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The Initial Development of Ablation Heat Protection, An Historical Perspective

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"...re-entry...is perhaps one of the most difficult problems one can imagine...It is certainly a problem that constitutes a challenge to the best brains working in these domains of modern aerophysics...possible means [include] mass transfer cooling, consisting of a coating that sublimates or chemically dissociates..."

—Theodore von Kármán[1]

Introduction

The purpose of this paper is to describe the development of ablation heat protection for hypersonic re-entry to which I made personal contributions. The perspective I hope to impart is three-fold: first, there is the importance of *connections,* that is, my technical experiences *prior* to my work on heat protection made it possible to apply techniques I previously developed to the solution of the heat protection problem. Second, the multidisciplinary aspect of the solution: the fundamentals of fluid mechanics, heat transfer, materials, and mechanics of materials were all applied to achieve a new solution. Third, I discovered that a doctorate degree is only a learner's permit. Throughout the paper, I have tried to indicate primarily the "firsts" that were accomplished; I have therefore not attempted to review the entire field of ablation heat protection.

Connections

My first contact with the world of rocketry occurred in 1953 as a student summer employee of the Jet Propulsion Laboratory at the California Institute of Technology. Working at the Jet Propulsion Laboratory gave me an unusual opportunity to learn about rocketry, heat transfer, and cooling techniques. I was placed in the liquid rocket engine development section, where I was given an assignment of devising a method of measuring the heat transfer in rocket nozzles, particularly at the throat where the heat transfer is greatest. My recollection is that the designers of the injection plates were trying to determine the optimum fuel and oxidizer injector arrangement which would limit the throat heat transfer while still achieving high combustor efficiency. However, combustor tests which used regenerative cooled rocket motors were very expensive and time consuming—since a poor injector design led to a coolant failure, which then caused a structural failure, and the data (if any) was destroyed. To reduce costs, my supervisor, Tony Briglio, had in mind the use of an uncooled steel motor which would only burn about a second. This was not long enough to cause any thermal damage to it, but a method was needed for measuring the heat transfer to the uncooled throat.

I devised a plug heat sink gage with a single thermocouple attached to the unexposed end. Then, by assuming a constant heat-transfer rate, its value could be determined by comparing the thermocouple readings to solutions to the heat conduction equation. I had always been bothered by the necessity for assuming constant heat transfer, but because of the diffusion property of the heat conduction equation, one cannot invert it to obtain the unsteady transfer from temperature measurements taken below the surface. I returned to this problem sporadically, and finally theoretically solved the problem by representation of the heating rate as finite polynomial series, solving for the coefficients in terms of the measured thermocouple response.[2]

At the JPL I also learned about forced convection nucleate boiling which was the method in general use to cool regenerative cooled liquid rocket engines; three aspects of which I used later. First, the cooling tubes were pressurized, at a pressure *higher* than the pressure in the combustor, in order

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to feed the coolant into the injector. This meant that the cooling tubes could be placed adjacent to the hot flow, and if they had a circular cross section, then each one could be designed for hoop stress only—meaning that the wall thickness of the tube could be quite thin. Low strength, high thermal conductivity materials which minimized the temperature difference across the tube wall could therefore be used. Second, the tubes were placed as generators of the engine shape, that is, the coolant flow was axial, and the tubes were bent into an arc which formed the throat. This caused a centrifugal pressure gradient in coolant at the throat, with the pressure highest at the side adjacent to the hot flow, which permitted the bubbles formed on the heated side of the tube during nucleate boiling to be transported to the cooler side and to recondense. This minimized the possibility of film boiling with its lowest heat-transfer capacity and thus avoided burn-out. Third, the arrangement of cooling tubes on the inside of the engine permitted the placement of the structure on the outside, where it would remain cool and retain its strength. Thus, the design *separated* the cooling from the structural functions; that is, the different pieces of metal were used for each. These concepts were later incorporated into the design of my ablation specimen test holder, probably more as exercise in elegance than necessity.

