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Origin of the Electron-Bombardment Ion Thruster

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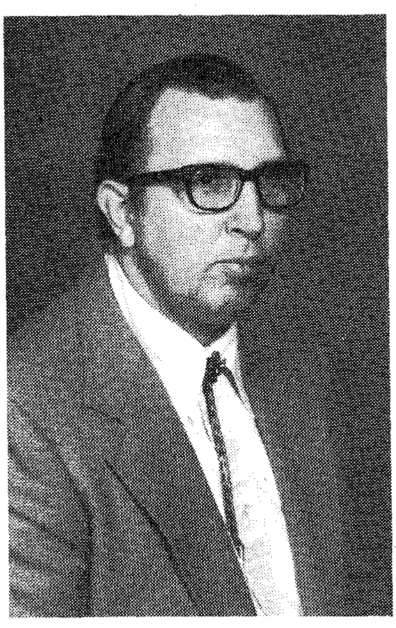
DURING June 1960, I operated the first broad-beam electron-bombardment ion thruster, shown in Fig. 1. Most broad-beam ion sources presently in use can be traced back to this thruster.[1] This includes sources used for cleaning and etching microcircuits, sputter deposition of thin films, and fusion injection, as well as the original application of space propulsion.[2-8] For propulsion, NASA is presently considering a solar-electric stage with about eight thrusters of the type shown in Fig. 2,[9] powered by solar arrays. Because of the higher exhaust velocities obtainable with electric thrusters, much less propellant need be carried along, leaving more mass for payload.

The early development of the electron-bombardment thruster was in many respects an individual matter, but, at the same time, it took place the way most modern technical developments do, within a large organization. In this case the large organization was the NASA Lewis Research Center in Cleveland, Ohio.

In October 1958, I resigned a first-level supervisory position in a propulsion analysis group at Lewis. I had transferred into that group from an experimental work area, also at Lewis, becoming a supervisor at the same time. But the repetitive calculation of different propulsion cycles proved to be a poor exchange for the rich interplay of theory and experiment that can take place with actual hardware. So, after much soul searching, I decided that the power and prestige of a supervisory position weren't important if I didn't enjoy the work.

So, after resigning the supervisory position, I joined an experimentally oriented group at Lewis under Howard Childs. This group was working on the problem of using electrical energy to accelerate propellant mass, thereby producing thrust. Researchers had proposed such electric rockets or thrusters for many years, but in 1958, the practical application of the concept was still quite new.

About mid-1959, after some small preliminary jobs, Haward Childs asked me to look at the possibility of using a duoplasmatron as an electric thruster. Thrusters were, and are, divided into electrostatic and plasma types. In the electrostatic type, ions are accelerated by the electric field between two electrodes or grids. The electrons removed in the production of ions are then added back to the ion beam after acceleration. In the plasma type, a mixture of electrons and ions called a plasma is accelerated by a combination of electric and magnetic fields, roughly similar to the way a rotor is turned in an induction electric motor. Because the electrons are never separated from the ions in a plasma thruster, there is no need to add them after acceleration to "neutralize" the



Harold R. Kaufman was born in Iowa. He received his B.S. degree from Northwestern University and his Ph.D. from Colorado State University, both in Mechanical Engineering. He served two years in the U.S. Navy during and after World War II. He joined NASA in 1951 as a research engineer where, until 1958, he was active in research and development of air-breathing propulsion for the National Advisory Committee for Aeronautics in Cleveland, Ohio. From 1958 until 1974, he continued at its successor, the Lewis Research Center of the National Aeronautics and Space Administration, in the research and development of space propulsion—primarily electric—where he rose to the rank of Assistant Chief of the Spacecraft and Technology Division, a research and development division of about 100 professionals. In 1960, he originated the electron-bombardment ion thruster and has continued as a foremost expert on broad-beam electron-bombardment ion sources until the present. From 1970 until 1972, he served as coordinator of the Lewis Advanced Study Program, which consists of about 15 graduate level courses given each year by Lewis personnel. He has also served for a period as Acting Electric Propulsion Technology Manager at NASA headquarters in Washington, D.C. In 1974, he joined Colorado State University as Professor of Physics and Mechanical Engineering, where, since 1979, he has been Chairman of the Physics Department. He has initiated a graduate research program in the application of electrical-bombardment ion sources for both space and terrestrial applications, such as the development of semiconductor devices. In establishing this research program, Professor Kaufman has combined his considerable experience in research and development with an understanding of basic physical principles, resulting in a sound interdisciplinary program. He has authored more than 70 publications. Professor Kaufman is a member of Pi Tau Sigma and Tau Beta Pi, as well as an Associate Fellow of AIAA. He received both the James H. Wyld Propulsion Award of AIAA and the NASA Medal for Exceptional Scientific Achievement.

