History of
Rocketry and Astronautics

Proceedings of the Twelfth, Thirteenth and Fourteenth History Symposia of the International Academy of Astronautics

Dubrovnik, Yugoslavia, 1978

München, Federal Republic of Germany, 1979

Tokyo, Japan, 1980

Å. Ingemar Skoog, Volume Editor

R. Cargill Hall, Series Editor

AAS History Series, Volume 10

A Supplement to Advances in the Astronautical Sciences

IAA History Symposia, Volume 5

AAS Publications Office
P.O. Box 28130
San Diego, California 92198

Affiliated with the American Association for the Advancement of Science
Member of the International Astronautical Federation

First Printing 1990

ISSN 0730-3564

ISBN 0-87703-329-3 (Hard Cover)
ISBN 0-87703-330-7 (Soft Cover)

Published for the American Astronautical Society
by Univelt, Inc., P.O. Box 28130, San Diego, California 92198

Printed and Bound in the U.S.A.

AAS 90-421

Chapter 21

A MAN OF THE FIRST HOUR - JOHANNES WINKLER*

Rolf Engel†

It is now 20 years since the space age began, an almost incalculable number of satellites and probes have been sent off into space, public interest has long since dropped below the "sensation" level--a satellite launch is at best briefly noted in the press, and even manned flights only rate the third or fourth page in the dailies. On the other hand, growing interest is being shown in historical reviews by many who want to know how all of this happened in such a short time and who the men were who provided the drive to carry the space idea forward.

It is an undisputed fact that Johannes Winkler (1897-1947) had a powerful influence on German rocket technology and space work between 1927 and 1931. The establishment of the Verein für Raumschiffahrt (VfR) in Breslau on 5 July 1927 and publication of the world's first rocket journal *Die Rakete* were due entirely to his initiative. The VfR was not merely a German society, for its members included from the beginning space pioneers from other countries, such as France, Britain, the Soviet Union and the United States. It was Winkler's energy which sparked the formation of associations with the same objectives in other countries. The British Interplanetary Society, Le Groupement d'Astronautique in France, The American Rocket Society and the Gesellschaft für Höhenforschung in Austria were in practice based on the VfR's example. It is to be welcomed on the one hand that Frank H. Winter, of Washington[1] should recently have dealt in great detail with the VfR's early history, but a little saddening on the other hand that no German writer should have concerned himself with the subject. Anyone who is familiar with the literature on space will concede that really very little is known about Johannes Winkler. Apart from his autobiographical contribution to the book entitled *Männer der Rakete* by Werner Brügel[2], one of my colleagues, very little has become known about Winkler's activities after 1929.

This is because from 19 September 1929 to 31 March 1941 he worked at the Hugo Junkers research establishment in Dessau and was naturally bound by the rules of commercial secrecy. On 1 April 1941 his whole field of work was transferred to the Aerodynamische Versuchsanstalt (AVA) in Braunschweig where, as

* Paper presented at the 12th History Symposium of the International Academy of Astronautics, Dubrovnik, Yugoslavia, October, 1978.

† Space Manager (retired) of MBB, München, FRG.

head of a department under Professor Busemann, he was governed by the Air Ministry's official secrets regulations. Many of his writings were destroyed by enemy action. Happily, however, some at least of his personal notes were rescued by his daughter, Mrs. Elisabeth Gruber, who has been kind enough to share them with me. All the quotations from these notes used in the following are identified as "Gruber Archives"[3]. With their help, it is now possible to present a clearer picture of Johannes Winkler, the man and his ideas (Figure 1).

Figure 1 Johannes Winkler (1897-1947)

Detailed historical presentations of many pioneers of space theory are available today. I would merely refer here to the excellent works by Dr. I. Esser on Max Valier[4] and Herman Ganswindt[5]. The many works on Hermann Oberth, Wernher von Braun, Robert H. Goddard, Robert Esnault-Pelterie, K. E. Tsiolkovski cannot even be listed here; that would add several more pages to the references.

I regard it as a moral obligation to say something more about Johannes Winkler, as I was his first assistant in 1931-1932, the years that were of such importance to his work. The suggestion that I could work for Winkler came in the beginning of 1931 from Hugo A. Hückel, the only faithful financier, who supported both Rudolf Nebel's rocket site group in Berlin-Reinickendorf and Johannes Winkler in

