A Career in Thin Air

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My long space science career did not start because of youthful dreams, nor after some long and careful planning, but rather by a fluke. In 1959 I was a graduate student in Electrical Engineering at the University of Michigan interested in automatic control and I needed a summer job. I had difficulty finding one, because I was an Australian citizen at that time. I went to see the Chair of the Department, who recruited me, and asked him for help. He told me to see Nelson Spencer and that he would give me a job. At that time I did not know that Spencer was the Director of the Space Physics Research Laboratory (SPRL). I went to see him and he gave me a job to design electronics for a sounding rocket instrument that they were planning to fly. I never left and that is how my wonderful 60 year career in space science began.

Initially I worked on retarding potential analyzers, Langmuir probes, and mass spectrometers. The first significant project that I was involved in was what we called the 13-inch sphere. This was the forerunner of future aeronomy satellites such as the Atmosphere Explorers. It contained omegatron mass spectrometers, Bayard-Alpert pressure gauges and a cylindrical Langmuir probe (as well as a telemetry transmitter). It was to be carried by an Aerobee sounding rocket, ejected and spun up. Unfortunately the Aerobee underperformed and while everything worked as planned, no science was obtained because the altitude was too low (slightly over 100 km).

SPRL had been launching Langmuir probes for a number of years, but the electron temperatures obtained by these measurements were not “in line” what was expected. These temperatures were significantly higher than the expected neutral ones and for years, it was suggested that there was something wrong with the results. Given great 20-20 hindsight it should have been clear that the electron temperatures in the ionosphere should not be the same as the kinetic neutral gas temperatures. When I was honored with giving the Nicolet Lecture in 1998 I did some research on the history of this issue (unfortunately I lost my notes from this lecture, so this is mostly from memory). The aeronomers in the 1950’s were mainly atomic physicists and they thought about rotational, vibrational and kinetic temperatures, but the issue of plasma temperature was not really addressed. The American edition of Rawer’s book *The Ionosphere*, published in 1956 (*Rawer, 1956*), has a one-line hint that the electrons may have higher velocities. In the early 60’s Bill Hanson and Frank Johnson looked at this issue in a couple of presentations and did some basic calculations to try to quantify the ionospheric electron temperatures (*Hanson and Johnson, 1961; Hanson, 1963*). Soon a number of papers started to be published looking at this issue in quite some detail (*Dalgarno et al, 1963; Geisler and Bowhill, 1965; Nagy et al., 1969*). It was not until about 1963 that the fact that the electron temperatures can be significantly higher than that of the neutrals (and ions) was uniformly accepted by the scientific community. It is also interesting to mention here that many years later Peter Banks found (*Banks,* 1969) a paper (*Drukarev*, 1946) published in Russia in the late 1940's in which the author predicted the mean electron temperature in the ionosphere in terms of the mean photoelectron energy; Western scientists were not aware of this paper.

The other project I was involved while a graduate student was what we called the 8-inch sphere. This was a spherical retarding potential analyzer and a cylindrical Langmuir (with a self-contained telemetry transmitter). Two of these were launched from Eglin Air Force Base in Florida in 1962. The two launches took place around midday and midnight; the spheres were ejected from a clamshell nosecone around 90 km on the up-leg portion of the flights. The data provided in-situ simultaneous measurements of the electron and ion densities and the electron and ion temperatures. These simultaneous observations of the daytime electron and ion temperatures demonstrated for the first time the significant difference between electron and ions in the ionosphere and that they are both higher than the accepted neutral gas temperatures. The data also indicated very good agreement between the electron temperatures measured by the Langmuir probe and retarding potential analyzer (*Nagy et al., 1963*).

Here I want to make an "interesting aside" comment. We (SPRL) had a couple of vacuum systems, used mainly to test and calibrate some of the instruments at pressures corresponding to thermospheric ones. This led me to my first scientific conference, namely that of the Vacuum Society around 1961. I listened to numerous presentations in which the speakers were proudly claiming all sort of breakthroughs and successes. The keynote speaker was Paul Redhead, the Director of the GE Schenectady Research Laboratory. He was the author of the major book on vacuum technology and the real expert in this field. In his presentation he indicated that we know little, are just learning and have quite a way to go. He really impressed me and ever since that time I have come to respect scientists who do not claim to know everything.

