Three Astronautical Pioneers—3

Hermann Oberth

from my life

"This is the goal: To make available for life every place where life is possible. To make inhabitable all worlds as yet uninhabitable, and all life purposeful."

—Hermann Oberth

Background

Any series of articles devoted to the pioneers of astronautics would be incomplete without reference to Hermann Oberth, whose slim pamphlet, "Die Rakete zu den Planetenräumen" ("The Rocket into Interplanetary Space"), published in 1923, fired the imagination of the early European rocket experimenters and was directly responsible for the formation of the German Rocket Society in 1927 and indirectly, a few years later, of the American Rocket Society.

This autobiography of Prof. Oberth, now living in Feucht, near Nuremberg, Germany, was specially written for Astronautics and, like the previously unpublished autobiographies of two other great astronautical pioneers—the American, Robert H. Goddard, and the Russian, Konstantin Tsiolkovskii—found in the April and May issues, offers an insight into the motivating forces which led to his interest in spaceflight.

A full-scale autobiography of Prof. Oberth was published in Germany last year, and is expected to be available in an English translation in the not-too-distant future.

While the name of Hermann Oberth has already won enduring fame, the work begun by Prof. Oberth some 40 years ago is being continued by a second-generation Oberth, his son Adolph, who recently joined Aerojet's Solid Rocket Plant in Sacramento, where he is engaged in research work on high energy propellants.

I WAS born on June 25, 1894, in the town of Hermannstadt in Transylvania, which, formerly an independent country, was part of Hungary at that time. My father, since 1893 an assistant surgeon at the Franz Joseph-Buergerspital (i.e., the Municipal Hospital of Hermannstadt), in 1896 became director and chief surgeon in charge of the County Hospital in Schässburg, Transylvania.

At that time, this was still a somewhat patriarchal town of some 11,000 inhabitants. It wasn't until 1902 that Schässburg acquired water supply mains and electric lights. In 1904, the telephone became available, with only 37 subscribers reported to the telephone service in the first three years. Even so late as the 1930's the "Burg" —the ancient part of the town, situated on a hill and surrounded by a wall—was without a sewer installation. Cesspools in the houses were emptied by gypsies and discharged into the river "Kockel" flowing past the town. The uncovered outlets from the sinks ran into the narrow streets, which were paved with round headstones. A rather bad smell spread over the town in summer when there had been no rain for extended periods.

The hospital and our home were situated somewhat outside the town on Michael Albert Street, which formed part of the highway leading from Budapest through Kronstadt to Bucharest. Our home, adjacent to the hospital, had its sewer installation, and was thus in a more comfortable condition. Our street was built like a causeway, and macadamized but not asphalted or tarred. In summertime the road was dusty and turned boggy when it rained, so that it would hardly have met the requirements of modern motor car traffic. Anyway, at that time it was used only by carts and carriages drawn by oxen or horses and later cyclists when bicycles made their first appearance. It was in 1904 when I saw the first automobile. Because of the dust, my father then acquired a cottage bordering a wood, called the Siechhofwald. From there we had a wonderful sight of our town, which was beautifully situated.

It was the railway station at the foot of the hill which interested me particularly. The railway station was about the only thing linking our town with the big world of industry and engineering—apart from six small local factories, of which our citizens had been rather proud. The biggest of them employed 150 and the others from 10 to 60 workmen.

Camels are said to discover new sources of water when they are thirsty. Maybe something similar happened to me, for I had been eager for engineering and progress. I had been particularly interested in anything moving and not requiring it be driven by men or drawn by animals—a means by which one could travel along, the quicker, the better. I designed fantastic projects for locomotives, airplanes, and spaceships. At the age of eleven, I received from my mother as a gift the famous books, "From the Earth to the Moon" and "Travel to the Moon" by Jules Verne, which I had read at least five or six times and, finally, knew by heart.