Dessau. During a private conversation with Hückel, who at that time was in the hospital in Berlin-Dahlem with severe tuberculosis of the bone, I told him of my doubts concerning the rocket site group, of which I had been a member since February 1930. I was not altogether satisfied with the entirely pragmatic manner of working, which aimed too much at publicity effects and too little at actual research on liquid rockets. Mr. Hückel shared my concern and told me about Johannes Winkler, whose private work he was supporting as well as his activities at Junkers. As my only acquaintance with Winkler up to that time had been through correspondence, Hückel enabled me to go to Dessau to meet Winkler. This encounter made a deep impression on me, for I found myself in the presence of a man who approached his self-appointed task with the utmost modesty but deep sincerity. He showed me the small HW I and HW Ia rockets, then nearing completion, with which he planned to demonstrate that a liquid-propellant rocket could fly - something which was seriously doubted by many at that time. Winkler told me that this was the sole objective of these experiments and that his next step would be a 2-meter HW II with which he hoped to break the altitude record for rockets. We agreed that I should work for him as soon as he had moved to the rocket site, as planned by Hückel. Hückel wanted to achieve a better utilization of the technical facilities, such as workshops, test stands, Dewar flasks for the transport of liquid oxygen etc., by amalgamating the two groups.

On 1 April 1931 Winkler sent me a handwritten report on the flight tests he had made with the HW I on 21 February and 14 March 1931, the second of which was successful. This report is the only authentic one written by Winkler himself. I have placed it at the disposal of the Space Hall of Honor at the Deutsches Museum in Munich[6]. In it Winkler wrote:

> "By 4:45 p.m. everything was ready for me to throw the ignition switch. It was an exalting and blissful moment when the apparatus rose from the launching table and climbed up with a rumbling, metallic hiss. Its motion was very steady. At a certain height the apparatus turned over further and further into the horizontal, then maintained this direction for some time and finally landed at a distance of nearly 200 m from the launching point".

At that time we thought this was the first flight by a liquid-propellant rocket anywhere in the world, and it was not until 1936 that we learned that Robert H. Goddard had already made the first successful launch on 16 March 1926.

Before Winkler moved to Berlin I had two more meetings with him, when he came to see Hugo A. Hückel, and we discussed in detail the technical design of his HW II. From the Summer of 1931 on we worked together every day at the rocket site, where I was able to persuade the outstanding mechanic Hans Bermüller of the Riedel group to join us. My own colleague, Heinz Springer, also started working with Winkler that Summer (Figure 2). Our work consisted in assembling the HW II and performing the necessary test runs on the final flight model. Here Winkler and I had our first disagreement. While with Klaus Riedel, I had seen how often test runs must be repeated on one and the same combustion chamber before one can be reasonably sure that the chamber will function dependably after many minor modifications. Winkler insisted stubbornly that two combustion tests with the tank half full, i.e., 25 seconds of combustion, would be sufficient.