There is one particularly interesting anecdote which connected my graduate courses and my summer employment at the Jet Propulsion Laboratory. New employees at the JPL generally read a book on the elements of rocket propulsion authored by H.S. Tsien. I decided that I would like to take a course taught by him, which turned out that year to be Engineering Cybernetics, based on his newly published book of the same title.[3] Tsien's classroom technique was to derive, on the blackboard, the equations contained in the text. Questions by the students were rebuffed irritably by him, to the effect that the student should have studied the book since the answer to the question was contained in it. Then he would continue writing on the blackboard. One day, a student had a question on the ballistic missile guidance equations, in which several simultaneous equations were to be solved simultaneously for the guidance correction. The question related to the determinant of the equations: it appeared to be zero. After castigating the student in his usual manner, he gave the class the assignment of checking the value of the determinant. Most of us also calculated zero. Tsien himself at the blackboard also obtained zero. The reason turned out to be that one equation was redundant, with the consequence that there was a major error in the derivation. The fascinating aspect is, that after that incident, Tsien's attitude toward the class changed completely; he became friendly and showed more humility, which demonstrated that he, too, was human.

Finally, at the JPL I learned of a then-secret proposed Air Force project, called Atlas, which was to be the U.S.'s first intercontinental ballistic missile. It is only by a coincidental combination of circumstances that I was later given the opportunity to make a contribution to the project.

Multidisciplines

My introduction to hypersonic heat protection occurred during my first full time industrial position, in 1955, at the Lockheed Missile Division Research Laboratory, then situated in a converted hangar at the Van Nuys, California airport. I had been hired by Dr. Joseph Charyk, ostensibly to conduct research on materials for missiles. My degree was in mechanical engineering and physics, and although I had taken some courses in fluid mechanics, I felt more comfortable in applied mechanics and materials. (Lasers had not been invented yet: population inversion, which was covered in my graduate course in physics was regarded as a paradox of negative temperature instead of something to be used.) To my surprise, my first assignment was to work on heat protection concepts for a proposal for a parallel effort to the Atlas program. Several re-entry heat protection concepts had been proposed already: heat sinks of high-conductivity materials, liquid internal cooling, ceramics (which might ablate), and transpiration cooling seemed to be the available choices. Initially, I spent most of my time calculating—numerically—the temperature profiles in various possible heat sink materials. They were initially favored because their response could be calculated accurately. I worked out a simple, analytical result for the temperature profiles during entry,[4] probably to satisfy my curiosity that a closed form explicit solution should exist. Of the metals, beryllium had attractive theoretical performance; but copper was selected.

The liquid convective cooling concepts involving nucleate boiling, which works well for rocket motors where all the coolant is used in the combustor for propulsion, seemed ill-suited for re-entry heating because nucleate boiling did not take advantage of the thermal effect of the phase change from liquid to vapor. If the phase change was permitted, one then had a vapor in contact with the hot surface, with a decreased heat-transfer coefficient, which was unacceptable. Transpiration cooling was theoretically very attractive, but had not been developed sufficiently. Ceramics appeared too fragile and subject to thermal shock. I had worked one summer for the Foster Wheeler Corporation on process plant instrumentation, part of which involved a very unsophisticated stress analysis: a requirement was that on *anything* which went into the plant a 200 lb plumber had to be able to stand, because in all probability he would! I devised a corollary to that: the heat shield would also have to survive a wrench being dropped on it. Hence ceramics were too fragile. (Some success was later achieved with metallic-honeycomb reinforced ceramics.[5]) This left primarily heat sinks. Graphite was a particularly attractive choice, because its strength and thermal conductivity increase with increasing temperature. But I could not find a good solution for fastening the graphite to the substructure, because the predicted backface temperature, for a reasonable thickness, became very high and in addition, I was still worried about the falling wrench.

Lockheed did not win the contract,* and I was assigned to work on another proposal, for the Fleet Ballistic Missile. I wrote the heat protection and other portions. It was a small effort—only a handful of people were involved, but it was successful. About this time there was a management reorganization at Lockheed and I was given the opportunity by Leo Steg to join the General Electric Space Sciences Laboratory, which was then being formed to conduct research applicable to the hypersonic re-entry. I had become interested in heat transfer from dissociated ionized gases, so in early 1956 I found myself in Schenectady, N.Y., where I spent some time educating myself on boundary layer heating theory and its extensions to the dissociated ionized gases. I also found that I could contribute little to the Atlas re-entry vehicle design. A decision had been reached to make it a copper heat sink and available laminar heat-transfer theory and measurements appeared adequate.[6-8] However, there was another unresolved problem: during re-entry, the degree of ionization in the shock layer was sufficiently high so that there was serious question concerning the attenuation of radio telemetry through it. The engineering department at G.E. decided to investigate the design of a "data capsule," which would have a small recorder, and its own re-entry heat protection system. In case of a re-entry vehicle failure, the data capsule would be ejected, survive re-entry, and be recovered by a surface craft, with the critical failure data recorded within it. A design problem was weight, which made it impossible to consider a heat sink. My immediate supervisor, Dr. Joseph Farber, asked if I would like to work on finding a suitable heat protection material, with a budget of $75,000. I accepted the assignment.