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ions. In both types of thrusters the thrust is generated by the expulsion of propellant mass, similar to the thrust generation in a conventional chemical rocket. Unlike a chemical rocket, though, the energy for the expulsion of propellant comes from a separate power source. Both nuclear and solar-cell power sources have been, and are, of interest, but the solar-cell approach is of more interest for immediate space propulsion applications.

Howard Childs was concerned that all of the electrostatic thrusters being worked on in his group were of a single type, in which cesium atoms are ionized by contacting hot tungsten. This was an attractive ion thruster concept, suggested by Ernst Stuhlinger in an early electric propulsion analysis. In practice, though, heat losses and fabrication problems were serious. Howard felt some other electrostatic thruster concepts should be considered, and the duoplasmatron appeared to be a promising candidate.

The outcome of this assignment was nearly a year of intense study and analysis, made more tolerable by the possibility of some new thruster concept being built as the result of it. The first thing I found was a serious shortcoming in the duoplasmatron acceleration process. For acceleration, ions effectively originate at a hole in one electrode and are accelerated through a hole in an adjacent negative electrode.

Today, we would use digital computer programs to determine the detailed motion of ions through the acceleration process. Such programs were not available in 1959, and the electrolytic tank analog that was available elsewhere was expensive and tedious to use. A mathematical solution for the idealized acceleration of charged particles between parallel planes had been available for about fifty years in the form of Child's Equation. (No relation to Howard Childs.) Child's Equation was useful for rough approximations by ignoring the effect of holes in the electrodes on the electric fields between them. For more specific two-dimensional configurations, a rubber-sheet analog could be used for a partial simulation.

A rubber-sheet analog was constructed in the NASA model shop, at my request. Such a device can be quite fascinating to play with. The height is assumed proportional to electric potential, so that boundary conditions are applied by using blocks of the correct height, above and below the rubber sheet. If the angles relative to the horizontal are small, the height of the rubber sheet is everywhere proportional to potential. A ball, rolling down the sheet, will closely follow the path of a charged particle in the electric field being simulated. There are, however, two major limitations of this device. As mentioned above, it simulates two-dimensional configurations. Also, there is no simple way to include the mutual repulsion of the charged particles being accelerated. In more technical terms, the latter means that Laplace's equation can be simulated, but not Poisson's. It should also be evident that the rubber-sheet analog is generally limited to results that are not accurate enough to be worth publishing. It was, however, an excellent means of obtaining a "feel" for the electrostatic-acceleration process, particularly for someone like myself with a previous background in airbreathing propulsion.

The current drawn from a duoplasmatron can usually be increased by increasing the diameter of the holes through which ions are drawn. My playing around with the rubber-sheet analog, though, convinced me that the practical limit for this increase corresponded to a hole diameter roughly equal to the spacing between electrodes. The duoplasmatron was a single-aperture device; that is, one hold in each of the electrodes used to extract and accelerate the ions. A simple analysis using Child's Equation showed that the maximum current through a single aperture was a function only of the extraction voltage, if the hole diameter were varied proportionately with the electrode spacing. Further, the magnitude of this current was only a small fraction of what was needed for a propulsion device.

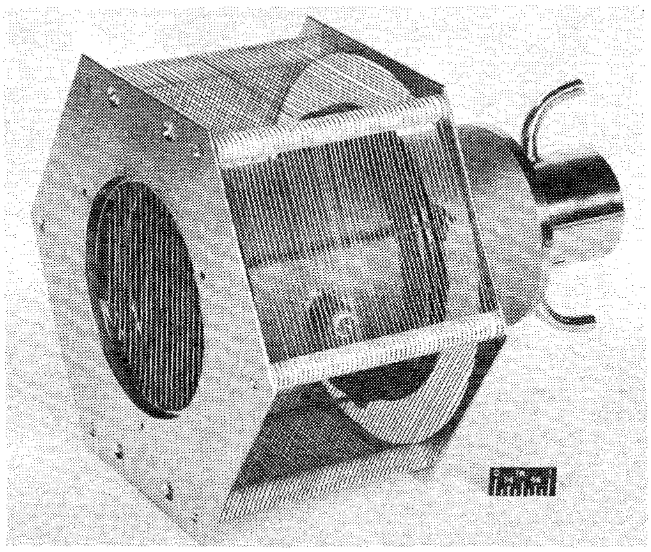

Fig. 1 First electron-bombardment ion thruster, tested in 1960.