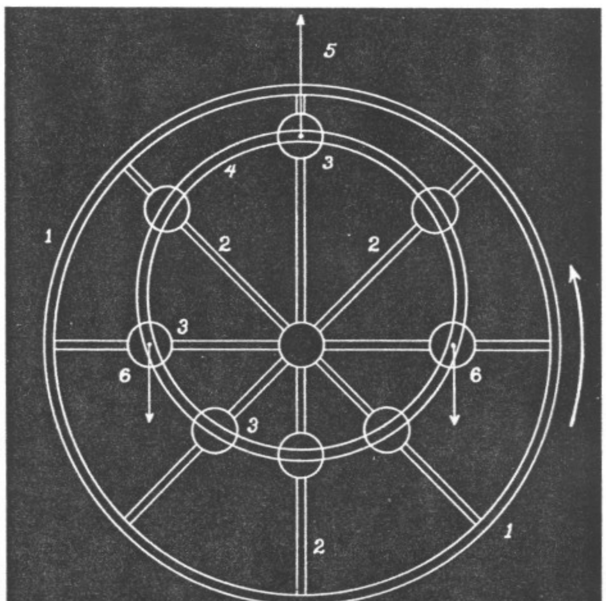
Realized Verne Was Wrong

Even though I had taken a lot of interest in these stories, I realized already at the age of twelve that shooting a missile out of a giant gun with the travelers inside, as Verne imagined for spaceflight, would not work. Even if it would have been technically feasible to produce such a gun, the travelers inside the missile would have been crushed without pity by the enormous acceleration.

With my knowledge in mathematics, still rather deficient at that time, I was able, however, to find out so much at any rate, that the escape velocity of 11,000 m/sec (about 25,000 mph) was roughly correct. At school we had learned the laws of gravity, so that I could derive by myself at least the formula $v = \sqrt{v_o + 2\,gh}$. Furthermore, our young and clever teacher in physics, Ludwig Fabini, managed to make us understand that the attraction of gravity exerted by the earth would decrease in proportion to the square of the distance from the earth center. So I divided the trajectory of the missile up to the moon into sections—smaller ones down toward the earth and larger ones up toward the moon—assuming for the gravitational acceleration in each section a mean value. For the first two sections I took the final velocity of the preceding one as the initial velocity of the next one, and finally I discovered that for the rest it was for me simpler to calculate $v = \sqrt{2g_1\,h_1 + 2g_2\,h_2 + 2g_3\,h_3 + \ldots}$ so that I had to extract the square root only once.

Seeing that a gun did not work, I set to thinking on other solutions or (CONTINUED ON PAGE 100)

This simple and austere cover lent ironic understatement to Hermann Oberth's history-making study of rocketry and spaceflight, "Die Rakete zu den Planetenraumen."



Hermann Oberth, spurred by his boyhood dream of spaceflight, designed this "levitating" wheel as a young teenager, but soon rejected the notion of force unbalance which led him to suppose it would rise. As he humorously remarks, "Every year I am approached by eight or ten inventors with what amounts to the same scheme."

Hermann Oberth, from my life

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means of giving a vehicle the velocity of 25,000 mph and lifting it out of the earth's gravity field.

At first I thought of a long tunnel lined inside with artificial ice. This tunnel was supposed to be nearly void of air and inside a powerful magnet was to run on skid runners. Round the tunnel was to be wound an electric coil in the fields of which the magnet was to be attracted in front and repulsed in the rear. Later, at the age of 14, I rejected this proposition, for I had calculated that the weight profile of the magnet would have to be at least 14 tons per square inch. Besides, for such a magnet, the tunnel was required to have a length of 10,000 km, since the acceleration then would be very small.

My next project was an airplane equipped with airscrews made of silk. Water trickling through the airscrews was supposed to cool them. This, if only for reasons of weight of coolant, was another failure!

Then I thought of a wheel, as sketched on page 39, with heavy weights (3) sliding to and fro and to on its spokes (2), the weights being guided by a fixed eccentric ring (4), so that they would be at a distance from the center on the one side and would come close to the center on the opposite side. In this way, I thought, a centrifugal force would develop on the side where the weights were at a greater distance from the axis, and lift the wheel.

I soon rejected this plan also, although I was not quite 14 years old and had not been told about a thing called "Force of Coriolis." I was nevertheless aware that in the points at (6) two forces, pushing exactly at the same amount downward as the excessive centrifugal force pulling upward, would appear, and that such a wheel never would rise. Every year I am approached by eight or ten inventors with what amounts to this scheme.

