The scientific method to open the Age of Reason

by Uwe Parpart-Henke

On June 15 and 16, a conference was held in Reston, Virginia in memory of the German-American space scientist Krafft A. Ehricke. The conference was titled, "The Age of Reason, in a World of Mutually Assured Survival and Space Colonization," and discussed scientific breakthroughs in the beam defense program and the classical scientific method that led to those breakthroughs.

The Fusion Energy Foundation and the Schiller Institute convened the conference to bring together a group of international military, scientific, diplomatic, and community leaders who would take responsibility for solving the profound crisis gripping the Western world.

Uwe Parpart-Henke, whose address to the conference on June 15 is published here, is the Research Director of the Fusion Energy Foundation, and co-author of Beam Defense: An Alternative to Nuclear Destruction.

I want to start out by simply recounting one element of our association in the Fusion Energy Foundation with Krafft Ehricke. It did not come about directly as a result of his work in space-related matters, but on a rather broader subject. I believe my recollection is correct that we first got in touch with Dr. Ehricke when an article appeared in the German daily newspaper *Die Welt,* in which he launched a pointed and direct frontal attack against the "Limits to Growth" philosophy that was being expounded by the Green Party in Germany and by similar kinds of organizations around the world, going back to the 1971 *Limits to Growth* book published by Forrester and Meadows at MIT, which expounded a philosophy that was so contrary to Krafft Ehricke's entire outlook that he felt it was absolutely necessary to say in print, and in very forceful ways, why and how he disagreed with that way of looking at the world.

In light of Mr. LaRouche's remarks this morning about what defines a successful program [see *EIR,* July 2, 1985, "Conference honors space pioneer with drive for SDI[, what is the conceptual depth and the conceptual breadth of a program such as the Strategic Defense Initiative program and other programs that we are now contemplating, it is absolutely critical to realize that it was, ultimately, A B

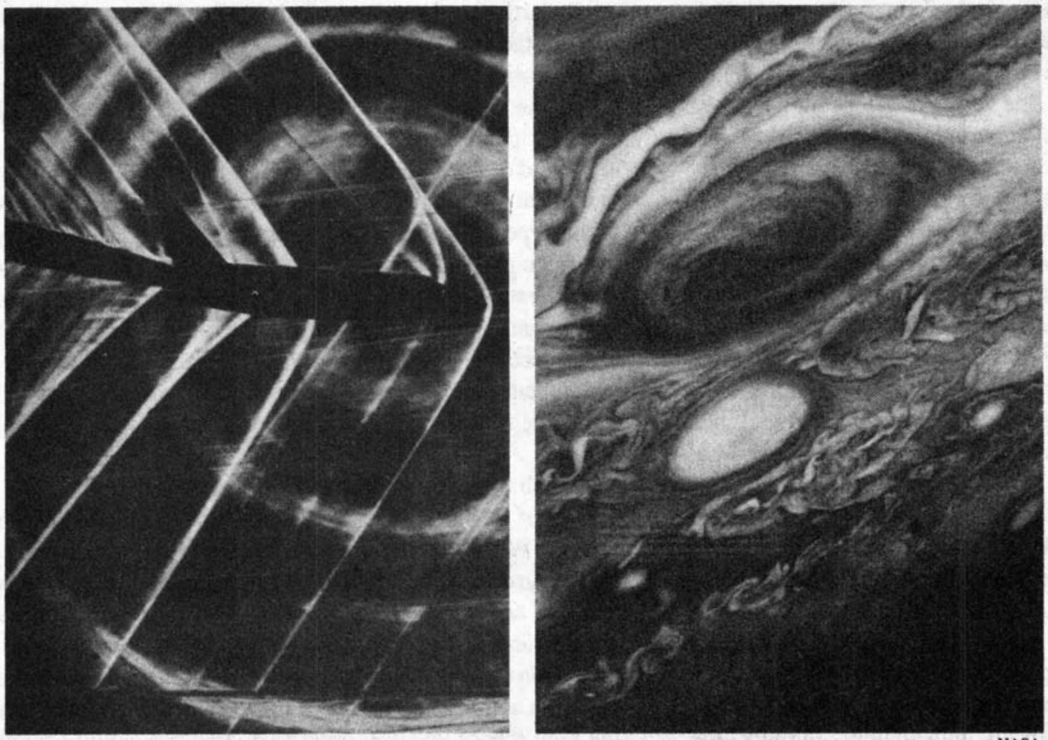


FIGURE 1

NASA

Study of the formation of vortices in fluid flow was pioneered by Ludwig Prandtl whose work influenced later advances in aerodynamics. These are photographs of air flow past a model of the Shuttle in a wind tunnel (a), vortical flow on Jupiter (b).

Krafft Ehricke's broad philosophical outlook that there are no limits to growth, that any kind of thinking of that sort will necessarily lead us in the wrong direction, that basically defined his approach to the specific technical problems that he tackled as well.

What I want to do is to contrast two types of approach philosophically, epistemologically, to the kind of thinking that ultimately finds its way into large programs, like the Manhattan Project, the Apollo Project, or the Strategic Defense Initiative program. One cannot simply see these as technical organizational problems or technological problems, but one has to get some understanding, of what is the broadest cultural background that defines the possibility of the successful development and execution of such large programs.

The Prandtl approach

The film that I want to show you now was made mostly in the 1920s and issued in 1927. Its title is *Generation of Vortices in Water Flows*. Such films were used for teaching students at the universities, on the characteristic features of fluid dynamics. This film was put together under the direction of Ludwig Prandtl, director of the Kaiser Wilhelm Institute for Fluid Flow at Göttingen, who is probably the single most significant researcher in this century in hydrodynamics and aerodynamics research. It is the Prandtl approach to these problems of fluid mechanics and fluid dynamics, which I want to use to exemplify for you the type of outlook and the kind of philosophy that has to find its way into the development of these large-scale research programs, if they are ultimately going to succeed.