*The re-entry vehicle development contract was awarded to the Avco Research and Development Division, then at Stratford, Conn.

I reexamined every material I had previously considered while at Lockheed and more. Beryllium oxide, a potential ceramic under investigation by the Chicago Midway Laboratories for G.E., appeared too brittle (because of the wrench criterion). I obtained copies of the Peenemmünde documents, and discovered the work of Carl Wagner, who calculated the heating and oxidation of graphite as a nose cone material. I looked carefully at the materials tested for jet vanes, which also must endure a high-temperature, high-pressure, and high heat-transfer environment. I read about oak, wet oak, graphite, etc., in reports signed by Werner Von Braun, and later personally discussed these with him. They all worked, but not very satisfactorily.

G.E. previously had a missile division, which made liquid propellant rockets, which, like those at JPL, used jet vanes for initial steering of the rocket at lift-off. As part of the G.E. project, tests were conducted of jet vanes made from plastic laminates, of which G.E. was a leading commercial producer. These were composites of alternate layers of glass cloth and thermosetting resins. I learned of this work through Dr. Bernard Levine who had been involved with the G.E. rocket project, and was now Manager of Advanced Engineering at G.E.'s Reentry Systems Division. The test results were quite promising, and reinforced plastic jet vanes were incorporated into all Hermes A-3 rockets. But in some tests, the laminates had failed primarily due to delamination.

I then conceptualized a technique to avoid delamination. I theorized that the heating would char the resin into a carbonaceous mass of relatively low strength. The role of the fibers should be to hold the carbonaceous char to virgin, unheated substrate. Here, low-thermal conductivity was essential to minimize the distance from the hot, exposed surface to the cool substrate, to minimize the mass of material that had to be held by the fibers as well as the degradation of the fibers. The char itself would eventually either be vaporized or be oxidized either by boundary layer oxygen or by CO_2 in the boundary layer. The fibers would either melt or also vaporize. The question was how to fabricate the material so that the fibers interlocked the resin, which was the opposite design philosophy to existing laminates in which the resin interlocks the fibers. I believed that a solution might be the use of short fibers, randomly oriented in a soup of resin, which was then molded into the desired shape. I then began to plan the experiments to test this hypothesis.

I communicated with Tom Jordan, who had been involved in fabrication of the composite jet vanes at the G.E. General Engineering Laboratory. Several techniques for producing short fibers were selected for testing: plain chopped fibers, chopped cloth, and chopped "roving." I had decided that the fibers must be refractory to hold the charred resin. This meant that they had to be oxides, for which the choice was limited to glass (mostly silicon dioxide), Refrasil(R), which is almost pure silicon dioxide, and Fiberfrax (R), which is aluminum oxide fibers. Asbestos was also selected primarily out of curiosity, since it suffered from being mechanically very weak. In addition, it is a water hydrate, and I visualized that once it became hot, it would lose its water molecules, which would expand by orders of magnitude into steam. The fibers would simultaneously lose their strength and the expanding steam would blow off the char. If the asbestos failed, it would help confirm the hypothesis that fiber strength was important. For resins, I selected a phenol-formaldehyde known as Cincinnati Testing Laboratory's 91LD. In addition, melamine, which is a urea-formaldehyde, was selected because G.E. and others were fabricating laminates with it. Finally, some newer silicone resins were chosen.