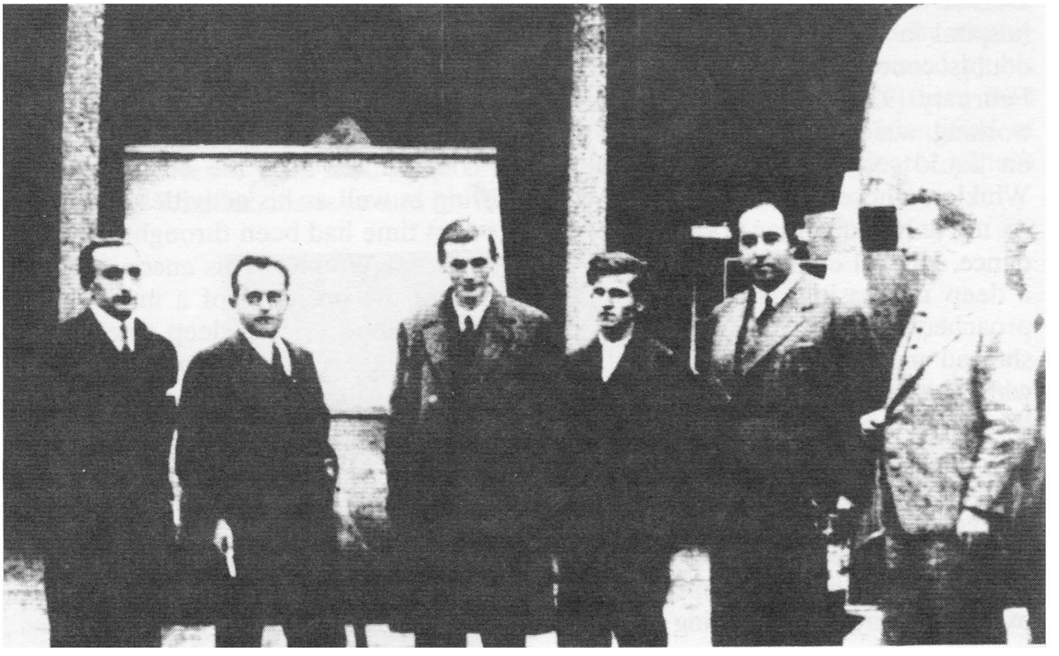


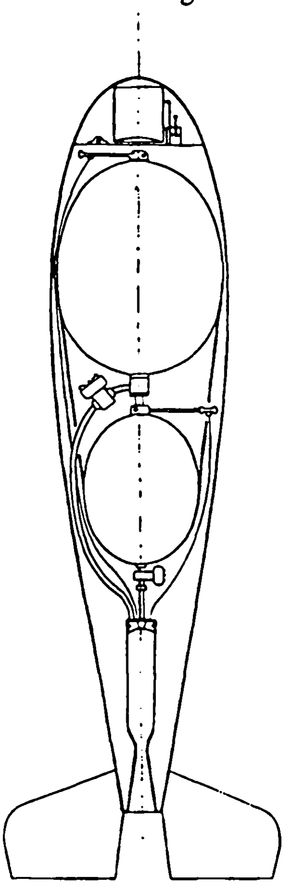
Figure 2 The Winkler Group in 1931 - from left to right: Hans Bermüller, Johannes Winkler, Rolf Engel, Heinz Springer and Journalists.

Permit me to quote myself at this point, from an unpublished paper entitled "Die geschichtliche Entwicklung der Raketentechnik" (The historical development of rocket technology)[7] in which at Hückel's suggestion I set down my own thinking in the Winter of 1931 about how all the practical tests had been performed up to that time:

> "On the basis of this incomplete result, namely, three combustion tests with a thrust of 12 kg each and one successful launch to an altitude of 90 m, he (Winkler) thinks that the road to high-altitude rockets is now clear and is beginning to build his 2 m rocket. This view is the major element in what I describe as his mental attitude to the problem. During one discussion on this point he said to me literally: 'If it works once, it will always work - at any rate for me!' But unfortunately our knowledge of liquid-propellant rockets is not yet sufficiently advanced for it to be possible to say that the conditions are always the same... The problem of the liquid-propellant rocket is unfortunately still a field in which every foot of ground has to be fought for bitterly."

The HW II was then prepared for launch in the Summer of 1932. The idea of launching it from the Island of Greifswalder Oie failed, because the Swinemünde port authority was worried about the lighthouse on the island and therefore refused us permission to attempt the launch. I then flew to Berlin, negotiated with various ministries and finally got permission to carry out the launch on the Frische Nehrung in East Prussia. We received a special grant from the Ministry of Transport for our move. When we filled up the HW II on the early morning of 6 October 1932, we found to our horror that the two liquid oxygen and liquid methane valves had become leaky. We were especially proud of these valves, as they were made of electron, an aluminum-magnesium alloy, an entirely new material at that time. Nobody then knew - not even IG-Farben AG, the manufacturer - that this material corrodes under the influence of salt water. Winkler and I spent a long time considering whether we should not be running at full speed. Members of the Königsberg government were on their way, and Navy vessels had already cordoned off the sea area and the Frische Haff. We decided to take the risk and to blow the rocket body through with nitrogen under pressure immediately before the launch. This was done, but perhaps not thoroughly enough. When the ignition was switched on, there was still enough explosive gas between the outer skin, the tanks and the combustion chamber to rip our "beautiful" HW II to pieces. The disappointment was tremendous. Winkler decided to return to Junkers, from where he had only been on leave for 1931/32. I tried to continue the work independently with the VDI's* voluntary service.

Let me add a few words here about the HW II, which is shown in Figure 3 along with its data. With the help of Messerschmitt-Bölkow-Blohm company in Ottobrunn, we reconstructed the HW II out of old parts. It is today in the Deutsches Museum. Compared with other rockets of the day, this liquid rocket was far in advance of its time. Its only defect was that nobody had foreseen that electron and salt water air did not agree.



Technical Data for the HW-II
(see figure)

Total length		190 cm
Diameter, max.		40 cm
External form Joukowski-profile		1 : 5
Stabilization fins		3
Propellant	liquid oxygen	32.0 kg
Weight:	liquid methane	4.0 kg
		36.0 kg
Structural	engine	1.7 kg
Weight:	tanks, valves, tubing form structure, fins	2.5 kg
Payload	barograph	1.5 kg
		10.0 kg
Engine data:	Combustion pressure	9 atü
	time of combustion (full thrust)	49 sec
	average thrust	96 kg
	specific consumption	8 kg/sec
	Cooling: Capacitance + boundary layer of liquid oxygen	

Launch 11.20 hrs on October 6th, 1932 at Frische Nehrung/East Prussia. Explosion because of new alu/magn.alloy (Electron). The ratio between dry weight and propellant weight was the best that could be achieved at that time =0.278, a figure which was not surpassed until 1943.

Figure 3 Technical data for the Winkler HW-II rocket, 1932.

* **Editor's note:** Verein Deutscher Ingenieure (VDI).

