GENISIS OF LASER SATELLITE TRACKING AT NASA /GODDARD
Henry Plotkin, OCTOBER 27, 2014

Thank you to the Workshop planning committee, for inviting me to give an old man an opportunity to reminisce, and thank you to those here who got up early to listen to him. As promised, I will relate some idea of events leading to launch of the first Beacon Explorers and receiving the first laser reflections. It is absolutely amazing to me that these events occurred more than fifty years ago. I won't go beyond that, because there are too many at this workshop who could do a much better job with a narrative about the later satellites, how the technology evolved, the wonderful accomplishments and applications to which it is being put.

In 1954, while a graduate student at MIT, I was drafted into the army. I was assigned to the US Army Signal Research and Development Labs, attached to a group developing atomic and molecular frequency control systems.I found myself working side by side with Fritz Vonbun, whom many of you remember with great affection. I got to collaborate with great people such as Robert Dicke at Princeton University, Charles Townes at Columbia University, Jerrold Zacharias at MIT.

In 1958 NASA was created. In May, 1960, Fritz and I succumbed to “Space Fever” and moved to the NASA Goddard Space Flight Center. My job was supposed to be developing optical systems for photographing satellites and stars in order to calibrate Microwave and Radio tracking systems. As I recall, typical radio tracking precision was then several hundred meters. Two months earlier, as Fritz and I were making our farewells before leaving New Jersey for Goddard, we visited Princeton and discussed our plans with Dicke and his group. Bill Kaula and Caroll Alley were also at that little meeting. Bob Dicke was developing a new general theory of relativity and he was looking for sensitive tests that would verify his theory rather than Albert Einstein's. He believed that tracking the moon or other high satellites with high precision and over a very long time would ultimately distinguish between the two theories. He suggested that when I get to NASA I consider putting cube corners on the moon or launching cube corners into space orbits and photographing reflections from powerful searchlight illumination.

Actually, the ealiest mention of putting cube corners into space orbit that I could find was by John O'Keefe in November, 1954 when he was at the Army Map Service. Then, in January 1960, a paper by Dicke, with Hoffman and Krotkov, appeared in the IRE Transactions, entitled “Pricise Optical tracking of Artificial Satellites”, recommending cube corners and searchlights as well as other possible techniques. All this before I arrived at Goddard. The most precise tracking of satellites at that time was to photograph them at twilight, while they were still in sunlight,against the dark sky star background. Precision was usually abount 2 arc seconds, i.e. about 10 meters along track for a satellite at an altitude of 1000 Km. But this was possible only over short segments of the orbit, and required considerable hard work in measurement and analysis.

So, I arrived at Goddard May 1, 1960. Bill Kaula, working with John O'keefe at Goddard, wrote an internal feasibility study on May 29 based on use of orbiting cube corners and searchlight illumination.Meanwhile, Dicke had been speaking to Homer Newell and Nancy Roman at NASA Headquarters and with John O'keefe at Goddard to advocate an experiment of this type. Robert Jastrow also wrote a memo to the Director at Goddard on the same subject. The funny thing is that my old notebooks don't show that I was at all aware of those memos and conversations, while I was busily involved in my new duties as Head of the Optical Systems Branch.

Meanwhile, I spoke to people at the Fort Belvoir Army Corps of Engineers in the Illumination Lab about capabilities of the latest anti-aircraft searchlights. (Ted Maimon at HughesAircraft in California had just demonstrated the ruby laser, but I hadn't yet connected it with my needs.) I studied the number of photons required to produce a spot on a photographic film, thought about alternate designs of retroreflecting satellites, and started to draft proposals for a space experiment. There were lots of interesting questions to consider: Velocity aberration? Solid cube corners or hollow? Silvered or total internal reflection? What size individual cubes? What precision? What configuration of array to account for satellite spin and changing orientation? Receiver: collocated with light transmitter or moved around to center of reflected pattern to account for velocity aberration for each pass?

