EBERT SPECTROMETER REFLECTIONS

Hermann Ebert's 19th-century spectrometer was reinvented and thus revived from undeserved obscurity to pioneer the study of planetary atmospheres in the age of space flight. Its history is a comedy of errors.

William G. Fastie

The Ebert spectrometer, named for its inventor, the 19th-century German spectroscopist Hermann Ebert, emerged from obscurity after the Second World War to play a significant role in the exploration of the solar system. I had a part in its resurrection, and the Ebert spectrometer has played a dominant role in my scientific career. Therefore I've taken the trouble to look into its history. The evolution of this instrument, now a century old, was curiously haphazard and fraught with mistakes. I have attempted here to put these events into chronological, and somewhat autobiographical, perspective.

Hermann Ebert described his spectrograph in 1889.[1] Figure 1 is the illustration from his original paper. The spectrometer consisted of an entrance slit, a concave spherical mirror, a plane grating and a small photographic plate in the plane of the entrance slit. The concave spherical mirror acted as both collimator and camera. Ebert's paper asserted that this arrangement gave very good spectra. But he did not explain why, nor did he ever publish anything further. That was the first mistake in this curious chronicle.

In the 1900 edition of his prestigious *Handbuch der Spectroscopie*, H. H. Kayser described Ebert's spectrometer.[2] Kayser asserted that the design wouldn't work because rays from the slit could go directly to the mirror and strike the photographic plate. But a simple baffle in the entrance beam (which Ebert probably used and failed to mention) would have sufficed to prevent these rays from reaching the photographic plate. This overhasty dismissal by Kayser was the second mistake in our story.

Kayser also said that its "double-diameter mirror" made the instrument impractical. He did not realize the great advantage in optical adjustment provided by a single large mirror in place of two mirrors half its size. The third mistake.

Kayser's two misjudgments were crucial. This was a classic case of "uninventing the wheel." The Ebert spectrometer was not mentioned again in the scientific literature for the next half century.

In 1930 M. Czerny and A. Francis Turner[3] described an infrared plane-grating monochromator that used symmetrical off-axis spherical mirrors as collimator and camera. The scheme is shown in figure 2. They explained that the coma distortion of the wave front arriving at the off-axis grating was cancelled by the symmetrically off-axis camera mirror. This was an important simplification over the off-axis parabolic mirror-grating systems of the day. But the Czerny–Turner system was not widely adopted. Perhaps everyone assumed that the coma correction was adequate for infrared wavelengths but would not be adequate for the visible or ultraviolet regions. If that was the general opinion, then almost everyone shares responsibility for error number four.

Hooked by neon lights

In 1933, at the very bottom of the Depression, I graduated from Catonsville High School near Baltimore with an undistinguished scholastic record. The new Roosevelt

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Administration had established a nationwide program of free college-level evening courses given in local high schools for those who could not afford to go to college. The instructors came from the horde of Ph Ds for whom there were no jobs. Thus the program simultaneously addressed the crying needs of both teachers and students. I attended classes at Forest Park High School in Baltimore in the fall of 1933. The physics instructor was John A. Sanderson, who had just received his Ph D at Johns Hopkins. His thesis advisor, shown in figure 3, was A. H. Pfund, a pioneer of infrared optical and detector technology.

After one of the evening classes, Sanderson gave me a small transmission diffraction grating. In those days many of the storefronts in Baltimore were adorned with neon signs. The transmission grating, held in front of the eye, produced a beautiful spectrum of emission lines. I soon found that many of the neon signs also contained mercuty or sodium. Some of the "neon" signs contained argon but no neon. I was seventeen years old and hooked for life.

Sanderson also convinced me that I should attend evening classes at Hopkins, which I did, starting in the fall of 1934. The $20 a month I was earning was enough for tuition and yet not enough to significantly improve the family finances.

In the fall of 1937, I was awarded a physics scholarship in the graduate school. Throughout its history, Johns Hopkins has never required an undergraduate degree as a prerequisite for admission to the graduate school, but that didn't make life any easier. I survived by concentrating on experimental physical optics and spectroscopy, studying mostly with Pfund, R. W. Wood and Gerhardt H. Dieke, and by communing with the many ghosts of Henry A. Rowland that haunted every darkroom of Rowland Hall. (Rowland is pictured in figure 4.) Advanced mathematics, quantum mechanics and theoretical physics were beyond my ken, and still are.