Let's first take a look at these filmclips without much commentary and then go into the background.

The film is from the Institute for Fluid Flow in Göttingen, and shows the vortex formation in water flow. Dr. Tietjen, who is mentioned here, was the co-author with Prandtl of what was probably the most influential book on fluid mechanics. The surface of the water has been sprayed with some aluminum, in order to make it photographable, and this is streaming around a cylinder. The fluid flow comes from the left, as the actual vortex formation, which becomes large-scale after a short period of time. Some of you may recall the pictures taken on Jupiter by the space probe, which showed a very similar kind of phenomenon of the large red spot on Jupiter (**Figure 1**).

This is a closeup of how this so-called boundary layer rips off and develops the vortex, the fluid vortex. The back stream, the backflow around this cylinder is next. You can think of this as an airfoil, as a wing, and you can see the backflow beginning on the lower right-hand side and creating the vortex. If that occurs on a large scale and in the center of an airplane wing, it leads to stall. I want to call your attention to the very critical role of the so-called boundary layer, very close to the surface.

Now we are looking at an elliptic cylinder in the so-called subcritical regime. The boundary layer is that sort of light, surrounding mass around the dark cylinder. Subcritical in this case means that the fluid flow is relatively slow and does not lead to rapid vortex formation. Now you see hypercritical flow around an elliptical cylinder. You can see now, how the backflow develops and the vortexes form.

Now the stream lines around the sharp object, like the tip of a knife. Because of the large difference in velocity on the left side and the right side, the vortex develops immediately.

Now we are looking at an airplane wing, an airfoil. The most interesting moment is the first moment, when you see the onset at the end of the first vortex, which then begins to rip off the boundary layer.

This is again around an airplane, an airfoil. You can think of the airplane starting out, and that's what you will get in the air. If you have ever been in an airport and are close to a 747 taking off, you know that these vortexes hit you quite hard, and in fact smaller planes cannot take off in the wake of a large jet. In this case the wing is accelerated and then stopped again and only the flow is followed to see what happens.

Now a rotating cylinder is investigated, and because of the rotation the boundary layer is not ripped off early and no vortexes are formed. You could build a rotating airplane wing, that might be fine, except that does not seem very practical. So, first there is the rotation, and then the rotation is stopped. The rotation prevents the boundary layer from being disturbed by the possibility of vortex formation. When it is stopped, the vortex forms immediately. Initially, the cylinder was not rotating the vortex form, and, when it started rotating, it got rid of the vortex formation. When it stops, the boundary layer is ripped off. By applying suction, you prevent the vortex formation. When the suction is reduced, immediately the vortex forms and the boundary layer is ripped off.

The Göttingen tradition

The research that led to such photography and teaching films, had started at the University of Göttingen around the turn of this century. Prandtl came to Göttingen in 1904 and initiated this kind of research, building the first sizable wind tunnel and similar apparatus, which made observations of this kind possible.

What I want to do, is review for you some of the broadest philosophical background to the kind of thinking, that enabled the researchers at Göttingen, at Berlin, and at Aachen in particular, to make the kind of breakthroughs in fluid dynamics and in aerodynamics, in the early part of this century, that made manned flight, ultimately supersonic flight, and then rocket flight a reality. I want to counterpose that to a different kind of philosophical tradition, which, if it had prevailed over the tradition that led to the work of Prandtl, would have left us in a situation, where most of the developments that we have seen in this century, and especially after World War II, would have been either very far delayed or might not have occurred at all. **Figure 2** shows a kind of derivation of the tradition, with some specific emphasis on the geometrical type of thinking, that was characteristic of the Prandtl school and of the individuals, whose earlier scientific ideas led up to that.

In particular, I want to make a few detailed remarks about perhaps the most influental and least known mind in this line of succession, Jakob Steiner.

At the beginning is Gaspard Monge. Monge was one of the principal researchers at the French Ecole Polytechnique, at the end of the 18th century, and he pioneered a method of looking at differential equations, equations which define different types of complicated physical processes, essentially from a graphic or a geometrical point of view. These methods proved extremely successful in the early work of the Ecole Polytechnique and then led to a situation, in which many of the students not directly of the Ecole Polytechnique of Monge, but students conceptually of these ideas, perfected this and were able to make enormous progress in a very short period of time.

Jakob Steiner was born in 1796 and he came to the University of Berlin, which had then just been founded by his mentor, Wilhelm von Humboldt. He came to Berlin as somebody who did not have a job; he knew a great deal of geometry and was convinced of his ability to solve the most difficult geometrical problems, but he did not have the kind of formal education that would have allowed him to become a professor in Berlin at that time. He could not even become a teacher at the high school level: In order to do that, he would have to pass a so-called State Examination, and he tried that in 1822 after he had just come to Berlin.

He had the bad fortune that one of his examiners in the field of philosophy was Hegel. Those of you, who attempted to read some of Hegel's writings, will appreciate two things that Steiner did: First of all, before he was examined in philosophy, he wrote a note of protest, objecting to the idea that he should be examined in the kind of obscurantism that Hegel's philosophy represented. Hegel then, as you might imagine, retaliated in the examination itself and wrote a report. Hegel said, "Jakob Steiner concerns himself only with entirely trivial reflections." These "entirely trivial reflections" define the conceptual basis in almost every respect of the type of work, which led to the film that I showed of Prandtl and his collaborators.