During this time period I studied the properties of fibers, and properties and fabrication of plastics, to more thoroughly understand their subsequent physical properties. The high-tensile strength of glass fibers (300,000 psi) was attractive for the application. However, to prevent the fibers from cutting each other, a thin layer of starch was sometimes placed on each fiber. It was essential to remove the starch prior to molding to achieve good adherence to the resin. In addition, additives were used in the glass to reduce its viscosity, so that it was easier to draw the glass into fine fiber. This low viscosity was a potential detractant for the application. On the other hand, the viscosity of Refrasil® was very high, approaching that of pure silica. But Refrasil® was, at that time, quite weak mechanically because it was made by leaching impurities out of glass fiber. The Fiberfrax®, like asbestos, was quite fragile; and although my colleague at G.E., Willard Sutton, later grew alumina crystal fibers of exceptional strength, they were not available at the time.

Of the resins, the phenolic had the highest-temperature capability; and since it was cross linked, it reduced to a char with some structural integrity, especially if there were refractory fibers in it to hold it together. The phenolic resin must be heated and held under high pressure to achieve the cross-linking. During this time the viscosity of the resin is very high, so it is difficult to mold. Finally, it is necessary to release the steam formed by the reactions of the resin. The other resins were generally easier to mold.

My plan was to have hemispherical shapes molded and to test them in a rocket exhaust. I emphasized that the specimen had to be a monolithic molding—all in a single piece. It could have been made of small tiles cemented to a substructure, since small cracks in the surface would probably not bother the ablation process. But adhesives for bonding tiles to the structure had low strength—perhaps 500 psi. I was concerned about the prototype—the failure of the adhesive for one tile would mean failure of the entire vehicle. Thus, the entire heat shield had to be monolith construction, and I wanted this feature to be demonstrated by the small models.

A diameter of one inch was chosen to both fit in the rocket exhaust, and also to simulate the heating and pressure gradient, considering that the rocket exhaust had a far lower total enthalpy and pressure than the flight condition. I also had molded into each hemisphere a thermocouple, below the surface, so that I could determine the thermal diffusivity of the specimen during ablation. The tests were to be performed in the supersonic flow at the exhaust plane of the rocket nozzle. That way, a curved shock was formed around the spherical model, similar to free flight. I later learned that the shape of the ablation specimen being tested by the Army at the Redstone Arsenal was conical; it was placed inside the first shock diamond so that the oblique shock waves were parallel to the cone surface. But this geometry entailed a risk which I chose to avoid: there is usually a small normal shock region at the centerline of the shock diamond which reduces the stagnation pressure below the original total pressure. The flow through oblique shocks of the diamond pattern does not suffer this same change, and I was uncertain about the overall effect on the flowfield simulation. In addition, the data capsule shape was spherical, not conical.

While waiting for the ablation specimens to be fabricated, I proceeded with establishing the ablation test procedure. The General Electric Company operated the Malta Test Station† which had liquid rocket test stands and some test rockets. At the time, G.E. was developing liquid rockets for the Viking rocket. I met with the director of the test station who agreed to perform my ablation tests. At the time, he was having severe difficulty with throat burnout of liquid propellant rocket motors related to the Viking project. It appeared to me that the engine was designed reverse from those at the JPL; the structural steel was on the inside with the coolant tubes on the *outside*. The director told me about the burnout problem and that he believed the burnout was caused by radiation from the products of combustion. He planned to cure the burnout problem by chromeplating the steel interior to reduce the absorptivity. He then told me that people with Ph.D.'s were useless and knew little.

I remained silent, since it was more important to get my tests performed than to engage in a fruitless argument. But

†Now owned by the State of New York.

that discussion established my determination to design the specimen holder for the ablation specimens based on nucleate boiling, and I deliberately chose a design in which the holder would be transverse to the flow (instead of a rear sting) to demonstrate how well my holder design could work. The structural element was a steel rod (see Fig. 1), with a helix cut into it. A thin-walled (0.079 cm = 1/32 in.) copper sleeve fit over the steel rod, and water flowed through the helix with centrifugal motion keeping the liquid water against the copper sleeve. The helix area, water flow rates, and pressure were selected on the basis of calculations to assure nucleate boiling heating transfer. The only problem was the design required a water pressure higher than that normally available at Malta. Fortunately, a water pump intended for fire fighting did the job. The rig, with its flimsy outer copper shell did not inspire confidence during preparation of its first hot test in a rocket exhaust. Fortunately, (particularly for me) it not only worked well, but held together for many years of ablation testing. At this point, having expended the available funds, I requested and received approval for an additional $50,000.