In 1940 Pfund, who had become detpartment chairman when Wood retired in 1938, offered me a research assistantship to work on optical and infrared programs financed by the National Defense Research Council. My student days were over, but I still had no academic degree of any kind. Perhaps by way of compensation, I had grown 8 inches taller since high school graduation.

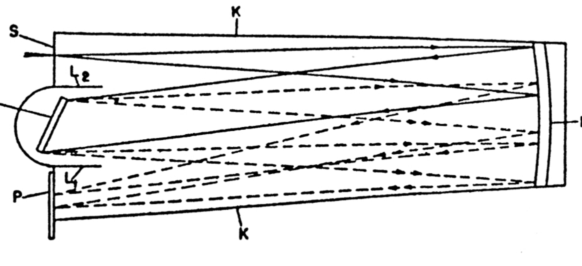
Pfund had developed an accurate method of measuring the refractive index dispersion of a Lucite prism provided by DuPont. He used a Czerny–Turner optical system with a photographic plate in the focal plane of the camera mirror. Plastic optics had become an important military priority. Pfund immediately received three more prisms, with a request for their dispersion curves. He handed me the prisms and the spherical mirrors he had been using. Of course, I set the system up backwards and got lousy spectra. I was smart enough, however, not to ask him what was wrong. I started randomly readjusting the optical setup. I even swiped the original Lucite prism from his laboratory and checked to see that the prisms I had were not of inferior quality. Ultimately I found the proper geometry and obtained good spectra. When I asked Pfund why this geometry worked, he gave me a reprint of the Czerny–Turner paper. I didn't try to find out how good the system really was. That was error number five of our story.

This work was only a very small part of the research I was involved in during World War II, but it had long-range significance. When the war ended, I left Hopkins in late 1945, still without a degree, to accept a position in the physics research laboratory of the Leeds and Northrup Company in Philadelphia. After a few exciting years working in optical and infrared pyrometry, I was appointed director of the firm's physics research division. This was the only administrative job I have ever held. It certainly had its dull moments, and one glorious highlight.

One of the programs I inherited with the new job was the development of a spectroscopic system for industrial steel analysis. Leeds and Northrup had academic consulting contracts for this development involving Dieke and Henry Crosswhite at Johns Hopkins. The steel analysis system was well along in its development phase, with one glaring exception. No spectrometer design had been established.

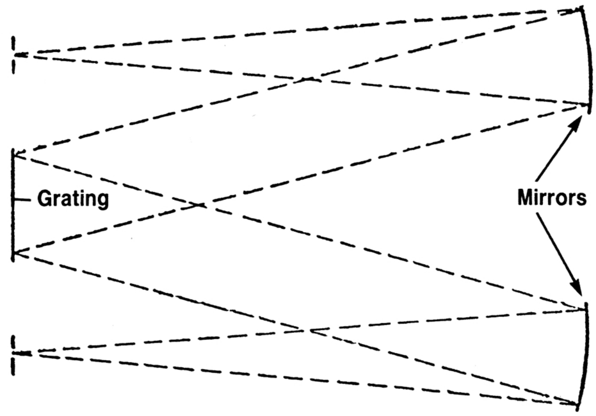
One afternoon in 1948, I visited Hank Crosswhite to discuss the spectrometer problem. We had become close friends when he was a graduate student in the early 1940s I spelled out what I thought were the necessary specifications. It had to be a wavelength-scanning instrument with a plane diffraction grating. It had to be small, simple and rugged, with high spectral resolution and precision wavelength readout. Not exactly a hash-house menu.

Hank had just finished assembling a plane-diffraction-grating scanning monochromator that used off-axis parabolic mirrors for the collimator and camera. I expressed concern about the complexities of making and adjusting off-axis parabolas. Then I flashed back to circa 1940 and told him about the Czerny–Turner system. I wondered aloud whether such a system would be good enough. Before he could respond, I began ruminating on the possibility of using a single larger mirror, one half for the collimator, the other for the camera. While we were thinking about this, Hank started to oscillate his head from side to side. I thought he was saying no, but he was in fact mentally tracing the rays through the optical system. Then he started nodding yes. I think we both almost immediately realized that the single large mirror was a Czerny–Turner system with the collimator and camera automatically (and eternally) adjusted with respect to



Hermann Ebert's 1889 drawing of his new spectrograph design. Light entering the entrance slit S is reflected by a single large concave spherical mirror H, which serves as both collimator and camera. After reflecting off the diffraction grating G, the light is recorded by a photographic plate P. **Figure 1**

Czerny–Turner correcting system for their 1930 infrared plane-grating monochromator. They attempted to cancel coma distortion by the use of symmetrically paired, off-axis concave mirrors. **Figure 2**



each other. The only remaining question was: How good is it?