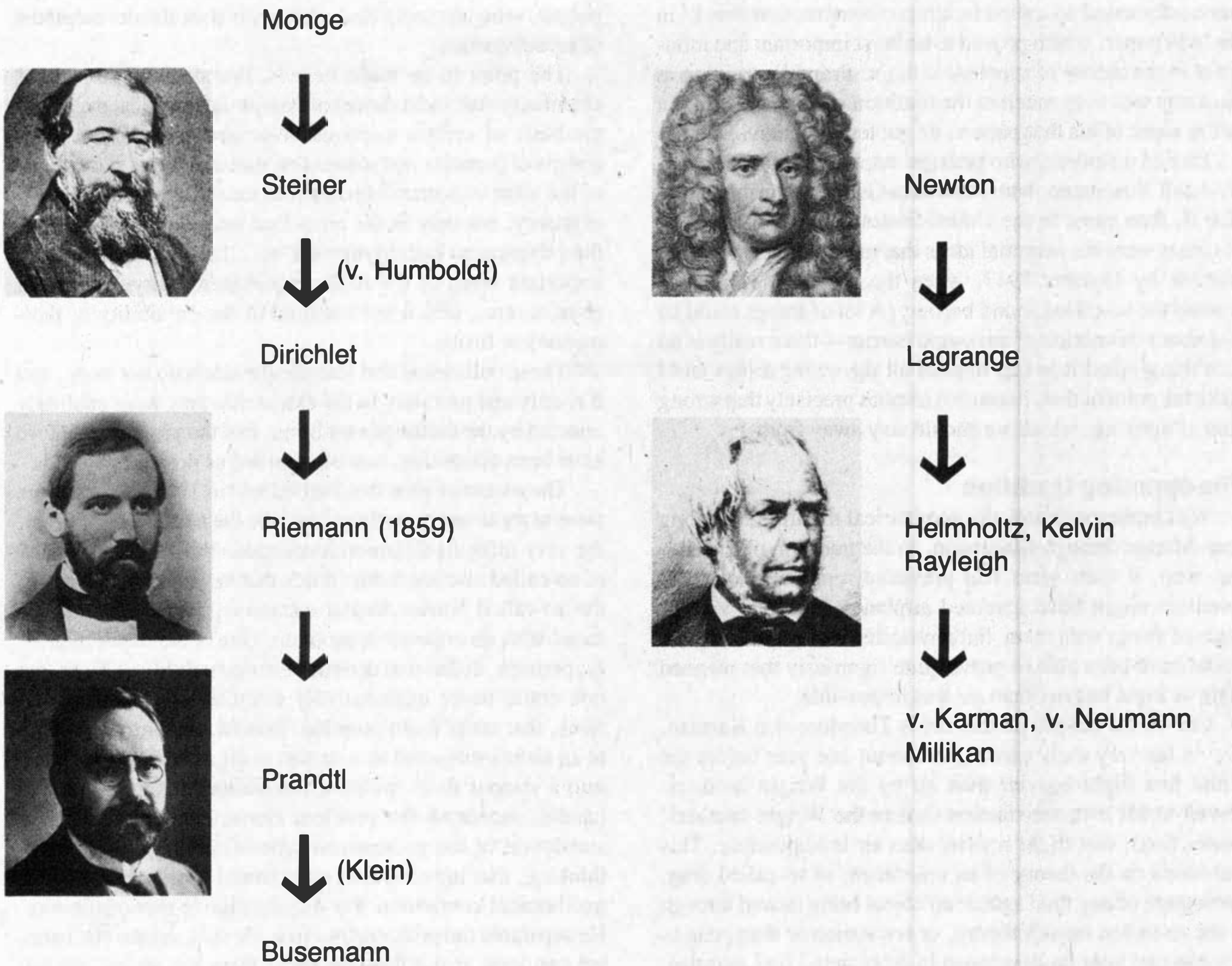
Steiner's so-called triviality in the mathematical field was characterized by the fact, that he abhorred algebra and he was also tested in algebra. The two things he was tested in were Hegelian philosophy and algebra. He flunked both of these tests marvelously. The quote from the person who tested him in algebra, was that his knowledge of algebra does not appear to go beyond the solution of equations of the second degree and he does not even seem to be very familiar with that. Equations of the second degree, are something that, perhaps unfortunately, people are now being taught at a rather early age. But in any case, his genius in geometry was recognized, and perhaps we don't know the details, but perhaps first recognized by Wilhelm von Humboldt, who founded the University of Berlin and was the minister of culture of Prussia for a while.

He had his youngest son educated in private by Steiner

FIGURE 2

The Göttingen tradition

The villainous tradition



after Steiner had been denied official certification as a teacher. The first book that Steiner wrote on geometry, which became the principal textbook in geometry at the University of Berlin later on and in many of the German universities and high schools afterwards, was dedicated to von Humboldt and von Humboldt's method of thinking. What Steiner always stressed in teaching his students was that there is a very close relationship between the kind of creative playfulness that we apply in geometrical contructions and our ability to develop entirely new concepts; whereas, on the other hand, algebra puts the mind into the kind of straitjacket that does not enable the student at a later point to apply himself creatively to new types of problems. I don't want to review in any further detail the career of Steiner. I want to point out that in 1834 he finally got his appointment at the University of Berlin, because it was recognized that he was an obvious genius in his field. His efforts to become a professor were supported by Crelle, by Bessel, by Dirichlet, and by Jacobi, who were then the greatest mathematicians in Europe.

The opinions of Hegel and of some other mathematicians, who initially examined him, were thankfully ignored at that point and he was made a professor. In 1847-48, he became a principal teacher at the University of Berlin of Bernhard Riemann, and it is the work of Riemann and Dirichlet in the 19th century that really laid the foundations for the work in fluid dynamics and aerodynamics that developed the possibilities of manned flight and of rocket flight later on.