The next step was to measure the stagnation point heat transfer. I returned to the measurement concept that I had developed for rocket nozzles. In this case, it was to be a small slug of silver embedded at the stagnation point of a dummy ablation specimen. During its first test, the slug melted prematurely, and at a very low temperature. This led to an inspection of the precious metal bin and the discovery that all that glitters is not silver: the silver had been substituted with tin! Replacement with silver permitted the measurements to be completed. The measured heat-transfer rates were higher than predicted, possibly caused by the acoustic noise and pressure fluctuations associated with turbulent combustion. But the higher heating rates permitted a more severe test, hence were satisfactory. The tests of the ablation samples then proceeded smoothly, with most of the thermocouples giving good data prior to their being exposed and burned out. I measured the recession to calculate the mass removal rate, per unit exposed area; divided that by the measured heating rate, first defined this ratio to be the "effective heat of ablation," or h_e. It was the effective amount of energy absorbed per unit mass of ablation material. The concept of the heat of ablation has been carried forward to this day, and even applied to material removal by lasers, but the symbol was changed from h_e to Q^*.

The results were quite astounding. The experimental results showed that this was *higher* than the sum of the latent and sensible enthalpy increase of the same material. Figure 2 shows the original post-test photographs of the specimens. The Refrasil® based material performed the best with either the DC2106 silicone or the 91LD phenolic resin, about 15 kJ/g; the glass was less than half that, at 6 kJ/g. The crack in specimen 8 precluded the use of the silica fiber until stronger fibers were developed. The motion pictures showed interesting phenomena. The glass reinforcement melted and ran downstream. The Refrasil®, on the other hand, even when molten did not flow, which reflected its higher viscosity. This raised the possibility of vaporization of the silica with additional cooling effect.

During this time there were some significant program changes. Programs were initiated for the Thor and Jupiter medium range missiles, and the Army was known to be developing ablation heat protection for the Jupiter based also on the jet vane experience, while the Air Force continued with the heat sink for the Thor.

My work on ablation became of some concern to the Air Force sponsor since it could appear that we were merely copying Army work; and in the inevitable future selection of Thor vs Jupiter, this could count against the Air Force. Actually, the copper heat sink was adequate for the Thor, so the question of ablation heat protection for the Thor never came up. But the inquiry provided an important opportunity. Additional information concerning transition from laminar to turbulent heating had been obtained, from which it was predicted at G.E. that the copper heat sink was inadequate as a heat shield for an ICBM. This provided the essential justification for increased emphasis on alternative heat protection concepts. My experimental results were presented in June 1957 at the Second Technical Symposium on Ballistic Missiles.[9] At the end of my talk, Dr. George Solomon, then at Ramo-Wooldridge Corporation, rose to his feet and stated that ablation was the solution to the heat protection problem.

I believe that the essential element which distinguished my work from the Army work at Redstone was my measurement of the heating rate in the absence of ablation; my recollection was that the Army tests were primarily qualitative in that they merely selected the *best* material, but did not relate the results to the heating environment quantitatively. However, some months later the Army held a briefing (an invitation was not extended to me) in which the Army presented $1/h_e$; that is, the amount of material needed to provide protection per unit heat input, g/J. The Army later adopted my definition.

Shortly thereafter, in August 1957, the Jupiter did make a successful re-entry after an intermediate range flight, carrying a letter addressed to President Eisenhower from General Medaris, proclaiming it to be the first object retrieved from outer space. Although the re-entry conditions (flight range of 1100 n.mi.) was considerably smaller than for the successful re-entry of the G.E. data capsule and "ABLE," the adoption of heat protection by the Army helped gain its acceptance.