We were in Rowland Hall, and Wilbur Perry was just down the corridor. He had been making spherical concave mirrors and ruling diffraction gratings on them with the Rowland engines since the year one. He also had his own basement mirror-polishing facility. (See PHYSICS TODAY, July 1986, page 34, for a discussion of Rowland's 19th-century engines for ruling diffraction gratings.)

Wilbur said he had a high quality spherical mirror with an 8-inch diameter and a 30-inch focal length that he could sell me for a ridiculously low price. A few hours later the mirror and I were on the train to Philadelphia. I had high hopes.

The catbird seat

The next morning I set up a 2½-inch interferometer mirror blank on a lens mount that hung over the edge of a laboratory bench. The mirror surface was horizontal and facing down. For an entrance slit I used an aluminized glass plate on which I had scratched many short, randomly oriented slits. The plate surface was also horizontal. It too hung over the edge of the bench, on one side of the mirror blank. I placed the concave mirror on an open optical supply catalogue on the floor. I varied its height by turning pages until the camera image was in the horizontal plane of the slit images observed on the other side of the mirror blank with a microscope. (See the layout in figure 5.) By lunchtime I had seen many images that were at the theoretical diffraction limit. I was sitting in the catbird seat.

The frantic pace with which I pursued this idea may have been important. If you think too much about what you are doing, your ardor may cool because there is a potentially fatal virus that can infect a good idea. Its Latin name is *Conventional Wisdom.* At Leeds and Northrup we immediately started the design and construction of a batch of 30-inch focal-length, single-mirror spectrometers, using stable iron castings for the structure and optical mounts. We obtained plane diffraction gratings from Bausch & Lomb. These were so-called replica gratings, molded from an original master. Bausch & Lomb had just entered this field with David Richardson as their key operative and George Harrison of MIT as their consultant.

In the 1930s, infrared spectroscopists were searching for more sensitivity. One can improve the signal-to-noise ratio by decreasing detector area. So they developed scanning monochromators with small f numbers (focal length/aperture diameter), which would focus the incident light into the smallest possible image on the detector. But that compromises spectral resolution.

It soon became obvious that the f number of the spectrometer should be made as *large* as necessary to obtain the desired spectral resolution, and that the radiation emerging from the wider and longer exit slit should be reimaged on the detector with a reimager of the lowest possible f number. This concept can be expressed by the equation

$$S \sim \frac{A_s A_g}{F^2}$$

where S is the output signal and F is the focal length of the mirror. A_s is the slit area (slit width $W_s \times$ slit length L_s). A_g is the diffraction grating area. This equation can be rewritten

$$S \sim (W_s/F) \times (L_s/F) \times A_g \qquad (1)$$

For a grating of a given size, one can only maintain a specified resolution by keeping the ratio W_s/F fixed when one fiddles with the specifications of the spectrometer. Thus the only instrumental parameter available to increase the output signal is the ratio L_s/F.

Curved slits

When we were testing the first set of spectrometers, we tried a rather long, straight slit and found that the

August Herman Pfund (1881–1948). **Figure 3**

spectral line image was sharp over only a short length at its center. It was soon clear that astigmatism was limiting the slit length. The Czerny–Turner system subtracts the off-axis coma of the two mirror sections, but the off-axis astigmatism *adds*. Because the centers of the two slits were equally distant from the central axis of the spherical mirror, we tried curved slits whose centers of curvature were on that central axis. This provided a spectacular improvement in spectral resolution, because each point on the entrance slit formed a short astigmatic image tangent to the curved exit slit. The catbird seat had moved to the top of a much higher tree. Nonetheless, the concept of curved slits may have been error number six, and the second one attributable to me. If so, it was an extremely creative error. I will explain later.