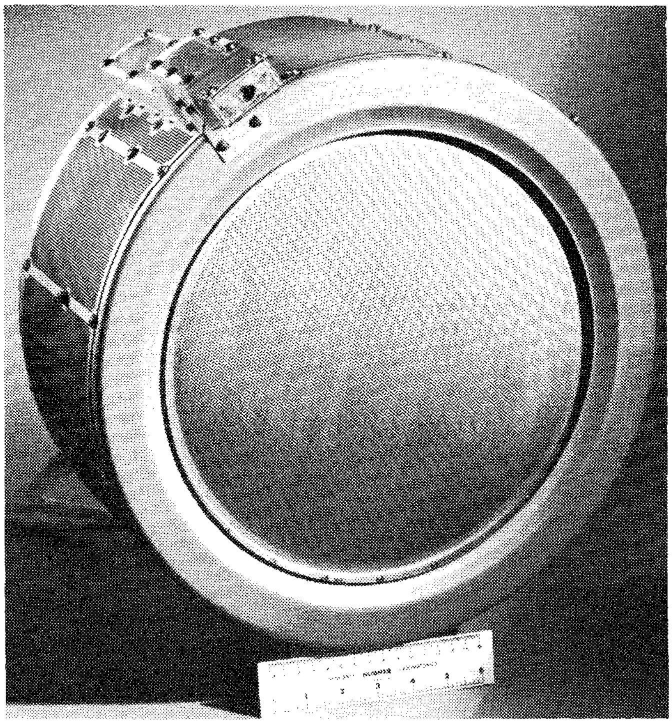

Fig. 2 Current 30-cm electron-bombardment thruster.

With this background in mind, the obvious solution was to use electrodes with many small apertures accelerating ions in parallel. In the state-of-the-art thruster shown in Fig. 2, there are roughly 15,000 of these apertures operating in parallel.

The next major problem was the production of ions. The duoplasmatron had been used to generate substantial ion currents, but only at voltages (and exhaust velocities) too high for electric propulsion.* For the voltages of interest, this ion current was too concentrated to match well with any practical multiaperture accelerating approach. The obvious next step was to make a parametric study of the electron-bombardment process for generating ions. The

*You may wonder why there is a limit to the exhaust velocity of interest in electric propulsion, since increasing exhaust velocity will always reduce the propellant mass required for a given mission. The problem is in the electric power required. Above a certain exhaust velocity in the 10-100 km/s range, the increasing power supply mass will outweigh any savings in propellant mass.

duoplasmatron had been developed almost entirely by the cut-and-try approach, so the analytical tools were not available from literature on that source.

About that time a plasma physics seminar was started by some of the members of Howard Childs' group. It appeared that this seminar might help me with some of the analysis required for the generation of ions. It took a number of weekly meetings before I realized that what I needed was tied up in a conductivity tensor. Further, just one element of this tensor was of interest. And lastly, the seminar was being taught by, and for, theoreticians who had no interest in actually calculating numerical values. Incidentally, some plasma physics courses in universities are still taught in this manner.

After this assessment of the seminar, I dropped out of it and studied *Physics of Fully Ionized Gases* by Lyman Spitzer Jr., instead. By current standards, the theory available was still inadequate for the job at hand. For example, I found no clue in the literature for calculating simultaneous effects of large angle collisions and Coulomb (or small-angle) collisions on electron diffusion. So I simply added cross sections for the two effects. This is mathematically equivalent to the addition of collision frequencies for different processes that we use today. Also, there was little understanding of the "anomalous" or "turbulent" process that today we recognize as being very important in electron diffusion. However, I felt, and still feel, that a rough qualitative analysis is preferable to no theory at all in providing a guide to experimental development.

After 8-9 months of analysis, I was ready to start mechanical design. Following the usual NASA Lewis procedure, I took the job through channels to a design group. With a moderately high priority by Howard Childs, I was directed to a designer with an impressive background in pressure vessel and wind-tunnel design. At this point I should perhaps mention that there are many designers similar to the one I worked with, in many different organizations. The basic problem he faced was to turn out a large quantity of work. In fact, his performance evaluation was closely tied to the construction dollar value of the designs he produced. To be even more specific, if he worked hard to make a design simpler, lighter, and cheaper to build, this could easily have an adverse effect on his evaluation, because it would lower the dollar value of his designs for that time period.