Especially from the standpoint of the possibility of supersonic flight, a paper that Riemann wrote in 1859 on shock waves—the kind of waves that are formed in a compressible fluid, be it a gas or any other kind of compressible fluid—proved extremely influential. It was one of the most important things to consider, when supersonic flight was contemplated in the period of World War II and afterwards. Contrary to many of the critics of Riemann, it was precisely the case that he discussed so-called isentropic compression shocks in his 1859 paper, which proved to be most important and influential in the theory of supersonic flight. Prandtl's training in Germany was very much in the tradition of Riemann, and in fact in some of his first papers, he quotes Riemann in detail.

He had a student, who perhaps many of you never heard of, Adolf Busemann, who worked in Germany during World War II, then came to the United States after World War II. His ideas were the essential ideas that made supersonic flight possible by October 1947, when the first Bell X-1 plane crashed the so-called sound barrier. (A lot of things could be said about this notion of the sound barrier—there really is no such thing—and it in fact implies all the wrong things and I make the point of that, because it implies precisely that wrong kind of thinking, which we should stay away from.)

The opposing tradition

We counterpose now the geometrical tradition, reaching from Monge through Busemann, to the tradition of the people, who, if their ideas had prevailed, engineers or other inventors might have invented airplanes and done various kinds of things with them, but physicists and mathematicians would have been able to prove quite rigorously that manned flight or flight heavier than air was impossible.

One of the people on this list is Theodore von Karman, who in his very early career, just about one year before the actual first flight heavier than air by the Wright brothers, proved to his own satisfaction (not to the Wright brothers' satisfaction), that flight heavier than air is impossible. This was based on the theory of air resistance, of so-called drag, a resistance of any fluid against an object being moved through it: the so-called impact theory, or resistance or drag, due to Newton and later on developed in more detail by Lagrange. One could perhaps say, somewhat ahistorically and facetiously, but nonetheless correctly, that Newton was the first to prove that flight heavier than air or any kind of flight—in fact it is not even clear, how birds could fly under Newton's theory—was impossible.

Prandtl makes the point in his famous textbook (actually written by Tietjen on the basis of Prandtl's lectures) by saying, that if it were the case, that drag or resistance increases with the square of the velocity, then under those circumstances it is extremely difficult to see how flight of any kind is conceivable. The way Newton arrived at this, is on the basis of this so-called impact or collisional model; i.e., thinking of an airfoil or even a plate injected into an airstream, and simply computing the impact and the forces of impact of the molecules that impact on this particular airfoil, that impact on any kind of object put into the flow. This way of thinking and von Karman's calculations that led him to believe that flight heavier than air was impossible, were based on that kind of impact model. Essentially, he said, the molecular pressure would prevent takeoff. You shall see later on, how this kind of thinking was quite pervasive, even at a point when von Karman later on became one of the celebrated people, who allegedly had a lot to do with the development of aerodynamics.

The point to be made here is, that this collisional and essentially statistical model of computing physical events on the basis of certain averages, averaged over particles and groups of particles and molecules statistically, is proved one of the most important barriers to a satisfactory development of theory, not only in the areas that we are discussing here, fluid dynamics, hydrodynamics, etc., but also in the equally important areas of the field of quantum theory, of plasma physics, etc., which are essential to the possibility of thermonuclear fusion.

These collisional and statistical models do not work, and it is only and precisely to the extent that they were explicitly rejected by the Göttingen tradition, that the programs that we have been discussing, can be regarded as possible.

The essential idea that Prandtl had in 1904, is that if one were to try to use directly to describe the possibility of flight, the very difficult differential equations that govern the flow of so-called viscous fluids (fluids that have internal friction), the so-called Navier-Stokes equations, then one would be faced with an impossible problem. One could experimentally, perhaps, define and determine the possibility of flight, but one could never quantitatively calculate the actual conditions, that make flight possible. Prandtl, rather than looking at an airfoil subjected to a stream of air as an airfoil injected into a viscous fluid, which mathematically is impossible to handle, separated the problem characteristically from the standpoint of the geometrical type of thinking, the type of thinking, that introduces as an essential characteristic of the geometrical continuum the singularities in this continuum. He separated the problem into two. He said, on the one hand, we can look at the flow far away from the airfoil, the so-called free flow, on the basis on the very simple potential equations according to Laplace. These are trivial and relatively easy to understand differential equations, which have an immediate geometrical interpretation in the context of so-called conformal mapping theory.

Prandtl said the only area in which we have to consider flow that has internal friction, is in the immediate vicinity of the airfoil itself, in the so-called boundary layer, and that is that little white layer that you saw around the objects in flight earlier. In this area, we can no longer ignore viscosity, we can no longer ignore the internal friction of the fluid, in particular, because we know, on the one hand, that directly at the surface of the airfoil the flow is zero; i.e., the air of the water, or whatever it is, actually sticks at the surface. A very small distance away from this, it is clear, that it has already attained the velocity, which is equal to the free flow velocity. What we must look at is this critical boundary layer or what he called surface of discontinuity, in which, over an extremely thin layer—which can in fact be thought of as arbitrarily thin—a very, very large difference in velocity is attained. If we take into account the theory of this boundary layer from the standpoint of thinking of it as a surface of discontinuity, under those circumstances we can simplify the Navier-Stokes equations quite significantly, and are therefore able to give a quantitative solution to the problems of drag, of lift, of the other aerodynamical problems that are critical to discuss the possibility of flight.

Without the kind of work, that Prandtl did—first published in 1904, and discussed by him previous to his coming to Göttingen, when he was a teacher at the Technische Hochschule in Hannover—without these kinds of discussions of the boundary layer problems, it is generally acknowledged today, that a quantitive discussion of the possibility of flight would not have been available.

One of Prandtl's most important colleagues was Runge, a mathematician who developed many of the mathematical methods for calculating the problems in aerodynamics that Prandtl raised.