In 1963, the first real aeronomy satellite from the Goddard Space Flight Center, where Nelson Spencer moved to, called Atmosphere Explorer-A (AE-A, originally designated as S-6) was launched. To a point it was the heir of the 13-inch sphere. It carried a quadrupole mass spectrometer, Redhead and Bayard-Alpert pressure gauges, and a cylindrical Langmuir probe. Previous experience in flying a mass spectrometer in orbit was a failure in effect, because the contamination it carried was so large it made no meaningful atmospheric measurements (note the densities in the thermosphere are about 8-9 orders of magnitude smaller that the terrestrial values). To eliminate/minimize this issue the mass spectrometer to be carried by AE-A was evacuated and vacuum sealed by a cap well before launch. The cap was to be blown off shortly after the spacecraft got into orbit. There was concern that the low temperatures would freeze the cap and then it would not release. So shortly after my graduation Spencer asked me to go to Woomera, in central Australia where there was a NASA ground station and send the command during the first orbit, shortly after launch, to blow off the cap. Of course I was thrilled to go there (especially because before I came to the US I spent 8 years in Sydney, where I received my bachelor's degree). The area around Woomera is truly flat except for a small hill. Of course once we established where AE-A would be, it turned out that it was no more than 5^o over the horizon, right behind the hill. Thus there was a lot of excitement. This was an era well before auto commands, so I stood at the control table and kept pressing the command button as fast as I could and happily it was received and the cap flew off and the spectrometer did obtain useful atmospheric data.

I continued to work on Langmuir probes, which included in being selected as a P-I for a cylindrical Langmuir probe on OGO-6. There existed a somewhat anomalous situation at Michigan as there were two large and successful research laboratories in competition with each other, SPRL of the Electrical Engineering Department and the High Altitude Laboratory (HAL) of Aeronautical Engineering. I was appointed an Assistant Professor in Electrical Engineering in 1963 and Paul Hays was also a young Assistant Professor working in HAL. We got to know each other and decided to try to build bridges between the two labs. So sometimes around 1967 we went to lunch and discussed this issue. Paul just got back from a year in Belfast where he heard about Fabry-Perot interferometers and I just learned about mid-latitude red arcs (SAR-arc). We agreed that we should jointly build a Fabry-Perot and study SAR-arcs. When professors decide to do something they usually look for a prospective graduate student to do the work. We were no different and enlisted Ray Roble to do this for his Ph. D. thesis. Ray did a great job. A 6-inch Fabry-Perot was built, thermospheric temperatures were successfully measured using the 6300 A oxygen red line and Ray got his Ph. D. After graduation Ray proceeded to never do anything experimental again and instead, became a fantastic modeler. Paul and I hired two excellent postdocs; first John Meriwether and then Vince Abreu who over the years made important airglow and auroral measurements with the interferometer and associated equipment, which was moved to outside Fairbanks, Alaska.

My very first two post-docs however were Rich Stolarski and Ralph Cicerone. Among the project that Rich was working on after his arrival in Ann Arbor was a 1-D auroral model. When I first went to Fairbanks and saw a “real” aurora I threw up my hands and thought that modeling this phenomenon surely needs a major model that we are far from being able to create. Ralph and I worked mainly on photoelectron models in the beginning. At some point we got a small contract from NASA/MSFC to look into the environmental effects of rocket and shuttle launches. Rich took a leave of about 9 months in 1972/73, and went to NASA/MSFC, where he worked with Bob Hudson. After Rich’s return, he and Ralph really started to work on atmospheric chemistry, especially the catalytic destruction of ozone by chlorine. They got a lot of help, especially from Don Stedman, who was a faculty member at Michigan. Eventually they presented a paper on the impact of chlorine on ozone at the Kyoto IAGA Meeting in 1973. When we all returned from this meeting, Rich and Ralph felt discouraged because of all the science politics they encountered. I strongly pushed them to get their work published nevertheless and it was published in the Canadian Journal to Chemistry, and this really put them “on the map”(*Stolarski and Cicerone, 1974*).

My first sabbatical year in 1969-1970 I spent at UCSD in La Jolla in the Electrophysics Department chaired by Henry Booker. It had a great group of people (among them Ian Axford, Peter Banks, and Jules Fejer). Working with Peter and Ian was a great opportunity. I learned to program in Fortran and wrote my first program solving the two-stream photoelectron problem.

(Another interesting aside is that I ran into some trouble in the solution of the upstream flux and I added a quick temporary fix. I shared the program with many groups around the country and as far as I know the quick fix is still being used!). With Peter and Ian we also published a paper on the refilling problem of the high latitude ionospheric flux tubes (*Banks et al.,* 1971). This period also introduced me to journal editing. Ian was the Editor of JGR Blue, or JGR Space Physics as it's known now, and during his travels he asked me to substitute for him. I enjoyed it and this led me later to a couple of Editorships.