From the very beginning in these childhood projects, I always had in my mind the rockets designed by Jules Verne for braking the gravitational pull toward the moon and for changing direction of travel in spaee. I gradually realized that reaction propulsion actually offered the only means of achieving space travel and that giant rockets would be used as spaceships of the future, even if they lost in appearance any resemblance to our fireworks, as it is the case with the electric spaceship designed by myself.

I should tell a lie in stating that I was delighted at this discovery. I was not pleased at all with the enormous fuel consumption, the hazards of rockets containing solid fuels, the difficulty of handling liquid fuels, the high costs of the chemicals, etc.

I was not able to carry out any experimental work for a long time. It might well have been possible for me to produce and launch some rockets containing gunpowder. But such attempts appeared to me silly tricks compared to what I wanted to achieve.

There was, however, one thing I was able to do, and which offered me some satisfaction—inquire theoretically into the working principles underlying the operation of rockets, into the forces affecting their operation, and into the ways and means of producing improved and more efficient rockets.

At first, all this was nothing but a hobby for me, like catching butterflies or collecting stamps for other people, with the only difference that I was engaged in rocket development. In this way I had made headway into an entirely unknown province, never seriously approached by anybody before me. The fact that in 1919 Goddard in his paper "A Method of Reaching Extreme Altitudes" had expressly stated that no analytical formulas could be developed for establishing the most favorable rocket speed as well as the interdependence between fuel consumption and potential speed, among other remarks, revealed that I had carried on my investigations completely independent of Goddard's work.

Proof Published in 1923

A proof for this statement is my paper published in 1923 under the title "Die Rakete zu den Planetenräumen" ("The Rocket into the Interplanetary Space"), in which I set out by establishing the necessary formulas and then deriving therefrom everything I had to say about rocket development by using liquid fuels, the transportation of human passengers, indispensable preliminary investigations, etc.

When new ideas first come forward, standard science is always opposed to them in principle, particularly in Germany. Let me comment here on my schooling, which perhaps helped me to this opinion, as well as experiences in my later life.

Like most intelligent and lively boys, I hated going to school, especially since in our town there was only a high school, where such subjects as Latin and Greek were taught and, on top of it, the pupils received their education in a rather conservative spirit, not at all intended for the future. Our school compared to a motor car that, in driving along a dark street, had directed its headlights backward and not forward. So, for instance, it happened still in the twelfth (last) grade that my German compositions were returned to me for recopying as a punishment. My writing was quite legible, except that I sometimes corrected my mistakes, considering I was not born to leave my mistakes uncorrected, once I had discovered them! All this happened in the years between 1900 and 1912, when any shopkeeper or lawyer of a certain standing and with a certain self-respect could already afford a typewriter. So any teacher who was then able to read the signs of the time would have realized that, later on, the youngsters in any walk of life would earn money enough also to afford a typewriter—at an expense that would have amounted to a fraction of those pains caused to them out of pedantry and backwardness, which perhaps was also mixed with a dash of cruelty.

Yet, our backward school system also did me something good. I became quite immune against the sayings of so-called authorities who were of a different opinion from my own. It was quite impossible that somebody could succeed with me in producing such arguments as, "Yet, the Privy Councilor so-and-so states that you are wrong" or "This is in contradiction to accepted scientific standards." But he who wanted to argue me out of my opinion, first had to prove that I was wrong. And this ability of mine would come in handy in the next 20 or 30 years!

[Ed.–Here, in a rather lengthy aside, Professor Oberth decries redundancy of research caused by poor intergroup communication, the lack of central abstracting agencies both here and abroad, and the imagination-sapping institutional pressure to get the paper out that keeps a scientist today "in the same position as a stuffed goose facing her food."]

In 1917, I submitted to the then German War Department a project for a long range missile with alcohol containing water and liquid air as propellants. It was to be controlled by rudders operated from a gyroscope through servomotors. The control of cutoff velocity was to be made by a weight pulling an elastic spring and thus generating through a potentiometer an electric current proportional to the acceleration. This current was to be conducted to an ammeter showing the speed, and triggering the closing of the propellant valves at the proper moment. This was the principle underlying the design of modern long range rocket-propelled missiles. In retrospect, we may say today that the lapse of time up to the end of the war and the resources of the Central Powers, then already exhausted, would not have permitted such a development. All the same, my project deserved more attention than it received.