The role of Felix Klein

I would like to make a few remarks about the role of Felix Klein, the teacher of many of the students in the late 19th and early 20th century in Germany in mathematics and in physics, who at the same time was one of the most accomplished organizers of the total scientific technological and industrial enterprise in Germany. Klein had earlier made a name for himself by developing some very interesting and significant work in elliptical function theory, and in the 1890s he came to Göttingen as a professor and made it his task to try to define a research program for the entirety of the technical and scientific disciplines at the University and importantly in close collaboration with Willamowitz, who was the senior faculty member in the field of *Altphilologie*, ancient languages with specific emphasis on Greek. Klein and Willamowitz jointly defined an outlook on research and education, which I think is uniquely responsible, in terms of its philosophy, for the advances that were made in Germany in that period. At the same time, Klein in particular enlisted and in a certain sense forced German industry into supporting this kind of research, both by financially supporting the research institutions that were being built at the German universities, and at the same time created inside their own companies and allocating up to 20% of the total profits of the company for research and development.

Klein founded the so-called Göttingen Association, the *Göttinger Vereinigung*, in 1898. This was the group of professors at Göttingen who collaborated with the principal people in German industry. The Göttingen Association mandated that any industrial company that wanted to get the top students from the disciplines of physics or mathematics, or the engineering sciences into their companies, could not get that unless they could demonstrate that more than 20% of their profits had, in fact, been allocated to research and development. They were otherwise not found worthy of being supplied with that kind of manpower.

Klein, because he had a very close working relationship with the Prussian minister of culture, Althoff, was able to quite rigorously control this situation, and was able to force those companies that did not want to comply into a situation where their competitiveness was, in fact, severely hampered.

Now, whether or not one wants to use that kind of model in the United States today is, I think, something you might want to debate and think about. But, in any case, the basic point here, I think, is very clear: that industry must make its contribution not only in the form of financial donations, but in terms of an actual, in-depth commitment to research and development, so as to be able to collaborate with the most advanced scientific institutions, so that there is not this tremendous and unnecessary gap between theoretical and applied research. And that was Klein's principal purpose.

He was able to enlist the heads of *all* of the large companies, from Krupp, to Siemens, to M.A.N. Any company of any size in Germany in the period before World War I became, at one time or another, a member of the Göttingen Association and collaborated in this program. It's this which made the developments possible which have been discussed and reviewed during this conference.

Von Karman and other villains

Now, let me review, in contrast to this, the type of approach that was taken by the second group of people. Some of you who have worked in the airplane industry and the space program, etc., may not only be surprised, but perhaps offended by the fact that I single out Theodore von Karman as one of the villains in this story, even though he admittedly made some significant contributions in certain areas. Von Karman, himself a Hungarian by birth, was a student of Prandtl at Göttingen. And Prandtl was instrumental in providing him with a professorship at the technical university in Aachen, in the westernmost part of Germany. In the initial years still directly under the influence of Prandtl, between 1908 and 1911, von Karman did quite excellent work there. In fact, much of the type of work on so-called vortex streets, vortex formations behind objects, and fluid flow, is due to the early work of von Karman. During World War I, he was drafted into the Hungarian Air Force, and he then returned to Aachen in 1920, to resume his post.

It is not quite clear what happens to one if one is drafted into the Hungarian Air Force, but whatever happened to von Karman was not very good. The actual scientific developments and the scientific initiatives that he took after his return to Germany, I think, are by and large, to be judged quite negatively.

In 1922, he organized a conference, along with others, at Innsbruck, Austria, in which he was the first to propose, directly in opposition to the geometrical approach of Prandtl, a statistical approach to the theory of turbulence. It was as a result of the disagreements that arose out of that—they did not really come very much to the surface or very much into the open, at least in these kind of disputes, scientists often tend to be polite, perhaps *too* polite, rather than bringing out these differences for everyone to see—but in any case, Prandtl quite strongly disagreed with this approach. It was directly contrary to his own way of thinking, and to his own insight into what had allowed him to succeed.

Prandtl blocked the appointment of von Karman to a professorship in Göttingen in the early 1920s. At that point, a different development occurred in the United States.

After World War I, it had become quite obvious that airplanes and similar kinds of high technology devices had already had a very significant influence in World War I, and might, in fact, become decisive if a new war were to break out in the future. At that point, various organizations of industry, as well as military organizations in the United States, realized that the actual level of physical science and of engineering science in the United States was abysmal, and attempted in the relatively shortest possible period of time, to remedy that situation. One of the principal protagonists, and there should be no question that it was the proper purpose, though, I think, badly executed, was Robert Millikan, who, in 1923, won the Nobel Prize for physics for his experiments with electron theory.

Millikan, at that point, or at least slightly later, became the leading physicist and, in fact, the leading organizer of the research at the California Institute of Technology. He collaborated very closely with Daniel and Harry Guggenheim, for the purpose of making money available for the development of research institutes, and also for the possibility of attracting researchers, primarily from Europe, and with emphasis on Germany, in order to remedy the backwardness of the United States situation as it existed under those circumstances.

In one way or another, it became known to Millikan that von Karman was getting disenchanted with his position in Germany, and by 1926, negotiations started between Cal Tech and von Karman. Initially, von Karman acted as a consultant in the construction of the wind tunnel at Cal Tech, and then later, in 1930, actually permanently moved to the United States.

Millikan himself did some useful experimental work, but his philosophical outlook on the scientific enterprise was essentially diametrically opposed to the kind of outlook that I have ascribed to Prandtl and others. His autobiography—mind you, this is not a biography I'm quoting, but an autobiography, so it reflects his own way of thinking—starts with recounting a little story when he is four years old. He is playing with his two-year-old brother, under their porch, in the dirt, playing with dust. He says, my younger brother picked up a bunch of dust and told me, "Well, *eat* it. One can eat this." And Millikan says, I didn't believe that, and I told him it's not possible, but my younger brother, at age two, did not want to believe me, so I told him, "Well, why don't you eat it yourself?" And the two-year-old picked up the dust and ate it, and then ran, screaming, to his mother.