After repeated emphasis of 10^{-4} Torr as the maximum pressure and the need for a lightweight design, followed by a two-month wait, the mechanical design was ready. It had a minimum tubular wall thickness of 0.125 in. and minimum flange thickness of 0.25 in. As a result, the weight was sufficient to make electrical isolation of different parts very difficult.

At this point, I dropped the formal approach and spent several hours with some of the very skilled machinists and sheet metal workers that were employed at NASA Lewis. I found out, for example, that it was fairly easy to fusion weld down to about 0.020-0.030-in. thickness, but difficult with thinner material. I also found out that aluminum oxide was very difficult to machine after firing. And I found out which materials were readily available from stock. In short, I was able to complete a design in about a week using information available from the fabrication group.

After another month or two for the actual fabrication, the thruster shown in Fig. 1 was ready for testing. Because of my redesign, it weighed only a few pounds instead of close to a hundred pounds. It was also fabricated faster and was easier to work with because of this redesign. For the testing, a 5-ft.-diam., 16-tt-long vacuum chamber with a 32 in. oil diffusion pump was made available. Facilities such as that are not too common even now, but were as scarce as the proverbial hen's teeth in 1959. Why NASA Lewis had several such facilities in 1959 is another story, too long to include here. But Howard Childs also had his hand in that, along with people like Bill Mickelsen and Warren Rayle.

There were the usual problems that had to be solved before the thruster would operate at all. Then other problems to be solved, involving the help of others before it would operate well. Then many more problems involving the help of many others before it would operate for a long time. But after the first operation, there was never any real doubt about the electron-bombardment ion thruster being worth further support. In fact, much of my professional work today is directly traceable to that test in 1960.

So what might a young researcher learn from all this, that is still true today? Experience is a very personal thing, but here are some of the conclusions I felt were justified.

1) *Try to use the creativity of the people working for you.* I didn't emphasize it, but Howard Childs made a real effort to enlist my enthusiasm. The evaluation of the duoplasmatron was one of several jobs that he felt needed doing. It would have been easy to select any one of those jobs and direct me to work on it. Instead, he was interested in a deeper, more creative commitment from me, and made it clear that I should work on something of interest to me.

2) *Reinterpret the assignment as the need becomes evident.* The thruster tested bore little resemblance to the original duoplasmatron studied. Naturally, I discussed this reinterpretation frequently with my bosses. Managers always dislike surprises, even when they are pleasant. Blindly following the original job assignment probably would have resulted in nothing memorable twenty years later.

3) *Use a level of theory appropriate to the job at hand.* I used a rubber-sheet analog for the ion optics and made crude assumptions to analyze the discharge-chamber plasma. Little of what I did was of a quality adequate for publication, but it was quite adequate for the design of a new type of electric thruster. Don't underestimate the value of crude models and theories, especially in a new field.

4) *Use the facilities of the organization around you, but don't be limited by them.* The wood-model shop at NASA quickly provided me with the rubber-sheet analog which was important for my education. Members of the fabrication division gave me valuable design information. The testing would have been extremely difficult without the very advanced vacuum facility placed at my disposal. But I was also able to ignore the misdirection of the plasma physics seminar and to avoid the use of an inappropriate mechanical design.

5) *Learn to make your own evaluations and decisions.* This is really the heart of the matter, and includes most of the previous nuggets of wisdom. I have gradually become convinced that man is, by nature, a herd animal, and likes reassurance from others around him. It is very difficult to decide that a choice of project made for you by your boss is wrong. It is easier to say that he must know what he is doing because the organization made him your boss. Yet research, by its very nature, consists mostly of things that are not immediately obvious. If you wait until your boss and colleagues agree on the proper course of action for you, you have almost certainly waited too long, and someone else did it before you.

Making your own evaluations includes deciding whether or not you should be in management. Too many of us in science accept the Horatio Alger approach of climbing the ladder of success without question. My most important scientific work stems directly from a willingness to relinquish the management role (the supervisory position I resigned in 1958). As it happens, since then I have spent most of my time in various management positions. But I have never let any management position prevent me from personally doing some scientific work. Naturally, one should avoid being in direct scientific competition with a subordinate, but there are ways to do this.

Even if you are fully committed to a mangement role, you may wish to maintain some personal scientific work. There are few things more pitiful in science than a scientific manager that has emphasized management activities to the point of becoming technically incompetent. If you keep up technically, you can always have a trade to fall back on when the management business goes bad.

Acknowledgments

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