In my accompanying letter, based on my research work, I had stated the reasons why the rockets produced up to that date did not give better performance. In his answer, the competent official simply wrote that, "As known by experience, the rockets would not cover a distance greater than 5 km, nor was it to be expected that they would be substantially improved, considering the notorious Prussian accuracy they had been developed with." What he had done was to read just some passages in the introduction to my paper and did not think it worthwhile to consider whether my statement would prove true and take the lead in solving the rocket problem.

After World War I, I attempted to arouse the interest of German officials in my projects, but in vain. In 1922, I elaborated a paper on the results of my research work conducted up to that time. It was my aim, first, to set forth the theoretical principles underlying rocket development and then to demonstrate the feasibility of space travel. Moreover, I did not want any blame put on me that my ideas would be technically unfeasible. I gave, therefore, some suggestions as to how my plans could be realized, this not so much that I wished to make a point of it, but to demonstrate that there were any practical solutions to the problem at all.

Many Suggestions Adopted

In fact, I am rather surprised that so many of my former suggestions have been adopted in modern rocket development. Among others, only these should be mentioned: Regenerative cooling, reinforcing tanks by internal pressure, liquid fuels such as alcohol containing water and liquid oxygen, directional control by means of elastically suspended weights, speed control by generating an electric current proportional to the acceleration and flowing through an ammeter, cutoff velocity control by closing automatically the fuel valves when reaching the rated speed, and, finally, the use of liquid hydrogen as fuel in upper stages.

In 1922, I handed my paper to the Heidelberg Univ. as a thesis for my doctor's degree, but it was rejected. I refrained from writing another one, thinking to myself: Never mind, I will prove that I am able to become a greater scientist than some of you, even without the title of a doctor. (In the United States I am often addressed as a doctor. I should like to point out, however, that I am not such and shall never think of becoming one.)

In 1923, I finally published my thesis paper under the title, "Die Rakete zu den Planetenräumen," thinking to myself that, in taking this roundabout byway of publicity, scientists might be induced to engage in this problem finally. Today, however, after having had the opportunity of looking closer into the university activities in Germany, I am no longer surprised about the fact that, even after years, none of them had properly read this publication.

In the meantime, however, such writers as Willy Ley, Max Valier, Otto Willy Gail, and Karl August von Laffert engaged in publications of a popular scientific character, carried my ideas into the limelight of public opinion.

In 1929, I was awarded for my second book, published under the title of "Wege zur Raumschiffahrt" ("Means for Space Travel"), the Rep-Hirsch Prize founded by Robert Esnault-Pelterie and André Hirsch. Then, also, the Opel rocket-propelled motor car made its appearance. So scientists were finally obliged to face the problem of rocketry.

In 1924, I moved to Wuerzburg in Germany at the invitation of a banker who wanted to finance my rocket project. However, it turned out that he was awaiting an opinion on the value of this project from a professor at the Berlin Technical Univ. Finally, after six months, the opinion arrived, by which time the money I had saved for experiments had been used for my support.

The professor wrote that my calculations were certainly correct, but, obviously, I had departed from sound fundamental premises. He advised the banker to abandon the project, but did not reveal how my fundamental premises were actually wrong. Nor did he so when I told him about my situation and called his attention in a courteous manner to his irresponsible procedure, since I was financially ruined by his action. Afterward, I was not able to carry on my research work for four years.

In the same year, Privy Councilor Prof. Dr.—a mathematician and authority in teaching mathematics—published a comment on my work in an educational review for mathematics and physics. It consisted in all of three sentences, running as follows: "We are inclined to give credit to the author stating that his rockets would reach cosmic speeds. All the same, it will not be possible to travel around in space because of the enormous wear of material. We think that the time has not yet come to deal with this problem, and probably it will never arrive!"

My retort to the effect that traveling in a rocket had nothing to do with coaching around in space, but rather compared to firing a shot, was not given publicity in that review, because at that time I did not know yet that I could have enforced its publication by virtue of the German Press Law.