At a backyard barbecue Ian, Peter, and Rick Chappell (who was also visiting for a couple of months) and I came up with the idea of a satellite program, which would study field-aligned phenomena. It was to consist of two highly elliptic satellites following each other along field lines and a third low altitude spacecraft in circular orbit measuring conditions at the foot of the field lines. Neil Brice liked the idea and volunteered to hold a meeting at Cornell to discuss these ideas in more detail. Not much later Ian Axford and I went to NASA Headquarters to sell the idea for this mission. I remember that John Naugle, who was the NASA chief scientist at the time, asked why should we have another mission to the magnetosphere, given that we already had been there. Eventually we did succeed in making this mission a reality, but only after serious descoping. The mission became known as Dynamics Explorer with only two spacecraft, one in elliptical and one in circular orbit (*Dynamics Explorer, 1981*). Bob Hoffman deserves a lot of credit in making this mission happen. It was very successful, providing a lot of exciting new insights into ionosphere-magnetosphere coupling science.

In anticipation of these new ionosphere-magnetosphere research Rick, Peter and I organized the first Yosemite Conference at the Ahwanee Hotel, a magnificent location. It was a great meeting and the start of many more conferences in Yosemite. There was one tragic event associated with this meeting; Neil Brice was on his way to Yosemite from Australia, and was killed when his plane crashed in Fiji. Rick arranged to have the meeting video recorded and 40 years later, in 2014, he organized a follow up meeting, also in Yosemite, where he showed some video clips from the 1974 meeting. It was very interesting to see how insightful the various speakers were at that time. Details have changed but overall a basic understanding was already present.

There was another fluky and very happy event in my career in the early 1970's. Up to this point all my work was in the area of terrestrial research. I believe it was sometime in 1971 when AO was issued by NASA for a proposal to become member of the Pioneer Venus Science Steering Group. This is an area which was totally new to me and there was no reason for me to apply. However, the proposal was limited to 5 pages, including references and biography. I really owe a great deal of thanks to George Carignan, who encouraged me to propose. Here is another of those fluky events where I was selected and this opened the door for my career in planetary aeronomy. My Pioneer Venus career lasted over 25 years and was a wonderful experience (*Fimmell et al., 1983*). I learned a lot of planetary aeronomy and met many wonderful people who became lifelong friends.

After the success of the Pioneer Venus Orbiter we tried to "sell" a similar mission for Mars. It was a long and hard sell. We had two brainstorming meetings at JPL in the Fall of 1985 and a report of the group's recommendation was published as a JPL report in 1986 (*MAO Report, 1986*). It is interesting to mention that the recommended payload is nearly identical to the MAVEN one (except of course for more up-to-date instruments). Nearly three decades later MAVEN (Mars Atmospheric Volatile Escape) mission became a reality and providing outstanding information on the ionosphere, upper atmosphere and solar wind interaction processes. I am fortunate to be a Co-I of this mission (thanks to Bruce Jakosky's generosity) involved in data interpretation. This helps me to stay active to a limited degree, and be involved in current and exciting science.

My second sabbatical was at Utah State in 1976-1977. It was another great year meeting new people and being exposed to new and different scientific issues. I got to know Bob Schunk and we eventually wrote two lengthy review papers on the observations and theory of the terrestrial upper ionosphere and on the ionospheres of the terrestrial ionospheres (*Schunk and Nagy, 1978; Schunk and Nagy, 1980*). This work eventually led to our book "Ionospheres" first published in 2000 and a second edition published in 2009 (*Schunk and Nagy*, 2000; 2009).

At the 1977 IAGA Meeting in Seattle I met Tamas Gombosi, who just finished working on the Venera electron flux data with Konstantin Gringauz at the Russian Space Science Institute (IKI). I bought him a dinner the first night, because he was a starving young Hungarian scientist and that was the start of a long and great friendship. I invited him to work with me on the upcoming PV results. He accepted the invitation and he and his family arrived in Ann Arbor in 1978. The 1980 COSPAR Meeting was held in Budapest and at a dinner at the Gombosi's I met Roald Sagdeev, who was the director of IKI and who invited me to provide an instrument for the upcoming Soviet Phobos mission to Mars. Without getting into all the horrible political details in getting this to happen there was a thermal electron/ion instrument designed by Paul Hays and Bill Sharp built by the Hungarian KFKI engineers and flown on both Phobos spacecraft. One of them provided useful data from the surviving spacecraft orbiting Mars before the mission failed. I also became involved in the Soviet VEGA mission, which introduced me to another new field, cometary science. My final involvement with a Russian planetary mission was Mars96. I was in Moscow at the Star City where we learned its unfortunate failure.

