Peninsular shield. As you are aware, all of our nifty mathematical results such as the Theis solution are not useful for analyzing flow in the highly heterogeneous and fractured hard-rock systems. In an effort to search the literature, I wrote many letters, at personal expense, to geological surveys around the world about the status of hard-rock hydrology. An upshot of this effort was my unusually profitable contact with that outstanding organization, the U.S. Geological Survey (USGS). The late C. L. McGuinness, a former recipient of the Meinzer Award, was the Chief of the Groundwater Branch at that time. Thanks to him, I used to receive publications, notes, and preprints on a regular basis. One of these preprints, I am proud to say, led to perhaps the first field application of the Papadopulos-Cooper method for analyzing flow to large-diameter open wells. I derive a special feeling of satisfaction that I am being honored in the name of Meinzer, who, as a member of the USGS, pioneered the establishment of ground-water hydrology in the United States. I could not conceive of a better way of expressing my gratitude to the USGS for all the help that it extended to me during the late 1960s.

This exposure to recent literature kindled in me a desire to do something useful in ground-water hydrology. Several fortunate circumstances, such as a cherished meeting with Don Helm, who now lives down under in Australia, and a letter of recommendation from Mahdi Hantush, who knew me only from the literature, brought me to Berkeley for higher studies with Paul Witherspoon in 1970. When I arrived,
Shlomo was working on his Meinzer Award-winning work on aquifers, and Paul decreed that I should be known henceforth as "Nari." I knew subconsciously then that I had joined a distinguished hydrology family whose eldest son is Al Freeze.

During the past 16 years at Berkeley, I have had the luxury of dabbling in several different areas of hydrogeology, including saturated-unsaturated flow, numerical modeling, land subsidence, geothermal energy, flow in fractured rocks, and chemical transport. Within the past few years, I have become specially attracted toward understanding large-scale hydrogeologic phenomena related to earthquakes and colliding plate margins.

As I stand here today, I realize how fortunate I have been in life. I come from a family and a tradition that places a heavy emphasis on education and scholarship. My parents sacrificed considerably to offer me good education with an idealistic slant. I have had the extraordinary privilege of studying at the University of California at Berkeley and later of pursuing a career at the Lawrence Berkeley Laboratory. The freedom of thought and the liberty to pursue any line of research are special blessings I enjoy at Berkeley, which few in the world have the privilege of enjoying. Recognizing these blessings, my family and I have recently become proud citizens of these United States.

As I reflect upon the many institutions that have contributed to my honor today, I recognize the debts I owe to that unique institution, my family. My wife Vijaya, who is here today, and I will have been married for 24 years this coming Friday. With her remarkable flair for details and organization, she could have gone on to become an outstanding experimentalist, had she been given the appropriate opportunity. Instead, she has, over the past 24 years, dedicated herself to the development of our family, which includes our son Ravi, who is currently pursuing physical chemistry at Stanford.

In 1970, when I became possessed with this desire to go back to school for graduate studies at the age of 34, our life's savings in India were barely enough to buy ourselves a one-way ticket to Berkeley. Vijaya was rightly concerned about this and tried to caution me that we could not get back if things did not pan out, and we would be in trouble. I was in too much of a fantasy world to recognize her concern then. Looking back, I now recognize how close to stupidity I was. Fortunately, luck has been on my side.

In addition to running a disciplined household, she has been performing other superhuman acts such as pursuing a job, being my head doctor during times of my depression, or sharing my joy when something went well for me at the computer center. Sometimes I wonder whether for all the support I have received from Vijaya, I have achieved enough in my profession.

As I conclude this expression of thanks to you, I recognize that Vijaya surely has a claim to part of this Meinzer Award. I could not have come anywhere near receiving this recognition had it not been for her institutional support. Ladies and gentlemen, may I request you to please give her a big hand as I proudly share this honor with my wife, Vijaya. Thank you.

Presentation of the G. K. Gilbert Award to Ralph B. Baldwin

CITATION BY DON E. WILHELMS

Today, as we present the G. K. Gilbert Award to Ralph B. Baldwin, we have the privilege of again linking the names of the two greatest early pioneers of lunar geoscience.

Now, that statement contains several elements that I need to justify. First, were they both pioneers? Of course, we know that Gilbert was. 1892 was the year in which a mere 18 nights at the telescope revealed the main elements of lunar stratigraphy and the impact origin of lunar craters and the Imbrium basin to this great and versatile scientist (as Baldwin described him). Or, as a member of Congress put it in an early Golden Fleece Award to the U.S. Geological Survey, "So useless has the Survey become that one of its most distinguished members has no better way to employ his time than to sit up all night gazing at the moon."

But what about Baldwin's status as a pioneer? First, consider that he was born in 1912, 6 years before Gilbert's death, and that the date of his first lunar studies was 1941. That landmark year is only a little more than half way from Gilbert's nights of "gazing at the Moon" to today. Let all of us ask ourselves what we were doing in 1941. Later, I want to enumerate some of Ralph's groundbreaking achievements that place him with Gilbert in the category of "great." For the moment, recall other pre-Space Age lunar literature that you may have read. I'm sure you will agree that with a few notable exceptions, it can be kindly described as nonsense.

Another term I used in the introduction was "geoscience." I suspect that Ralph flinched at that word. He was trained as an astrophysicist and probably had never heard the word geoscience in 1941—certainly, he had never heard it applied to the Moon. His doctoral thesis concerned Nova Cygni III, which flared up in 1920, and he devoted much of his early work to stars of a certain spectral class. As he confesses in an address delivered in 1978 to the Meteoritical Society and published in Meteoritics, he was among that great majority of astronomers who regarded the Moon as a nuisance whose presence in the sky interfered with the observation of more significant objects. And his ultimate profession concerned neither the stars nor the