Commented on Concepts

The concepts of rocket propulsion and its use in spaceflight continued to receive such worthy comment. In 1924, an astronomer and an engineer-chemist, both well known, expressed doubt that the power of reaction would be effective in vacuum, for there was nothing on what the exhaust gases could thrust. In 1925, the Professor of the Prussian War Academy and Director of the Prussian Rocket Agency, a major at that time, admitted that Newton's principle of reaction was correct. At the same time, however, he doubted that the power of reaction would be effective in vacuum. His first argument was that gases in the vacuum would get very thinned and therefore would lose mass. His second argument stated that it would be impossible to ignite gunpowder in the vacuum. In 1928, a professor, before the war a specialist in ballistics and afterward a professor ordinary, stated that the density of gases from gunpowder would be the lowest behind a rocket rising vertically into space, if the effective upward acceleration was 2 g and the relative acceleration 1 g. The fact that the integral, all the same, could assume a lower value than its integrand, if the limits were dependent on the integrand, and that it would decrease in proportion to the integrand increasing, the fellow had not grasped yet. Besides, he considered the launching of rockets in curved trajectories as pure nonsense, stating that the best solution to the problem would be offered by using thrusts effective always in the same direction. And the staging principle he had not grasped either, for he stated that two rockets would not rise higher than a single one of the same type. Based on his highly scientific calculations, he arrived at mass ratios amounting almost to the square of those actually required. These were almost the only arguments and opinions uttered by German university teachers in the years between 1923 and 1930.

I came to deeper realization that there are mistakes which are made only by people who have learned such a lot that they "cannot see the wood for the trees," as it was put once by Martin Luther.

In 1927, for the first time, I received from a German astronomer more credit. He wrote in a periodical that the statements contained in my paper stood his test wherever he was able to check them. He contested, however, my assertion to the effect that a telescope located in space would have a substantially greater range of visibility than a telescope located on the earth.

But to summarize some of my later activities. In June 1923, I passed an examination entitling me to teach mathematics and physics at Klausenburg, Transylvania. I obtained the title of "Professor Secundar," i.e., secondary schoolteacher. In 1923–24, I was a supplementary teacher at the Schässburg high school, and in 1924–38 a teacher at the Stefan Ludwig Roth School, a high school in Mediasch, Transylvania. During my years of studies in 1918–19, and later in 1929–30, I was granted leave to work for periods on rocketry, mostly in Berlin. There, among other activities, I acted as a scientific adviser for the film "Frau im Mond," produced by Fritz Lang. I also engaged in some experimental work with liquid fuels.

In 1938, I obtained another leave and was invited by the Vienna Technical High School to do some secret research work. (This was the preliminary work for the V-2.) In 1940, I received a similar commission from the Dresden Technical High School. In 1941, I became a German citizen and was commissioned on wartime duty to the German Army's Research and Experimental Center at Peenemünde. In 1943, I accepted an appointment to the W.A.S.A.G. (Westphälisch-Anahltische Sprengstoff–Aktiengesellshaft) and was assigned the task of developing a gunpowder rocket for anti-aircraft defense, for which I had submitted a suggestion. After the defeat of Germany, I was kept in custody by the Allies up to August 1945, and then released to my family living in Feucht, Germany, where I had bought a house in 1943.

From 1945 to 1948, I was a private teacher, a writer on scientific topics for *Mundus*, and occasionally a scientific adviser. In 1948, I went to Switzerland, living there initially by writing and advising. Later on, as a guest of the fireworks manufacturer, Hans Hamberger, I spent some time at Oberried on the Lake of Brienz, where I continued experiments I had formerly started when I was working for W.A.S.A.G. I then brought my research work, which I was able to continue in La Spezia from 1950 to 1953, commissioned there by the Italian Navy, to a certain conclusion.

In 1953, I returned to my home in Fuecht and lived there until 1955. Since March 1954, I am acknowledged as a former civil servant and entitled to an old-age pension. At the beginning of June 1955, I moved to Huntsville, Ala., where I worked at Redstone Arsenal on a secret project. In 1958, I joined ABMA, where I was in charge of a department commissioned mainly with the task of investigating what innovations might be anticipated in the field of rocket development in the next 10 years.

New Radiation Lab Underway

A laboratory for radiation damage experiments upon various materials and fuels, to be ready for operation in early 1960, will be built with Air Force funds at the Connecticut Aircraft Nuclear Engine Laboratory, Middletown, Conn. A Pratt & Whitney aircraft technical team which has been performing similar work at AEC's Livermore, Calif, facility will staff the new lab.