That, says Millikan, is how he became a physicist. That is how he was first convinced of the value of the experimental method. Well, as I said, if I wanted to slander the man, I might have *invented* this story, but in fact, it is the first paragraph in his autobiography; so therefore, presumably, he was deeply impressed by this and somehow *believed* this kind of nonsense. Well, that's not how you become a physicist, or anything else. That's how you become a *fool*.

I want to read you a list of whom, later on in his autobiography, Millikan regards as his scientific heroes. This reads like a list of the villains that I showed you earlier, but somewhat amended. He regards Maxwell as the greatest genius in the history of science. He then lists Kelvin, Rayleigh, Helmholtz, Boltzmann, and J.J. Thompson. Now, mind you, this is a man speaking in the 1940s. There is not a single mention here of people whom, I think, we rightfully should regard as the greatest scientific geniuses of the 19th and the 20th centuries.

The problem is, that the scientific enterprise in the United States, even at a point when, quite correctly, it was realized that it was backward, then came under the guidance of an individual who had done valuable experimental work, but whose entire outlook and way of looking at the scientific enterprise, was so slanted and so wrong, so badly misguided, that there is no real surprise that his programs, in fact, did not prove particularly successful.

Now, in terms of the Guggenheim-Millikan enterprise, they decided that they needed somebody. The phrase they used, was "finding a scientist of ability, bordering on genius." They wanted to find a scientist with the ability bordering on genius, give him some money, and let him develop aerodynamics in the United States. And the one they found was von Karman.

Why did they hit upon von Karman, rather than Prandtl? Well, here's the actual quote from a letter: Harry Guggenheim had gone to Germany at that time in order to look for such a genius. He had gone to Göttingen, he had seen Prandtl's work, and for whatever reason, Guggenheim was impressed, and said to Millikan, well, "let's get Prandtl."

Millikan responded, "Dear Mr. Guggenheim . . . with respect to the suggestion which you made as I left your house, that we try to get Prandtl over here for a short time, I have talked the matter over at length with Epstein and Bateman. Both of them think that in view of Prandtl's advanced age [mind you, he was five years older than von Karman] and his somewhat impractical personality, he would be far less useful to us than von Karman."

And then, later on, a little footnote is added, where he makes some remark about G.I. Taylor and Britain. In fact, they preferred G.I. Taylor as well. That may not mean much to many of you, but to some of us who know about G.I. Taylor's work, it means something. But in any case, it says: "The other thing that speaks for von Karman, by the way, is that he is Hungarian in nationality. We have between us reached the conclusion, partially because of von Karman's nationality, that he would be the better person than Prandtl."

One of the most famous quotes that I have of Millikan, is also in his autobiography; this was right after World War I and perhaps understandable in the heat of the argument in some respects; he said, what we can't have in the United States is the German barbarism reflected in World War I, and we can't have people associated with scientific work, in Germany at that time—which was true for Prandtl who had a great deal to do with the development of airplanes. Then he said, "we Anglo-Saxons have overcome these tendencies toward barbarism. The British Empire, after ridding itself of some of its worst excesses, has become the veritable model of freedom and development in the world today."

So, this was the person who brought a genius to the United States.

The gist of what was the outcome of this, you could see at the end of World War II. From 1930 on, von Karman was effectively in charge of all of aerodynamic research in the United States. There was really nobody who could have challenged, in any way, negatively, or otherwise influenced, what he wanted to do.

In 1935 the so-called Volta Congress took place in Rome, a congress on aerodynamics and fluid dynamics, in which certain presentations were made, the primary ones by Adolf Busemann, whom I already talked about, and the other one by General Crocco, who was one of the principal aerodynamics researchers in Rome. Von Karman went to that congress, after he had been in the United States for five years and had gotten more money for developing aeronautical research at Cal Tech than the entirety of European institutes taken together. He came back with the impression that the Europeans were far ahead. And he made a report to this effect, but couldn't figure out why. He said, we seem to be doing what we should be doing, but somehow, we don't seem to be succeeding.

In particular, he was quite rightfully impressed with the fact that after four years of trying at Cal Tech, they had built a wind tunnel that was operating at several hundred kilometers per hour speeds, and something like 5,000 horsepower. When he went to Rome in 1935, he found a supersonic wind tunnel operating at twice the speed of sound, and with 20,000 horsepower. So he came back and was shocked and made the determination that all energies must be mustered to develop this work better in the United States. Nothing came of it.

In 1938, the question of jet propulsion was first investigated in the United States. There was some suggestion that jet propulsion should be a good way of driving airplanes. A committee was called together by the National Academy of Sciences, under the leadership of von Karman and Millikan, with the able assistance of Professor Marks of Harvard University. And they delivered their report on June 10, 1940. The report said, in essence, gas turbines are no good for flight because they're too heavy. Well, several months before that, the first model of the Messerschmitt 262, the actual German jet fighter of World War II, had already successfully flown and gone through much of the testing routine, etc.

Von Karman delivered a report of the impossibility of jet propulsion for aircraft, at the time when such aircraft were already flying in Germany! Well. He later on apologized and said that he just put his signature to this report, he didn't really read it. And then he said that when the report was issued in 1938, he was in Japan. He in fact was in Japan in 1938; however, the report was not delivered until 1940, so that doesn't make much sense.