Time is too short here for me to describe Johannes Winkler's whole life. Instead, I should like to cite a few general historical points which show that Johannes Winkler was not only one of the leaders of German rocket technology between 1927 and 1932, but that from 1930 on he consistently built up a development philosophy that differed entirely from the view of the time on the development of large space rockets, representing a "third approach" which has remained largely unknown down to the present day. To make this clear, I must ask you to think back to the early phase of space work. This was initially stamped by the tremendous achievements of Hermann Oberth, whose book *Die Rakete zu den Planetenräumen*[8] in 1923 removed the whole space problem from the realm of fantasy and dreams to the sober technical and physical plane. In later editions, Oberth stated plainly that the road to space ships was via the large vertically launched rocket. Almost independently of him, Max Valier in his 1924 book entitled *Der Vorstoss in den Weltraum, eine technische Möglichkeit*[9] had presented another development philosophy that was definitely attractive. He advocated the aircraft approach. An aircraft, he said, should be equipped step by step with first of all auxiliary engines consisting of small liquid-propellant rockets, which should then be made bigger and bigger until finally a pure rocket-powered aircraft was obtained, and this could then be developed into a real space ship. However, such a vehicle would no longer take off horizontally on its flight into space, but from an inclined launching ramp.

Many of the rocket engineers engaged in practical work at that time instinctively rejected this approach, for virtually nothing was known then about the drag of an aircraft-type vehicle at low and high supersonic speeds. A certain amount was known from classic ballistics, however, about vertically launched, shell-like vehicles. This was much firmer ground than the still somewhat mysterious field of supersonic aerodynamics. Finally, Oberth's work on the problem of synergy demonstrated in 1928 that a rocket climbing ballistically would always be superior to a rocket aircraft taking off more horizontally. The situation changed in 1933, however, when Eugen Sänger's book on *Raketenflugtechnik* was published[10]. Here, for the first time, the idea of a rocket aircraft was given the necessary scientific and technical underpinning that had been lacking in Max Valier's work. From then on, two development approaches emerged increasingly clearly - not only in Germany, as we now know, splitting the space and rocket experts into two groups, the "vertical starters" and the "horizontal starters". Our small circle of old rocket and space fans called Oberth's book the "Old Testament" and Sänger's was the "New Testament". But we knew from our five years of practical experience that a clear decision in favor of one approach or the other would have to be made as soon as the development of liquid-propellant engines had reached a certain technical level. The two approaches involved a chain of development steps in which the priorities were set quite differently and, for the most part, were in themselves quite divergent. Once one had decided on one of the two approaches, one had to adhere consistently to its ineluctable chain of development steps. With the mass of individual problems awaiting solution, there was little or no prospect of abandoning the approach selected and switching to the other.

As is known, both approaches were adopted in the subsequent development of German rocket technology. The one led to the A4 (V2), the world's first long-range ballistic rocket weapon, and the other to the Me 163, the world's first pure rocket aircraft. However, both had one field of work in common, the development of high-thrust, dependable, liquid-propellant rockets. Doubtlessly, Sergei Pavlovich Korolyov chose the ballistic approach to intercontinental rockets, at Stalin's behest, whereas in the United States the Air Force gave priority to the rocket aircraft, as represented by the North American X-15. When the Soviet lead in long-range rockets became known to the United States in 1952/53, the U.S. Air Force had to institute a crash program to try and catch up in the development of long-range rockets. The development of the rocket aircraft therefore necessarily had to be abandoned. The pursuit of both approaches at once is beyond the capacities of even a major power. Holding consistently to its original decision, the U.S.S.R. still has not embarked on development of a rocket aircraft even today. This shows clearly how compelling is the complex of problems in each of the development philosophies; it can only be escaped by ruthlessly and completely abandoning the path originally followed.

I have already pointed out that both approaches required the development of large liquid-propellant engines. As anyone who has started out with the dream of space travel and been obliged by the political objectives of the powers to develop military equipment, for example, can hardly refrain from pursuing his old ideals of future space travel, at any rate in private. It is understandable that the old pioneers at the development centers everywhere--even in the midst of the war in Germany, as well as after the war when working for the two super powers--should have racked their brains about what a large rocket, or for example the construction of a space station, should be like. And it was easy for them to work out that, for any half-way reasonable payload, the first stage would have to have a thrust of about 500 tons. Building a combustion chamber for a thrust of 500 tons still seemed to most of them to be somewhat futuristic. Experience had shown that the laws of similarity could only be applied to the design of combustion chambers within very narrow limits. Test stands for such thrusts would require a tremendous capital outlay.

These thoughts had already emerged in the rocket site group in 1931/32, and I recall discussions on this subject with Wernher von Braun and Klaus Riedel. Wernher was optimistic and felt that it would certainly be possible to build combustion chambers of this size, while Klaus was more skeptical and thought that, though it would be right to have a common tank and feed system in each stage, the total thrust should be split up between, say, five chambers with a thrust of 100 tons each, to be on the safe side. He felt the difficulties would lie primarily in mixing the propellants. In a 500-ton chamber the propellant throughput would be on the order of 1500 kg/sec (at that time we were working on the basis of a maximum specific impulse of 300 sec), whereas for a 100-ton chamber it would be only 300 kg/sec. This was a quantity with which it might perhaps still be possible to obtain an adequate combustion efficiency. Ten years later, I saw the first 100-ton combustion chamber for Eugen Sänger's super-bomber on the Trauen test range.