Learner's Permit

Although I was quite optimistic about applying ablation heat protection to higher speed re-entries than the Jupiter, there were several technical questions that needed attention. First, a quantitative theoretical basis was needed for the ablation of reinforced plastics, including pyrolysis of the resin and melting of the glass. Second, the large heat of ablation needed to be explained. Third, I was concerned with the presence of the hydrocarbon products of pyrolysis in the boundary layer, and possible addition of heat load due to their combustion with atmospheric oxygen. I enlisted the aid of my office mate, Sinclaire Scala, who at the time was

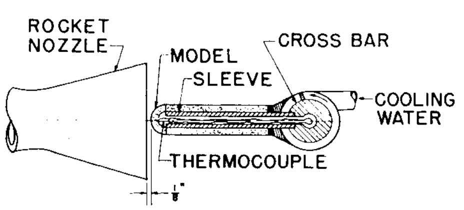

Fig. 1 Schematic diagram of original rocket exhaust—ablation test apparatus, including the nucleate-boiling cooled specimen holder.[9]

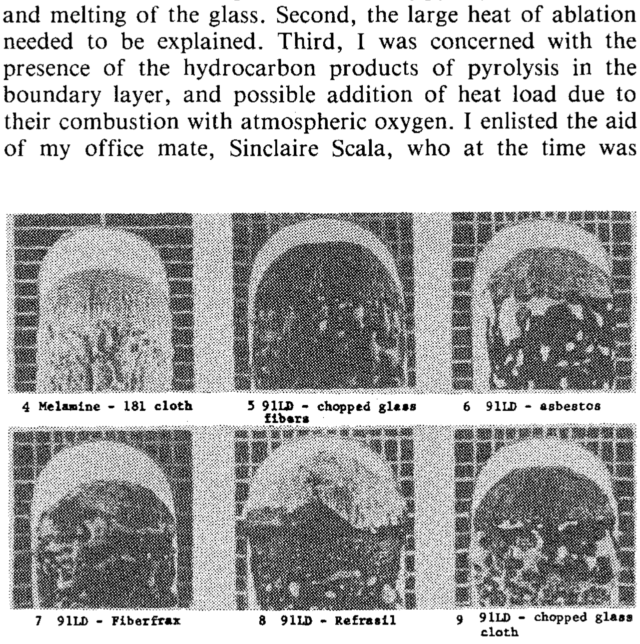

Fig. 2 Ablation models after test. Original diameter was 2.54 cm.[9]

working on exact solutions to the hypersonic boundary layer.

The first question we jointly investigated was the effect of the motion of the surface of molten glass on heat transfer with the mass transfer due to the pyrolysis of the resin. At that time, the work on ablation was still classified, so we called it "vectored injection"; that is, at the interface between the air boundary layer and the surface, there were two components of velocity, the usual normal velocity associated with mass transfer plus the tangential velocity associated with the flow of the molten glass. The result was that the glass speed was so low that it had little effect on the heat transfer,[10] but for Scala and I, it was an important educational process. We had been wondering about how to solve quickly the nonlinear ordinary differential equation for the boundary layer which is obtained from the similarity condition. The best numerical tables for the laminar boundary layer at the time had been obtained by Howard Emmons,[11] by rearranging the equations in integral form, and using successive numerical iterations. That technique was too time consuming for us, but again, circumstances were fortunate for Leo Steg had obtained the consulting services of Myron Tribus, and we posed the question to him. He stated that one uses an analog computer, and showed us how to solve $f''' + ff'' = 0$ on a differential analyzer. This involved literally wiring the patch board of the REAC to differentiate the signal successively with respect to a multiplier and a summer. A Brush pen recorder was used to plot f'. $f''(0)$ was adjusted until $f'(\infty) = 1$. Then one obtained the solution. For our ablation work, three equations were involved: momentum, energy, and diffusion, instead of one; hence there were three unknown initial conditions. To find them, we started with three guesses for the unknown initial conditions. Then we obtained a solution which did not satisfy the correct final conditions as the variable (time)$\rightarrow \infty$. We then varied each of the initial conditions by a small increment, one at a time, and calculated the change in the final condition. Thus, we found an influence coefficient matrix. By matrix inversion we then found (approximately) the three correct initial conditions, which would converge after a few iterations. This procedure was essentially the Newton-Raphson technique, but we did not know that at the time.