My last major and extended scientific effort was associated with the Cassini Mission. I was selected to be a member of the Radio Science Team (*Kliore et al.,* 2004) . The Orbiter and Probe spacecraft were launched in 1997 and the Orbiter was put in orbit around Saturn in 2004. During the 13 years of operations we learned a tremendous amount of new and exciting information about Saturn and its satellites. My interest was concentrated on the ionospheres of Saturn and Titan. We made significant advances in elucidating the physical and chemical processes controlling their behavior, but for better or worse there are still numerous questions which remain to be answered by the next generation of scientists, with a new mission, hopefully not far in the future.

My AGU career was a long and very satisfying one. My first AGU Meeting was the Washington one in 1962. I went to my first Western AGU Meeting at Stanford in 1963, where the attendance was only a few hundred. I also attended a number of Western Pacific AGU Meetings, before they were taken over by the AOGS Meetings. My first official position with AGU was as Secretary of Aeronomy, a position I held from 1974 to 1977. The main job of the Secretary was to arrange the sessions for the meetings, which was done at Headquarters using nothing fancy just a board and thumbtacks. The struggle was centered around the "good" rooms (e.g. the ones in the Jack Tarr Hotel), where the meetings were held, until some sessions spilled over to the nearby Holiday Inn. I was elected President Elect of SPA in 1990 and took over as President two years later. I served on and/or chaired numerous AGU committees, such as Editorial Search, Macelwene, Waldo Smith Medal, Fellows and Meetings committees.

In 1975 Noel Hinners contacted me to encourage me to apply for the Editorship of the new Geophysical Research Letters. I was selected to be the Editor for a 3-year term. The job of the Editor at that time was multifold. Besides acting as a single editor covering all fields from hydrology to solar physics, the job also included work to ensure that this new journal survives. That involved a somewhat contradictory tasks of recruiting papers while acting as the editor. We tried very hard to keep the time from submission to actual print publication down to 3 months, in order to make GRL a very desirable place to publish. We pretty much succeeded and remember this was before e-mail and Internet! I was warned that as a new editor people will try to sneak in crank papers. I did receive a few Bermuda Triangle and similar submissions. Another interesting memory is a paper that I asked a very well-known and respected scientist to review. His review came back with a letter saying: "Dear Andy, This is biggest BS I have ever read". However, the review was not very specific, so when I rejected the paper the author complained bitterly. In general, during my 3-years of Editorship I only remember one paper for which both reviewers said publish as is.

In early 1980's I became the Editor for Reviews of Geophysics. This was a much easier job than GRL, but it still required recruiting good papers. When I became the Editor I felt that here at least I did not have to worry about crank papers. I was wrong; one of the first papers I received was titled "Natural satellites (!!) of the Earth".

I received my first faculty appointment at Michigan in 1963 and a rich and satisfying career followed. Initially my appointment was in the Electrical Engineering Department, but eventually my appointment was shifted to what was then known as Department of Atmospheric, Oceanic and Space Sciences (AOSS). In the 1950's and 1960's space science was housed in Electrical Engineering Departments at numerous universities for historical reasons. Michigan also had a very strong space science group in its Aeronautical Engineering Department. Here is a typical academic story. A number of us from the various departments decided to create an interdepartmental program and decided to call it Planetary and Space Science after the journal. The Geology Department vetoed "planetary" and the Astronomy Department vetoed "space". We decided to call the program Aeronomy, given that very few people ever heard of that term and we got it approved. It only lasted a few years and eventually all space science programs found a home in AOSS.

My students and postdocs provided me with a lot of gratification. Among my very successful students, just to name a few, are Pierre Bauer, Ray Roble, Sushil Atreya, Janet Kozyra, Hunter Waite, Steve Bougher, M. C. Fok, Yingjuan Ma and Dalal Najib. I basically only had five postdocs, but was I lucky! In chronological order they were Rich Stolarski, Ralph Cicerone, Bill Chameides, Tom Cravens and Tamas Gombosi. I owe all these people a tremendous amount of thanks.

In summary I have had a wonderful journey in space science. I was lucky that my career started pretty much with the beginning of the space science era, when most measurements presented something new, exciting and unexpected. It was also a time when there were plenty of opportunities and finding support was relatively easy, unlike the challenge it is today. There is still a lot of great science to be done and I wish my younger colleagues all the best.

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