By this time, I was working in a goldfish bowl. In particular, Dieke and John Strong, who had accepted a professorship at Johns Hopkins after the war, were watching me closely. I found out how closely when Strong told me he thought the spectrometer was extremely important, and that I should return to Hopkins to work on it full time under his funding. I returned to Rowland Hall in August 1951 after an absence of five and a half years. If Leeds and Northrup was unhappy about this move, it didn't show. The company gave me a set of spectrograph castings to help get me started. In Rowland Hall there were plenty of mirrors and gratings. Very soon I had an operating spectrometer.

I had met Strong when he was at Harvard during the war, and we had become close friends when he came to Hopkins. He had started the design of a single-mirror, plane-grating infrared scanning monochromator with a 30-inch mirror that his students had dubbed "The Monster." He retained Howard Head to design the mechanical support system. Head was at that time also developing the Head ski. I don't know how good his skis were, but the design of the spectrometer structure was superb.

When I arrived at Hopkins, Strong told me the curved slit was a great idea. But, he added, it might not be diffraction limited, because grating spectral lines change their curvature with wavelength. He was very familiar with this problem because his thesis adviser at the University of Michigan, Harrison Randall, had designed an infrared scanning monochromator with the entrance slit configured so that it could be slightly curved as the wavelength varied, enabling one to use longer slits.

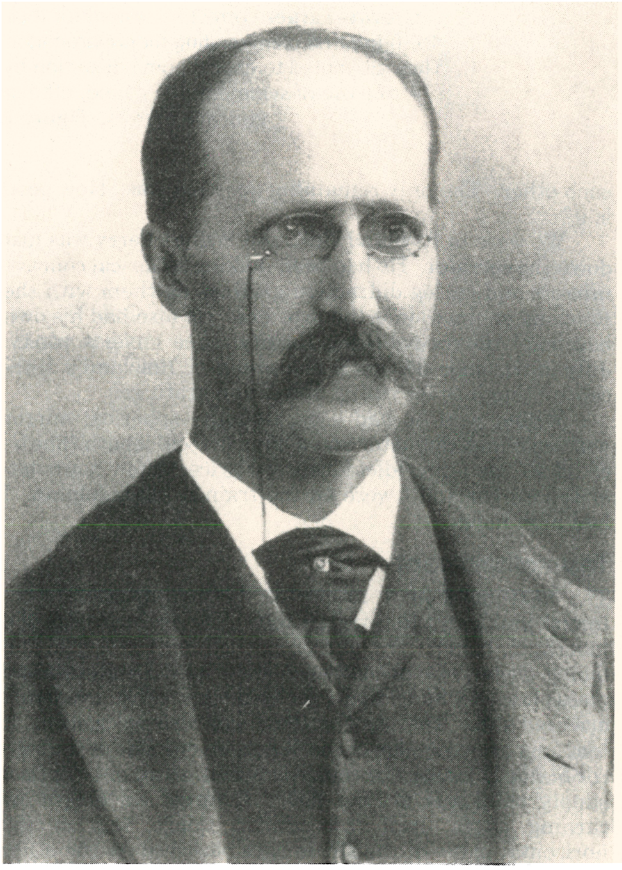
In attempting to calculate the wavelength error along the circularly curved slits, I used the grating equation

$$n\lambda = a(\sin\alpha \pm \sin\beta)$$

where λ is the wavelength, n is the spectral order, and α and β are the angles of incidence and refraction with respect to the grating normal. The arithmetic was messy, so I made a coordinate-axis shift to the bisector of the incident and diffracted ray, which was, of course, the central axis of the spherical mirror. The new equation was

$$n\lambda = 2\mathrm{a}\sin\theta\cos\varphi \qquad (2)$$

where θ is the angle between the grating normal and the central axis of the spherical mirror and φ is the half angle between the incident and diffracted rays. Nothing new or brilliant here, but *voila*! With circular slits centered about the central axis of the spherical mirror, the angle φ is constant at all points on the slits. The wavelength error $\Delta\lambda$ is therefore zero along the slit no matter what the wavelength. No other slit curvature can produce that result.