Now occurred one of the fantastic pieces of luck that I have experienced throughout my life. One day, John O'Keefe told me there was a series of scientific satellites being planned that might be able to carry retroreflectors as piggyback experiments. The project was the Beacon Explorers, identified by Goddard as S-66. It was to be in the shape of an octangular cylinder 20 inches across, magnetically stabilized so that one end always pointed along the north-seeking magnetic lines of force. It's main objective was to transmit signals to receivers on earth over a set of frequencies in the radio spectrum in order to map ion and electron densities in the ionosphere. I met with the Project Manager, Bob Bourdeau, and, just like that, he agreed to leave the north-pointing face free of instruments and available for an array of retroreflectors. (There were truly giants on Earth in those days.)

Suddenly there was lots to do: I had to prepare a proposal and convince my boss, Buck Schroeder and his boss Jack Mengel that this was a worthwhile investment. At this point I was only promising to extend the precision of photographic tracking a little further along the orbit. I was not yet aware of the power of precision range measurements. About this time Bob Coates became my boss and close colleague, and his encouragement and advise were very important to me. My budget was small and very naïve. I think that most of the funding support for the experiment came from OART, the Office of Advanced Research and Technology side of NASA Headquarters. My colleagues at NASA HQ were Roland Chase and his boss John Walker. I remember that the Office of Tracking and Data Acquisition, OTDA, which was supposed to support most of the other work I was doing, looked on with mild interest from the sidelines.

The formal proposal was submitted and approved in early 1962. By then, we had wised up and dropped the plan to use searchlights, proposing instead to use a ruby laser similar to Ted Maiman's. (I think ours may have been the very first application of the newly invented laser.) Howie Genatt, in our team, still planned to record reflections on astronomical photographic plates, but we had added another photon-collecting telescope, with a photomultiplier at its focus to record time-of-flight. We planned to photograph the delayed sweep of an oscilloscope showing the photomultiplier signal.For later passes, Tom Johnson, Lou Caudill, and Paul Spadin designed and built high-speed time-interval-units and experimented with triggering TIU's with transmitted and reflected photons.

We collaborated with General Electric Space Division at Valley Forge, Pennsylvania through a contract to build the reflector array and some of the ground equipment. They subcontracted with Boxton Beal for manufacture of the fuzed silica cube corner reflectors. GE also built several copies of Maimon's ruby laser, both air-cooled and water-cooled. The senior engineer there was Al Grafinger, and he was joined by Gordon Snyder, Stan Hurst, and William Meyers. The S-66 project was built and/or assembled at the Johns Hopkins University Applied Physics Laboratory.

There seemed to be a good deal of excitement generated by our project. In February, 1963, I presented my plans for S-66 at the Third Quantum Electronics Conference in Paris. At that same meeting, Prof. Dicke spoke about the benefits of retroreflectors on satellites and on the Moon. Alfred Kastler, who later won the Nobel Prize in physics, suggested that we try to measure Doppler shifts in the reflected ruby radiation. After the meeting a number of European representatives spoke to us about setting up their own stations. I particularly remember speaking to Jacques Blamont of the observatory at Meudon in France and to Jacques Bonanomi from Neuchatel, Switzerland.

By now we had assembled a small but very elite team at Goddard: Walter Carrion was my Associate Branch Head and an experienced developer of tracking telescopes and optical instruments, Tom Johnson, an optical physicist, Lou Caudill, Paul Spadin, John Moye, Nelson McAvoy, Howard Genatt, Dennis Mathews, Ed Reid, Charles Peruso., and certainly others whom I can't remember. Oh yes! John Degnan joined our team as a very young co-op student from Drexel University.

We all know about BE-B and BE-C, but no one talks much about BE-A. Third-stage rocket failed to ignite and it never made it into orbit. BE-A was launched on a Scout Rocket from Wallops Island early in 1964. So, … this was my first launch experience. The seasoned project team and launch range engineers must have been amused by this group of young earnest “laser scientists” huddled nervously in the control bunker near the launch pad. The first stage rocket launched flawlessly. Afterwards, as we are standing around congratulating each other, a dusty launch tower technician comes into the control room and asks,

“Does anyone know what these are? We just found them scattered over the ground near the launch tower.”