The Army Air Force, in 1945, was quite shocked when they saw what they had found in Germany. Several people were sent over in 1945, to Germany, to investigate what was going on. Von Karman was one of them. He and another researcher from Cal Tech went to Germany, and then they questioned for long hours Prandtl in detail about what he had been doing. Adolf Busemann was questioned in detail about his ideas on supersonic flight.

After von Karman came back, he was asked by the NACA, the National Advisory Committee on Aeronautics, as well as by the Air Force, to deliver a report. And he wrote a report which said, we weren't really very impressed with what we saw in Germany. In fact, in many cases, the German work was good, but it certainly was not spectacular. Many of the things that have been praised, we were ourselves thinking about.

The Air Force did not issue the report. One of the top people in the NACA, Hunsaker, wrote a letter to von Karman, saying that, this seems to be a rather self-serving and nonsensical report, and you will make yourself a laughingstock of the world if you issue it. For your reference, said Hunsaker, I will list to you precisely those areas in which the Germans *were* ahead in 1945, and in which we did virtually nothing, and he went through it: Supersonic research, missile research, rocket research, jet propulsion, swept-wing design, and so on and so forth. And he just listed those areas, primarily in the field of aerodynamics.

So, the report was not issued, but von Karman was promptly charged by the Air Force to write another one, outlining the next 50 years of aerodynamical research for the United States. I don't know if that was ever written, or maybe it's a classified document. I hope it's so deeply classified that nobody will ever see it.

The question of method

That brings us to the fairly obvious conclusion. There's no question that the financial and material means at the disposal of the German effort in aerodynamics and related fields during and before World War II were in no way superior. What was superior and was different, was the type of outlook and the basic method that I have stressed here.

Von Karman is associated with the statistical turbulence theory and with the idea of using the classical hydrodynamic theory, making certain linear adjustments in it, in order to

FIGURE 3

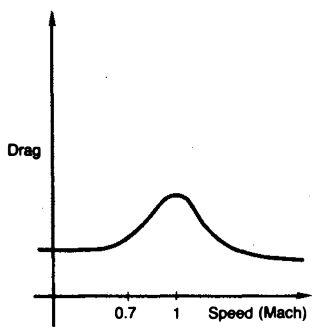
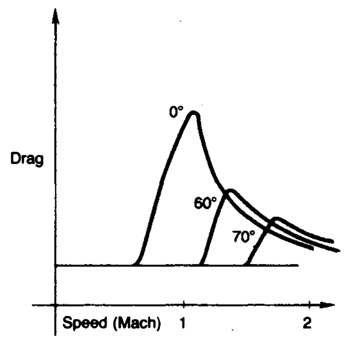


FIGURE 4



The so-called sound barrier has nothing to do with a barrier. If you get near the speed of sound to about 0.7 Mach, the drag coefficient on the airfoil increases very steeply, because shock waves develop that affect airflow over the airfoil. The critical zone for the development of shock waves that influence flight and lift negatively is at the 0° angle. If the wing of the plane is at right angles with the fuselage, you get the onset of the critical area at 0.7 Mach. But if you have a 60° angle of the wings, then not even half the drag coefficient develops, and if you have a 70° inclination you get a point where you get a very low, very late onset of the critical phase.

get away from the nasty singularities that plague this kind of research. He's associated precisely with the outlook which, if it is adopted in principle, will not allow any significant advances in the physical sciences, and has never, in fact, been responsible for the development of such advances. That is the very simple fact that we have to face.

It has nothing to do with Germany versus the United States, or anything of that sort. It has something to do with *method*. These points of method were shared by the people of the Ecole Polytechnique in France, they were shared by the group around Riemann, they were shared by the great hydrodynamicists of Italy in the tradition of Riemann, and they were shared by all of those researchers whose names I already mentioned, most notably, Prandtl and Busemann in Germany at that time. It is not a point, as Millikan says, of nationality.

In the postwar period, there are a number of important things to look at. I will not look at the rocket programs because they have been reviewed here competently.

Look at the so-called sound barrier (**Figure 3**). I object to the word "barrier" because it implies precisely that kind of collisional approach. It has nothing to do with barrier; there is no barrier, there is nothing there. There is just air, like anywhere else. The point is, that if you get near the speed of sound to about 0.7 Mach, then under those circumstances the drag coefficient on the airfoil increases very steeply, exponentially, until you in fact reach the speed of sound.

The reason for that is the fact that through the development of shock waves, which affect the airflow over the airfoil, a certain amount of the lift energy is converted into shock formation. That energy is taken away from the lift capability of the plane, and under those circumstances you experience various kinds of instabilities and difficulties with the plane itself, which have to be countered simply from the standpoint of understanding the problem—of making the kind of geometrical adjustments, in wing design, or anything else, that are necessary to do that.

One of the principal adjustments in wing design that can be made, was invented by Busemann, the so-called swept-wing design, the arrow design. You can see here (**Figure 4**) how the critical zone for the development of shock waves that influence flight and lift negatively at the 0° angle; that is, if the wing is at right angles with the fuselage, you get the onset of the critical area at 0.7 Mach and then the drag coefficient declines afterwards.

If you have a 60° angle of the wings, then not even half the drag coefficient develops and you get it also at a much later point; namely beyond Mach 1. And if you have a 70° inclination with the fuselage of the wings, then you get to a point, where you get a very low, very late onset of the critical phase. Also, the amount of reduction in lift or the amount of increase in the drag coefficient is not very substantial. It is there, it will always be there, because shock waves form.

Shock waves are real, as was certainly determined by these methods of research in aerodynamics that were carried out in the 1930s and 1940s in Germany, primarily under Busemann's direction in Braunschweig. They are not what Rayleigh had critically said, when he criticized Riemann's 1859 paper. He said, well shock waves do not exist, what exist, are singularities in the mathematical formulation of the wave equations, but we cannot assign any reality to such singularities. All it means, is, that we have failed to come up with a solution.