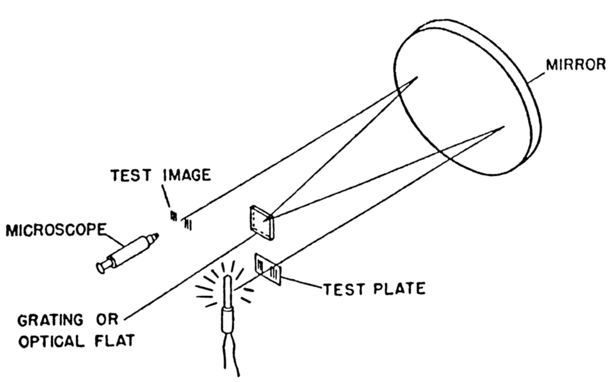
Henry A. Rowland (1848–1901). **Figure 4**

I said earlier that the curved-slit idea was an error: If I had been as smart as I thought I was, I would have derived equation 2 earlier and designed the system with curved slits in the first place, so that the astigmatism would never have shown up. Whereas the removal of astigmatism by circularly curved slits is a simple consequence of geometry, the reduction of wavelength error by such slits is a diffractive effect. The coincidence suggests that I probably have a guardian angel.

To this day I am astounded that this simple analysis had escaped all the spectroscopic instrumentalists who were searching so hard for means to increase the throughput of their instruments. Perhaps the solution seems trivial because it is now so obvious. The geometry of the curved slit system is not unique to the single-mirror system, or to the Czerny–Turner system. The single mirror was the catalyst, however, that led to the slit geometry.

When I showed Strong the analysis, he was both flabbergasted and ecstatic. He said one final word: "Publish." I had been preparing a manuscript for some time, and now I knew I had something really unique. But I was still concerned about whether there might have been a prior publication of a single-mirror spectroscopic system.

I kept asking spectroscopists, "Have you ever seen this before?" Not Wood, nor Pfund nor Dieke. Not Strong nor Franco Rasetti nor Fritz Zernike. Not Sanderson nor Richard Tousey nor Ed Hulburt. Not William Meggers nor George Harrison nor David Rank. Not Shirleigh Silverman nor David Richardson nor Walter Baird. Obviously not Czerny or Turner. Not nobody.



My first experimental attempt at a single mirror spectrometer setup in 1948. The entrance slit is replaced by a resolving-power chart, and the exit slit is replaced by a microscope. **Figure 5**

One day I was talking to Claud Rupert, who was completing his Ph D under Strong. His hobby was reading early scientific papers in optics. I asked him my loaded question and he said he had a vague memory of a single-mirror spectrograph. A week later, he brought me a copy of Kayser's old *Handbuch*, and I learned that I had reinvented the Ebert spectrometer.

I cannot remember any feeling of disappointment, partly, perhaps, because I had narrowly escaped a charge of plagiarism, but also because I knew that the curved-slit geometry was a more important contribution. Claud Rupert went on to a distinguished career as a biophysicist, but his skill as a science historian remains strong in my memory.

In 1952 I published a paper in the *Journal of the Optical Society of America* entitled "A Small Plane Grating Spectrophotometer."[4] My second paper in the same issue, "Image Forming Properties of the Ebert Spectrometer,"[5] presented equations 1 and 2, and showed experimental data that demonstrated nearly diffraction-limited spectral resolution in the visible region. The throughput of the instrument was about ten times greater than one could get at such resolution with short, straight slits or with Randall's variable-curvature slit. I was flying very high. This tenfold improvement meant that one could scan a spectrum in one tenth the time.

Soon thereafter, Strong suggested that I find independent funding for my work. This may sound like I was being thrown off the project, but I interpreted it for exactly what it was—a high compliment. Dieke, who had become physics department chairman, was very helpful in finding funding. He wanted vacuum-near-infrared and far-ultraviolet Ebert spectrometers for his research in atomic and molecular spectroscopy. He gave me the run of his laboratory.

With Per Gloersen, one of Dieke's students, I designed and built the vacuum-near-infrared instrument.[6] (See the spectrum in figure 6.) Crosswhite and I developed some extremely precise curved slits.[7] I built several variants of the Czerny–Turner system that were applicable to photographic spectroscopy. I became a consultant to the Jarrell–Ash company. Many of these instruments found their way into the Jarrell–Ash commercial line of spectroscopic instruments. I also became a consultant to Los Alamos on the development of a very high resolution spectrograph that also became a part of the Jarrell–Ash line. There was and is a considerable market for these devices. They have played a significant part in the widespread search for laser materials.

In the early days I placed the center of the grating surface at a distance from the slit plane of about 1/5 the focal length, just so I'd be able to tilt the grating freely. This design feature stuck. Only later did I realize that this configuration produced a flat focal plane, with all points on the slits in a plane perpendicular to the mirror axis. That made it possible to design a slit pair whose inner jaws were a solid circular plate, with a flexible ring forming variable outer jaws. My guardian angel must have been working overtime.