Let us recall that the development step common to the two approaches always aimed at large, high-thrust engines. In the ballistic rockets, each stage was to be designed as a single tank and feed system, and it was only in the basic stage that the designers were prepared to split the thrust up between several combustion chambers.

After these general remarks about later work in Germany, I will now return to discussing Johannes Winkler's work. As already mentioned, Winkler joined the Junkers research establishment on 19 September 1929, taking over a specific task as the head of a section under Dr. Philipp von Doepp, namely, that of examining all the solid-propellant rockets on the market to see it there were any among them that could be used to assist the take-off of the Junkers seaplanes. If there were not, Winkler was to submit proposals for development of a new rocket engine. From notes in the Gruber Archives[3] it emerges that some 15 different rockets, all of them using black powder, were tested on the test stand under this program and evaluated by very modern methods. The reports on the tests are of great thoroughness, and the tests provided Winkler with a wide range of experience of test methods.

Winkler was then released between 1 April 1930 and the end of 1932 to carry out the work on the HW I and HW II which Hückel financed. Naturally he did not discuss his earlier work with me, as it was confidential, and it was not until I asked him in the Summer of 1932 to write a contribution for Werner Brügel's book[2] that he talked about the problem of large rockets and mentioned that he had solved it better than Oberth and the men at the rocket site, who had been trained in his way of thinking. When I asked him for details, he absolutely refused to say any more, for this concept was so revolutionary that he preferred not to talk about it for the time being; he did not want to "let go of the opportunity of his lifetime." A few weeks later, he mentioned that in his contribution to Brügel's book he was going to publish one of his ultimate formulas, for reasons of priority, but without revealing how he had arrived at it or the principle of his concept. The subject was also not mentioned again between us up to the end of 1932, for the day-to-day problems concerned with our preparations for launching the HW II engaged our full attention.

In Brügel's book (Ref. 2, page 111) Winkler then did in fact give one of his final formulas and a few indications of his ideas about large rockets. He wrote:

> "High performance levels, i.e., those which can no longer be obtained by a single jet motor, can be reached with a large number of similar jet motors of a size and power already produced. Propulsion power can be increased by having a suitable number at work simultaneously. Thus the payload can be of any desired size. Propulsion duration can also be extended by conceiving of the payload, including the first layer of jet motor underneath it, as a new payload for a second, larger layer of jet motors, etc. The concept has no limits in principle.
>
> The low power of the individual unit can be made up for by the larger number. Needless to say, the propellant tanks, for example, can be gathered together into one layer, and the number of combustion cylinders can even be reduced, etc. Under this approach, we can always say just how far we have got, and the formula also has a deeper meaning, but that would take us too far here."

His formula then follows. As Winkler himself never spoke about his "secret", I felt challenged to arrive at his formula myself, so as to get at its bases and thus judge its plausibility. However, I did not manage to do this until 1935, in company with my colleague Horst Laskowski, when we retraced all of Winkler's arguments and reconstructed his formula. In the process, the meaning of his veiled reference to the "deeper meaning" of the formula also became clear. He had indeed discovered a new - third - approach to the creation of large rockets for space use, an approach which nobody else had ever thought through to the end so consistently. His basic idea was to develop a small "standard rocket" with a thrust of, say, 10 tons, through a maximum power and absolute dependability on relatively small and therefore low-cost test stands. A suitable number of these standard rockets would then be clustered in each stage, ignited simultaneously and jettisoned as a complete stage after burnout. The system of formulas automatically provided the number required in each stage for a given mission. Anyone who carefully reads Winkler's articles in *Die Rakete* for 1928[11-12] will note that he always took care, in his mathematical model, to include only those parameters which were amenable to direct measurement. This also applied to the formula published in Brügel's book.