There, cupped in his hand, were what looked like a half-dozen of our fuzed silica cube corners. I turned pale, and almost had a heart-attack. After a long terrifying moment I finally realized that these were dummy cube corners I had given to the project for vibration testing for spacecraft integrity. Bob Bourdeau, the Project Manager had arranged for the spoof.

For BE-A we had set up a laser station next to one of Wallops Island's optical tracking telescopes, replacing the movie camera with a photomultiplier assembly. We were also planning to photograph the reflected light from the satellite. I have this image of the young John Degnan lying on his back under a Minitrack camera installing the 8x10 film cassette. (That's what co-op students do.) So much for BE-A which is resting peacefully on the bottom of the Atlantic Ocean off the coast of Brazil.

Explorer 22 BE-B was launched from the Western Test Range, Vandenburg AFB, California, on October 9, 1964. In the interval between BE-A and BE-B we had set up the laser tracking station at the Goddard Optical Research Facility and moved all our operations there. A few weeks later, on October 31, 1964, the team at GORF recorded the first weak laser echos from BE-B. In that first successful pass, the output of the 9558A photomultiplier tube was photographed on an oscilloscope. For that pass, we found fewer than 10 verifiable returns in approximately 200 oscilloscope traces. Fortunately, it was not too difficult to distinguish laser echos from the ever-present photomultiplier noise, when we had finally learned to make all of the necessary adjustments.

The telesope for the GODLAS system (short for Goddard Laser) was mounted on a modified Nike-Ajax missile radar tracking mount controlled by two operators at viewing scopes, guiding on the sunlit satellite using joystick control. One operator controlled azimuth and the other controlled elevation. On that eventful pass when we received the first returns, Walt Carrion was controlling one axix, and I was on the other.

Early in 1965, an SAO team, I remember Carlton Lehr very fondly, also using a ruby laser provided by General Electric, received its first laser returns from Explorer 22 at its optical tracking site at Organ Pass, New Mexico. Both of our results happened to be reported in the same March 1965 Issue of the IEEE.

That 3-week period between launch of BE-B and receiving our first returns was absolutely excrutiating. There were so many parameters that had to be calibrated, co-aligned, adjusted, optimized, ... whatever, … Looking back, it was really not very long, but it seemed interminable at the time. Each week, I appeared before my boss's staff meeting and lamely tried to summarize my excuses. And at each opportunity, Fritz Vonbun, who really needed major funds for computer time for his complex orbit and gravity field analysis, tried to divert the money that I was spending over to his program: “All that money for gadgets, gadgets, gadgets, and nothing for science.” Finally, when my signals started to arrive regularly, they had precisions and accuracy of about a meter rather than the hundreds of meters from radio trackers. When inserted into orbit computations, the sudden improvements were very dramatic. It wasn't long before Fritz appeared at my door, asking meekly, “Henry, do you think you can get 10 centimeters for me?”

Once we learned how to track, it's amazing how quickly our operations became almost routine.Our predicted pointing coordinates and times were sent by telephone and turned into punched paper tape to drive the mount; Oscilloscope sweep times were replaced by digital Time Interval Units;Triggers based on pulse leading edges were replaced by pulse centroid triggers; And so on … We started to track daylight passes, routinely.

BE-Cwas launched April 29, 1965 from Cape Canaveral, about 6 months after BE-B. Within days we were making hundreds of range measurements per orbit. We built a laser tracking station into a mobile trailer so we could move it to radio tracking stations to help in their calibration.

I'm going to leave the story there, fifty years later. Wow! The refinements and applications have been fabulous. They could never have been anticipated, at least not by me. That story must be told by those in this auditorium, to whom the credit must be given for turning the technology of fifty years ago into so powerful a scientific tool. I am grateful to have had the fortune to be there at the beginning and to know and work with such distinguished scientists and good friends. I wish you a successful workshop.