On two occasions in those days I was asked to referee long manuscripts that sought to prove, analytically and by ray tracing, that the Ebert spectrometer with curved slits worked very well. My short response to the editors was to explain equations 1 and 2 above, followed by *Q E D*. Neither paper was published.

Spectrometers in space

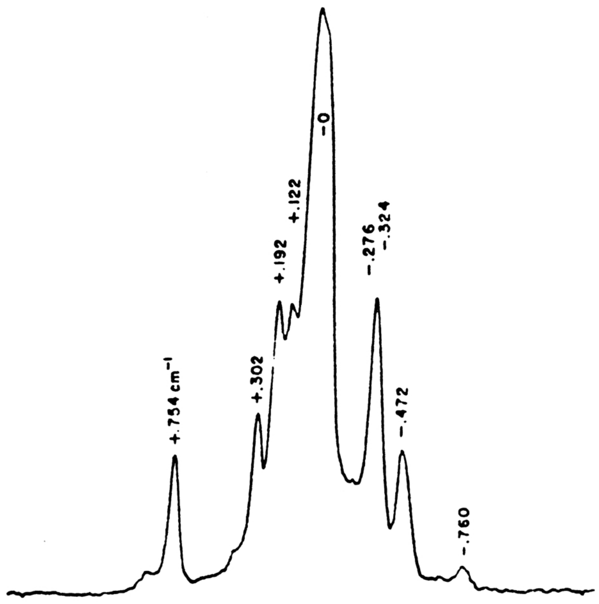
And then there was Sputnik. A few days after its launch in the fall of 1957, I was standing with my late wife and our three children in the evening twilight on a hill behind our home in Owings Mills, Maryland, to see it go almost directly overhead. The kids were too young to appreciate it, but I was awestruck. I knew that a new age had begun and that I had in the palm of my hand an instrument that was intended for space research. It was a rugged, folded optical system, easy to assemble and adjust. Its spectral resolution was close to the theoretical limit, and it had a very high throughput.

I was forty years old and had spent my entire professional life as a laboratory researcher and instrumentalist. Since seeing Sputnik in the twilight sky, I have done virtually nothing else but space research and space instrumentation.

The first Aerobee rocket flight of an Ebert system was launched in February of 1960 from the Fort Churchill rocket range in Manitoba. Figure 7 shows the spectra we obtained from a very bright aurora throughout the wavelength range from 120 to 300 nanometers.[8] Because auroral spectra at these wavelengths had never been obtained before, the results were very exciting. When I returned from Fort Churchill, I showed the spectra to Richard Tousey at the Naval Research Laboratory, an old friend and a pioneer of solar spectroscopy with rockets. He urged me to present the results to space science meetings in Copenhagen and Helsinki that were only a few months away. I told him that I hadn't registered and that I didn't think I could make arrangements or analyze the data in time. He said that he would make the arrangements, and that the analysis didn't matter; the data would speak for themselves. I stopped protesting.

Tousey was absolutely right. The international community of planetary-atmosphere scientists, most of whom I did not know, came to the meetings and excitedly analyzed the data even as the slides were being projected. I met Charles Barth for the first time. He was then at the Jet Propulsion Laboratory, having recently finished his Ph D at UCLA under the ultraviolet spectroscopist Joseph Kaplan, a Hopkins Ph D. Barth was involved in science planning and instrumentation for interplanetary missions to study planetary atmospheres. He invited me to join that effort. He also pointed out that a broader program of far-ultraviolet studies of the Earth's upper atmosphere was needed, including airglow and auroral studies.

The results of these discussions were that I became a consultant to JPL and expanded the scope of our rocket studies at Johns Hopkins. The JPL management had



Resolution test of the Ebert spectrometer in the 11th diffraction order of the 5461-Å green line of mercury.[6] The spectral resolution is essentially at the diffraction limit. Numbers are wavenumber differences from the central peak. **Figure 6**

subcontracted the design and construction of a prototype ultraviolet spectrometer to a large space-industry firm, which came up with a Czerny–Turner system. With only mild persuasion, Barth and I were able to convince JPL to authorize the subcontractor to design and produce a prototype Ebert system.