This method of working was in itself a distinct advance compared with the theoretical publications of that time. It must be admitted that the thought of clustering standard rockets rather than uniform stages containing large combustion chambers had something intriguing about it. I gave this design concept the name "aggregate principle" ("Aggregat-Prinzip"), as opposed to the "stage principle," and spent more than a year investigating this principle with my colleagues, gaining a considerable insight into the interrelationships between thrust, propellant weight, dry weight and permissible acceleration. I presented a small selection of the results obtained between 1935 and 1938 at the 7th International Astronautical Congress in Rome on 21 September 1956[13]. Of course, throughout all these years I had not known whether Winkler had come to the same or similar conclusions. I was therefore not surprised to find among this works in the Gruber Archives a comprehensive report on "Composite Rockets", which gives what must be the first overall presentation of the aggregate principle, practically with the same premises and results as we had worked out between 1936 and 1938. I should like to stress specifically that this does not prejudice Winkler's priority. He got his results at least six years before I did, but maintained complete silence about them throughout the years vis-à-vis both Dr. von Doepp at Junkers and Professor Busemann, his superior at the AVA in Braunschweig. This is also proved by a question which Professor Ernst Schmidt put to me about 1944, when he wanted to know what was really behind Winkler's mysterious hint that "he had long since found the solution to building large rockets". In his capacity as head of the engine department at Braunschweig he would inevitably have been acquainted with any reports Winkler might have written during the war.

Many of the notes in the Gruber Archives show that, even during his testing activities at Junkers, Winkler had already been privately examining the results to see if any of the rockets tested would be suitable to become the standard model. On pages 21, 21a, 22 and 22a of the above-mentioned report[14] he lists 23 solid-propellant rockets and 34 liquid-propellant rockets with their operating data and the "rating" given them in the aggregate theory. However, as none of them correspond in full to his idea of a standard rocket, he gave on page 26 a drawing of an engine with compressed gas feed and the propellant combination of nitric acid and orthotoluidine, which delivered a thrust of 10 tons. It is both saddening and astonishing that Winkler should have brought himself to keep silent about his discoveries for nearly 18 years. It was not until Professor Otto Lutz from Braunschweig was in England in 1946-1947 and wrote on 11 March 1947 asking him to come to England too that Winkler saw he would have to offer something "attractive" and therefore - in the desolation of those times - summarized and revealed his ideas on the aggregate principle[14]. Anyone who has "plowed through" the early literature on space as we young men did in the 1930s, will obviously remember that in his first book in 1919[15] R. H. Goddard specifically mentioned the aggregate idea alongside the classic ideas of a step rocket. Unfortunately, he did not bring out the differences sufficiently in his theoretical treatment. W. Hohmann also developed similar ideas on pages 8 to 11 of his book in 1925[16]. But Winkler was undoubtedly the first to follow these ideas through consistently to their conclusion and to collect them very skillfully into a mathematical form. This must be stated quite plainly, for now that the papers in the Gruber Archives have been studied nobody today can deny him the priority of an intriguingly simple and cheap principle for the design of large rockets.

You will now quite rightly ask why this intriguing idea has not been accepted in space work to date. There are many different reasons. In the Soviet Union, the old guard of space pioneers from the 1930s certainly did occupy itself with similar ideas. When S. P. Korolyov set out to push through his first intercontinental rocket to obtain "carte blanche" for his space aims, he put the A launcher together out of four conical R-14 rockets and one cylindrical central unit from the existing SS-6 = Sandal medium-range rocket. Both were rocket models that had been proven over many years and both were exceptionally dependable thanks to the engines developed by W. P. Gluschko, the RD-107 in the R-14 and the RD-108 in the Sandal. From the Summer of 1957 up to the present day - since it carried the first Sputnik into space on 4 October 1957 - this composite rocket has experienced virtually no troubles in its one and half basic stages. The Al, A2a, A2b and A2e, it and its upper stages have remained the U.S.S.R.'s major launcher. But it is not a real aggregate rocket. Korolyov chose this combination so that he could assemble existing, highly dependable equipment into a launcher. The D launcher specifically developed for space applications (roughly equivalent to the United States Saturn I and IB) is based on the same principle. Whereas in the A launcher 4 x 4 + 1 x 4 = 20 engines are launched simultaneously, there were 6 x 6 + 4 = engines which had to be ignited at the same time in the D launcher. Although Glushko's engines in themselves had a high operating reliability, the complex control process for the 40 engines led to the bitter recognition that the combination of so many engines was not altogether compatible with the requirement for a uniform thrust build-up. The D launcher had to go through a "learning curve" that is quite comparable to that of the United States' Thor and Atlas launchers.

As the Soviet Union's rocket expert with the greatest test experience, Korolyov realized that the construction of composite rockets has its limits. His rival,

Y. Yangel, had not yet acquired this experience and designed the giant G-1 launcher (roughly equivalent to the Saturn V) more consistently as an aggregate rocket. Although it took from 1963 to 1969 to assemble this launcher, the first flight model exploded on the launch pad on 10 (?) June 1969. (The exact date is not known because monitoring by U.S. photographic reconnaissance satellites did not yet provide full coverage). The second flight model exploded on 25 (?) August 1971 at an altitude of 12 km, roughly in the zone of maximum dynamic stress. The third flight model exploded on 24 November 1971 at an altitude of about 40 km. After these three failures the Soviets definitely buried their hopes of at least "drawing even" with the Apollo project for a landing on the Moon. It is known that the entire launch site organization for this heavy launcher had been ready for operation in 1972, but so far the new heavy launcher itself has not been sighted. It would probably not be wrong to assume that the whole concept of this launcher has been completely altered since the three failures and that the partial aggregate principle has been abandoned in favor of the classic stage principle. But a new design of this kind takes at least eight to ten years unless - as was the case with the Saturn family - it is prepared systematically in three development stages, which has so far not been observed in the U.S.S.R.