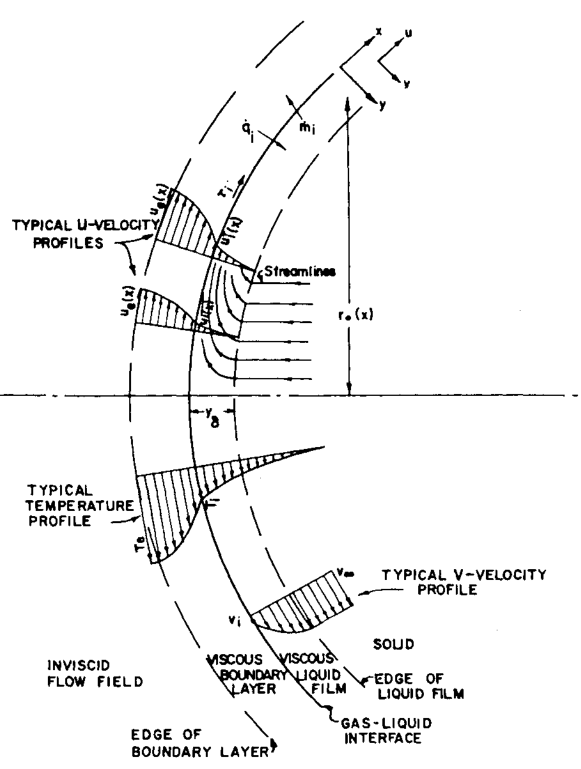

Fig. 3 Original diagram for melting ablation.[16]

The question concerning combustion in the boundary layer was largely solved by Richard Dennison and Don Dooley[12]; similar results were obtained by Lester Lees.[13] Essentially, boundary layer combustion increases the enthalpy at the edge of the boundary layer by the product of the enthalpy of combustion and the concentration of oxygen in the air. At the same time the coefficient of heat transfer is reduced by the increased boundary layer thickness caused by the positive mass transfer of combustibles. For low speed flight in air, the former can be very large; hence, the net heat transfer to the surface is increased by a very large amount. But for a hypersonic boundary layer, the added term is small, and the reduction in the heat-transfer coefficient more than compensates for the combustion effect. I embellished this theory slightly by including finite reaction rates.[14] More importantly, Scala and I derived the correct boundary conditions at an interface including diffusion and reactions.[15] With the problem of combustion of the pyrolysis resolved, I returned to the question of the role of the melting glass.[16]

There were available some theoretical analyses of one-dimensional heat conduction and melting, with and without melt removal, which concentrated primarily on the transient aspects. The solutions did not deal at all with *how* the melt is removed aerodynamically, nor any possible effect of mass transfer on reduction of the heating. It seemed to me that the molten glass would form a flowing boundary layer, as is shown in my original figure, Fig. 3; which has been copied many times since. The equations for the glass layer were formulated as a liquid laminar boundary layer, which needed to be matched to that of the air boundary layer, with gaseous mass transfer in between. The difficulty was that *none* of the interface parameters were known a priori; neither the mass transfer, tangential velocity, nor interface temperature; the latter because glass does not have a definite melting point, but its viscosity merely decreases as its temperature is increased. I curve-fitted an Arhennius-type temperature dependence on the measured viscosity for Pyrex glass, as an example, and omitted the pyrolysis and vaporization as a simplification. The results showed that the molten glass carried the heat energy absorbed from the aerodynamic heating downstream. Shortly afterward, Scala and I working together matched the mass transfer due to pyrolysis and vaporization as well, to obtain the first complete theoretical solution.[17]

We shared our knowledge of ablation with the Avco Everett Research Laboratory, at which the first measurements of hypersonic heat transfer had been made,[8] and at which advanced heat protection techniques were now being investigated. These techniques included phase change concepts, liquid metal cooling, transpiration cooling and perhaps some other concepts, but not the ablation of reinforced plastics. Hans Bethe and Mac Adams nevertheless produced an approximate theoretical solution in a short time,[18] which they prefaced "Material ablation is recognized as a powerful heat protection for high-speed applications." They were referring to the specific type of ablation material—a composite of glass and plastic for which I had already demonstrated the feasibility.

The essence of their solution was to neglect second-order interactions; and to approximate the normal component of glass flow as a constant. This permits an analytic solution for the temperature profile similar to that for a subliming solid, which permits, in turn, the determination of the glass viscosity, and hence the glass tangential velocity. This was then used iteratively to obtain the glass normal velocity from the continuity equation.[18] It was interesting to me since I had previously thought it might be possible to approximate that the glass normal velocity decayed exponentially, becoming zero at the surface, instead of the above approximation; and while I worked out the glass temperature profile and tangential glass velocity, I discarded the result since I could ascertain its accuracy only by solving the equations precisely.