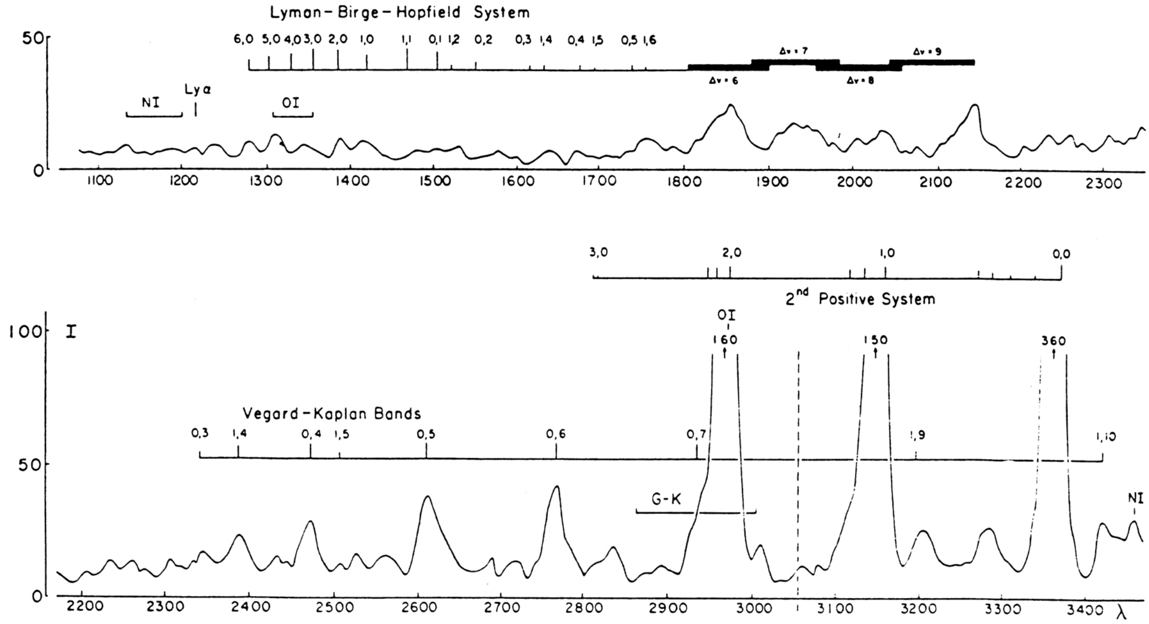
Curtsy to a harlot

When I arrived at JPL for the unveiling of the subcontractor's prototype, I found a large contingent of JPL management in attendance. In the lab where the model was to be demonstrated, a sign read "No Smoking—Space Instrument Under Test." After about 90 minutes of vigorously complaining about almost every detail of the design and construction, I lit up a cigar, blew a cloud of smoke at the monstrosity and said, "Asking me not to smoke in the presence of that piece of junk is like asking the Queen of England to curtsy to a harlot." Then I walked out.

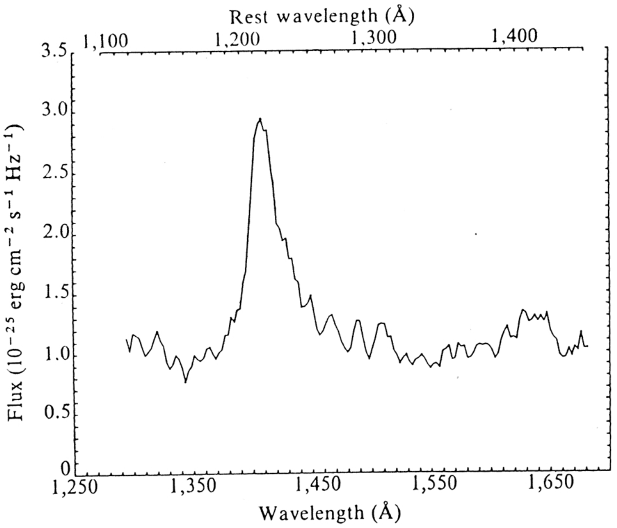
The next morning I was invited to meet with a higher-ranking contingent of JPL management. I thought it was my farewell party. I was shocked to find that they agreed with me, to a man, and that they had already taken steps to have the subcontractor start over again, this time with closer oversight and liaisson. I thought they were throwing good money after bad, and I said so.

To my surprise and relief, the second prototype turned out to be a very fine piece of work. But I am still troubled that a large organization like JPL could have teamed up with a large corporation to produce a space package that had no relation to the requirements or objectives of the scientists, and that clearly demonstrated a Neanderthal design capability. It reminded me of Kayser. As Yogi Berra would say, it was *deja vu* all over again.