Wernher von Braun did not think highly of the aggregate principle for the following reasons, which he expounded to me during a conversation in 1941:

1. The empty weight of a small standard rocket is basically too high and therefore has a negative effect on the overall ratio.
2. The aggregate principle basically requires a considerably larger number of stages for the same flight performance than a rocket designed on the classic step principle.
3. with the right design of the injection and cooling system, the big combustion chamber with thrusts of 100 tons or more produce the same efficiency as a sophisticated standard rocket.

He pursued these principles consistently and only deviated from them when it was a question of providing the preliminary stages for the ultimate Saturn I (Block II) and Saturn IB. Von Braun built up the basic stage out of eight tank units from the Redstone rocket and one from the Jupiter, while the overall engine in this stage was replaced by eight clustered H-1 engines. However, this was in no way a concession to the aggregate principle, but a compromise dictated by deadline requirements, as he himself told me, because the complicated jigs needed for construction of the tanks were available at Huntsville. Providing new tank welding machines for Saturn I would have put the program back at least two years. With Saturn I he overcame his own and NASA's reservations about such a composite stage, namely that the failure of one engine unit would jeopardize the launcher's entire flight mission. On 28 March 1965 he deliberately cut off one of the eight engines. The resulting asymmetrical torque was corrected by the other seven after only a few seconds. Von Braun was then able to demonstrate his ideas about large combustion chambers in the giant Saturn V launcher. Each of the five F-1 engines delivered a launch thrust of 680 tons. Their efficiency was entirely comparable to that of smaller combustion chambers.

As it is not to be assumed that Glushko, Korolyov and von Braun deliberately went for large engines and large stages in order to push up costs and make the engine and stage units as complicated as possible - as is maintained today by a small group in Germany, who attack the aggregate principle as a "cheap rocket"; it must be clearly recognized that the combination of small standard rockets has its technical limits. This has been clearly demonstrated by Yangel's bitter experience with the D and G launchers in the U.S.S.R. The time will perhaps come when the technology of controlling the thrust build-up of many clustered engine units has been mastered better than it has been today. It will then be possible to turn back to Winkler's dream of an aggregate rocket. But all these objections cannot alter the fact that it was he who pursued this idea with utmost consistency from 1929 until his death on 27 December 1947. Many rocket scientists have pursued ideas that reached far into the future and can only be put into practice by future generations. Nobody can reproach them for this, for it was their job, as genuine pioneers, to point the way into the future.

Let me add a few more words. In about 1940 scientists--in particular those engaged in aeronautical research in Germany--were called upon to create a better basis for selection of future propellant combinations through theoretical work on the combustion process in rocket combustion chambers. In practice this amounted to finding out about the complicated dissociation processes of combustion gases at extremely high temperatures. Among aeronautical research workers it was first Otto Lutz[12], G. Damköhler and R. Edse[18] and M. von Stein[19] who worked out new, in part semi-graphic methods, but these were intended primarily for air-breathing engines. It is therefore understandable that Johannes Winkler should have tried to transform these methods so as to bring them closer into line with conditions in a rocket combustion chamber. I did the same in 1943 for the combustion of solid-propellant rockets, at Damköhler's suggestion. Winkler also tackled this task and submitted a very practical method of solving the problem mathematically in 1944[20], as I discovered from a "secret report" by the MAP, Volkenrode in the Gruber Archives. This report again reveals Winkler's primarily practical attitude to research. His method is oriented much more clearly to the needs of the engine designer.

To close, I should like to say something about Winkler's personality. In my "youthful report" - I was barely 20 years old at the time - I wrote, after working with Johannes Winkler for six months:

> "He is very calm in manner and a definitely quiet person. Close cooperation with him is possible if one subordinates oneself to him. But he is also understanding and does not always insist on his own opinion. Then it must unfortunately be said, he is very afraid of explosions and the like, which happen very often in our type of work..."

Leading research personalities under whom Winkler worked had an excellent opinion of him. When he left Junkers voluntarily (to complete the HW I and HW II), Dr. von Doepp[21] wrote the following:

> "Mr. Winkler has advanced his difficult task excellently in the short time he has been here, despite the paucity of the means placed at his disposal. He was highly qualified for this not only because of his complete mastery of this special area and his outstanding qualities of character, such as thoroughness, perseverance, self-discipline and adaptability to the requirements of the situation, but also and in particular because of his inventiveness and his high level of interest in the duties assigned to him..."