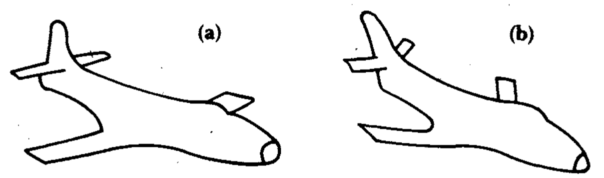
As Riemann said, these things *are* real, and he said it 50 years before Rayleigh made that idiotic criticism. It was precisely because of that realization of the reality of the shock waves, that when supersonic flight was studied in supersonic wind tunnels in Göttingen and Braunschweig, and later on in Munich, Lake Kochel, etc., that these things were taken into account.

Here is an interesting example (**Figure 5**). This is the Douglas D-558, which was developed simultaneously with the Bell X-1 as a supersonic design in 1945 before the von Karman mission went to Germany and interviewed Busemann and others. That was their design (a): a straight wing sticking out, so you have the 0° angle situation of before, a tail end that sticks up, just as in the old designs of aircraft in the subsonic range.

Then von Karman and others came back to the United States in the summer of 1945, and after the summer of 1945 the D-558 looked like (b). It was all of a sudden a swept-wing model with a swept-tail configuration etc.

In fact, one of the interesting stories that I learned several years back when visiting a scientific conference in Moscow, was that a Russian researcher showed me a picture of one of the models for a supersonic passenger jet type that the Russians had acquired when they moved into the eastern part of Germany. "What do you think that is? he said. I said, "Every-

FIGURE 5



The Douglas D-558, which was developed simultaneously with the Bell X-1 as a supersonic design in 1945, had a conventional straight wing (a). After von Karman and others visited Germany and interviewed Adolf Busemann, their design was modified to his swept-wing design (b).

body knows, that's the Concorde." But it was not the Concorde, it was a model built by Busemann for a supersonic jet, to which the Concorde design is identical—done in the late 1930s.

There is no mystery of any kind involved here. It is a simple and straightforward story, it's a question of method, both of scientific method and of method of organization. It's a question of assembling the kind of scientific team, which is capable, on the basis of the right kind of methodological approach, to find the mode of organization most appropriate to its goals, and simultaneously, as was pointed out by one of the previous speakers, setting your goals never with regard to so-called state-of-the-art designs, but in fact, setting them as far beyond as possible.

To the extent that you do that, you will be capable of changing this so-called state of the art rather than being stuck with it. What we have to do in any program, whether it is a crash technological development program or a basic research program, is to set our sight on the kind of goals and tasks that are way beyond what we initially anticipate the most immediate goal of the program to be. If that is not done, then we will not confront ourselves with the type of challenge that in fact is necessary in order for the scientific enterprise to succeed.

The lesson to be learned, is that we do not need state-of-the-art programs; that is nonsense, and leads to precisely the wrong approach. The cheapest programs are not state-of-the-art assembly programs; the cheapest programs will always prove to be those crash programs that look as far ahead as possible in order to accomplish the immediate task. This may appear to be quite expensive in the long run, if you have to bring in basic research and technology and design and all of that together into a program, rather than just saying, let's do the state of the art, on the basis what we have on the shelf. The latter is going to be the most expensive and the least workable approach, and I am afraid, to a significant extent, when we are talking about the SDI today, it is precisely that kind of approach to the situation, that is most problematical.

Concluding on that, I have to mention one other villain, who had something to do, not so much with the scientific side of these developments, but had a tremendous influence on this organizational side: John von Neumann, another Hungarian-born mathematician, who also studied at Göttingen and later came to the United States in the 1930s.

I have no time here to review von Neumann's career, even any significant aspects of it, but you probably know that he is associated in the minds of most, not so much with his mathematics and physics, but rather with his ideas in economic theory. In particular he wrote a book, along with Morgenstern, called *The Theory of Games and Economic Behavior,* viewing economic development essentially as a kind of competitive game between players much as players face each other in a poker game. In fact the first paper von Neumann wrote on economics so-called in 1928 was *The Theory of Parlor Games.*

The next thing, he studied in order to be able to model economic development in the late 1920s, was poker, and he invented a simplified version of stud poker and abstracted from a simplified version of stud poker his basic ideas of economic development. Don't underestimate the influence of this nonsense. What had come out of that, is the Rand Corporation, the Airforce Systems Command, and every single bit of so-called cost-benefit analysis optimization nonsense, that we are suffering from right now, and it is one of the principal problems, in order to be able to get defined and pushed through the kind of crash program for the SDI, that is desirable.

The other thing that has come out of it, is the famous McNamara way of "winning" the Vietnam War. You remember what that was: it was the body count method—cost benefit analysis applied to military strategy and tactics. You all were treated to that, most of you, I am sure, every night on TV: You had a body count, so many Vietcongs, so many North Vietnamese killed, so many Americans killed, the ratio looks good.

They made detailed analyses of how many people exist in each age group in Vietnam, to see how many people were being eliminated per day, and then the question was, how many troops do we have to put in and all to win on the basis of cost-benefit analysis? How much do we get out of it, if we put so many soldiers, so many tanks, so many this and the other things in, from the standpoint of linear programming and optimization analysis? How do we win? You can't win that way.

The principal strategic problem in military terms and otherwise in politics is the principle of the flank. The principle of the flank defies by its very definition the idea of cost-benefit analysis, and this has precisely to do with the unexpected, to put a tremendous amount of cost into one area, where it is unexpected, in order to be able to then succeed as quickly as possible. The very opposite of the kind of thinking, so much associated with von Neumann and much of the Pentagon thinking today, is what is called for under these circumstances.

If we keep that in mind, and let that be reflected in our political approach to these questions, we may have a chance.