The Johns Hopkins program expanded in many directions. John Doering, a young photochemist in the chemistry department, became involved. He was an expert at designing electron spectrometers that could measure the low energies from photo-ionization reactions in the upper atmosphere. Warren Moos, a young physics professor, joined the program in 1965. Two years later Paul Feldman and Richard Henry arrived from the Naval Research Laboratory's space program, which was headed



Auroral spectra, captured for the first time in the far ultraviolet by Crosswhite, Zipf and Fastie[8] in 1960. These data come from the first flight of an Ebert spectrometer aboard an Aerobee rocket. The horizontal axis is labelled in wavelength. **Figure 7**



Quasar ultraviolet spectrum, measured by Davidsen, Hartig and Fastie[17] in 1977, marks the end of an era. With the advent of multi-element detectors in the 1970s, Davidsen never got to use an Ebert spectrometer. The strong peak is the Lyman α line of hydrogen, red shifted by 16% from its proper 1216-Å wavelenght due to the recession of the quasar, QSO 3C273. **Figure 8**

by Herbert Friedman, a 1940 Hopkins Ph D. Another faculty member to join what had become known as the astrophysics group was Arthur Davidsen, who finished his Ph D at Berkeley in 1975. But Davidsen is another story, about which I will have more to say.

We also established a joint rocket research program with Thomas Donahue and Edward Zipf at the University of Pittsburgh. They had both been students of Dieke at Johns Hopkins.

In 1965 Barth moved to Boulder to head the University of Colorado's new Laboratory for Atmospheric and Space Physics. Barth established a sounding-rocket program and an interplanetary program, both of which made use of Ebert spectrometers. I joined Barth to become a co-experimenter on the Mariner 5 flyby mission to Venus[9] and the Mariner 6 and 7 flyby missions to Mars. Later Barth was the principal investigator on several Ebert spectrometer experiments that orbited Mars.[10] He also participated in an orbital mission to Venus and the mission now on its way to orbit Jupiter.

Donahue and Barth were co-experimenters on an Ebert spectrometer experiment that orbited the moon aboard Apollo 17, for which I was the principal investigator. We were searching for a lunar atmosphere[11] and measuring the moon's ultraviolet albedo.[12] A modified spare Ebert spectrometer from Apollo 17 was subsequently flow on the Apollo–Soyuz mission (with Donahue as principal investigator) to measure the atomic oxygen density at high Earth altitudes. The Apollo and Apollo–Soyuz missions were supported by the Johns Hopkins University Applied Physics Laboratory. We also had a cooperative program with NRL, involving Ebert spectrometers aboard rockets and orbiting satellites.

Several factors made these various alliances so successful. We hired each others Ph Ds as post-doctoral fellows, research associates and faculty members; we coordinated our rocket experiments, interchanged results and traded detailed descriptions of our evolving instrumentation. There were, admittedly, a lot of old school ties, but the "good ole boy" system worked very well.

In the Johns Hopkins ultraviolet rocket program, we continued the auroral investigations and studied the Earth's night and day airglow.[13] We recorded the spectra of Venus and Jupiter,[14] obtained spectra from Halley and other comets[15] and studied the intergalactic ultraviolet background.[16] Davidsen[17] obtained the quasar spectrum shown in figure 8. But then Davidsen is, as I said, another story.

Davidsen's quasar spectrum led to the development of the Hopkins Ultraviolet Telescope, which flew aboard the shuttle Columbia last month. Davidsen doesn't really belong to our story because by 1975, when he arrived at Johns Hopkins, the evolution of multi-element ultraviolet detectors had added a new dimension to space research. New spectrograph designs were required. I am distressed that Art Davidsen has never used an Ebert spectrometer. But I suppose that's life—and progress. It may well be that the University of Colorado's Ebert spectrometer now on the way to a Jovian orbit is the last of the breed.

These recollections of the last six decades have involved a much larger galaxy of names than I have been able to mention here. I will end by repeating one name: John A. Sanderson. He got me started.

Curriculum vitae of the Ebert Spectrometer

Born	1889
Crucified, died and buried	1900
Resurrected	1948
Ascended to Heaven on the arms of an Aerobee rocket	1960
Reburied	?

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This article is based on a talk delivered last May by the author at the University of Colorado.

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