Professor A. Busemann, his superior at the Luftfahrt-Forschungsanstalt Hermann Göring in Braunschweig, gave him a parting testimonial at the end of the war[22], in which he wrote:

> "Mr. Winkler has advanced the work entrusted to him in excellent manner. He was especially capable of this not only because of his long experience of experiments in the field of equipment design, but also because of his thoroughness combined with practical sense which never strayed into non-essentials, but quickly returned to the main line. He was always an example of diligence and conscientiousness to his department and he possessed the ability to arouse his staff's other researchers. His quiet, poised and unassuming nature made working with him particularly pleasant."

I have tried to show you in a few words that Johannes Winkler was not only one of the trailblazers about whom nobody quite knew after 1933. He is quite definitely one of the great space pioneers, as can now be proved from the Gruber Archives. He pursued his own course alongside those of Oberth/von Braun and Valier/Sänger with unshakable determination. Winkler believed he had discovered the secret of designing large rockets and waited patiently for his "great moment". He died during the desolation of the post-war years, without having had the opportunity of putting forward his work for public discussion. It is not only the right but also the duty of a genuine pioneer to pursue his own dreams. This Johannes Winkler did with never-failing courage. We must therefore see in him one of the great German pioneers. I hope that it will be possible some day to exploit the Gruber Archives in full, for then the greatness of his character will emerge more clearly than it has in my present attempt to describe it.

REFERENCES

1. Frank H. Winter, (National Air & Space Museum, Washington, D.C.), Birth of the VfR - The Start of modern Astronautics, *Spaceflight* 19, No. 7/8, July/August 1977.
2. Werner Brügel, *Männer der Rakete*, Vlg. Hachmeister & Thal, Leipzig, 1933.
3. Frau Elisabeth Gruber (=Arkiv Gruber), Dettem b. Braunschweig.
4. Dr. I. Esser, *Max Valier - Ein Vorkämpfer der Weltraumfahrt*, VDI-Verlag, Düsseldorf, 1968.
5. Dr. I. Esser, *Hermann Ganswindt - Vorkämpfer der Raumfahrt*, VDI-Verlag, Düsseldorf, 1977.
6. Johannes Winkler, Dessau, *Der Start der ersten Flüssigtreibstoffrakete* (handwritten manuscript), 1.4.1931, Deutsches Museum, Munich.
7. Rolf Engel, *Die geschichtliche Entwicklung der Raketentechnik*, Manuscript, Winter 1931, Archiv R. Engel - Deutsches Museum, Munich.
8. Hermann Oberth, *Die Rakete zu den Planetenräumen*, Oldenbourg-Verlag, 1923, Munich.
9. Max Valier, *Der Vorstoss in den Weltenraum - Eine technische Möglichkeit*, Oldenbourg-Verlag, 1924, Munich.
10. Eugen Sänger, *Raketenflugtechnik*, Oldenbourg-Verlag, 1933, Munich.
11. Johannes Winkler, "Die Treibkörper des Raumschiffes aus Raketenelementen," *Die Rakete*, 15.1.1928.
12. Johannes Winkler, "Hochwertige Raketen," *Die Rakete*, 15.4.1928.
13. Rolf Engel, *Remarks on the Design of Step Rockets*, Proceedings of the 7th IAF Congress, Rome, 1956, page 115.

14. Johannes Winkler, *Zusammengesetzte Raketen*, MOS - (A) - Volkenrode, Bericht 657.

15. Robert H. Goddard, *A Method of Reaching Extreme Altitudes*, Smithsonian Institution, Washington, 1919, page 66.

16. Dr.-Ing W. Hohmann, *Die Erreichbarkeit der Himmelskörper*, Oldenbourg-Verlag, 1925, Munich.

17. Prof. Otto Lutz, *Die Ermittlung der Zusammensetzung dissoziierender Gasgemische*, AVA-Monographie VII, 1942.

18. G. Damköhler, R. Edse "Zusammensetzung dissoziierender Verbrennungsgase und die Berechnung simultaner Gleichgewichte Zeitschr." f. *Elektrochemie*, 39, 1943, pp.178-186.

19. M. von Stein, "Verfahren zur Berechnung der Flammentemperaturen, der Enthalpie und Entropie von Feuergasen Forsch." a.d. *Gebeit d. Ing.-Wesens*, 14, 1943, pp.113-123.

20. Johannes Winkler, *Neuere Verfahren zur Berechnung der Dissoziation von Verbrennungsgasen von Raketen*, MAP - Volkenrode, Bericht No. 159R, 1944.

21. Dr. von Doepp, Forsch. Anstalt Junderswerke, Zeugnis über die Tätigkeit v. J. Winkler, 20.3.1930.

22. Prof. A. Busemann, LFA-HG Braunschweig, Zeugnis f. J. Winkler, 30.6.45.