The next step was to obtain a rational explanation of the large effective heat of ablation, as was being both measured and predicted by the precise theoretical solutions of Scala and myself. These values were larger than the sensible heat rise plus latent heat of phase change of the ablation material. Lees deduced the reason from the structure of the equations, which was that the enthalpy difference, from the edge of the boundary layer to the wall, was reduced by the derivative of the heat-transfer coefficient with respect to the mass transfer blowing parameter.[13] It was well known that mass transfer increased the boundary layer thickness, which in turn, reduced the heat transfer. For a range of mass transfer, this reduction is linear with the mass transfer, and the net reduction is related to the product of the slope and the edge-to-wall enthalpy difference. In other words, the enthalpy change of the ablation material was effectively increased by this product.

The final problem involved the effect of surface roughness on premature transition to turbulence. The original metal heat shields had been finely polished to delay transition, but the ablating surfaces were always rough and at high temperature which promoted transition. But I realized that future re-entry vehicles would maintain their speed at lower altitudes which increases the Reynolds number, hence the overall effect of boundary layer tripping and premature transition would be negligible. The inverse was more important; it was the development of these ablation materials which made possible re-entry at higher Reynolds number.

More Learning

My development of ablation materials was originally intended for the G.E. data capsule, which was flown and recovered successfully in June 1958. However, the design of the capsule was based on flotation, which meant that the heavy heat shield had to be scuttled after water impact. Thus, the heat shield was never recovered. Similarly, the G.E. "ABLE" re-entry vehicle with an ablation heat shield re-entered successfully (with a live mouse) in July 1958, but was not recovered. After the Air Force (Western Development Division) realized that the metallic heat sink was limited in performance, General B. Schriever authorized flight tests of a new experimental re-entry vehicle, the RVX-1, with flights for both G.E. and Avco. The Project Engineer at G.E. was Walter Schafer, and he consulted with me about the test. Since several materials had given acceptable ablation results in the laboratory, he wanted to try several materials on the first flight, instrumented with ablation (breakwire) sensors. I suggested that each material be a 60 deg segment with the same material repeated every 180 deg, to achieve symmetry. In retrospect, I think we were lucky, because the use of three different materials substantially increased the probability of a flight failure.

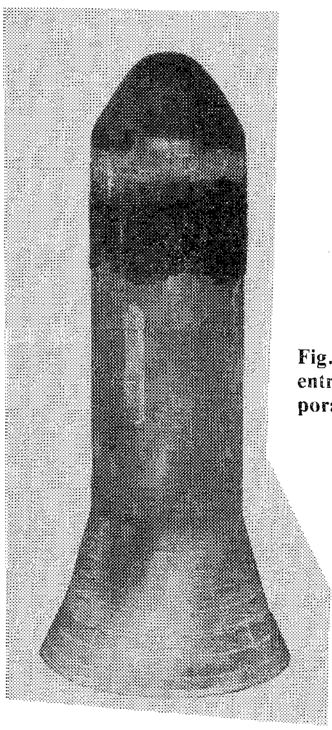

Fig. 4 Recovered RVX-1 ablation re-entry vehicle. Courtesy Avco Corporation.

G.E. designed the vehicle and recovery system; Avco was given the first flights to test their ablation material. The Avco RVX-1 successfully re-entered in April 1959, as did the G.E. RVX-1 both of which were successfully recovered (Fig. 4). Later I learned that the Avco ablation material used silica fiber as opposed to the stronger glass fiber on G.E.'s RVX-1. But it held together and had lower ablation, as had been predicted, see Fig. 5.

The development of ablation materials accelerated after these successes. Dr. Irving Gruntfest of G.E. suggested the use of nylon hydrocarbon polymer fibers instead of glass or silica fiber, based on the concept that it would release more gas of lower molecular weight, thus giving a higher heat of ablation.[19] Finally, the development of carbon fiber and techniques to carbonize a resin completed the cycle, resulting in carbon-carbon heat shield material, with desired low thermal conductivity and high-impact strength.[20]

Ablation test apparatus also improved in this time period, due to the introduction of the electric plasma jet. Visitors from the Redstone Arsenal, in 1956‡, called our attention to the development by Maecker in Germany of the wall-stabilized electric arc, otherwise known as the plasma jet.[21] At G.E. there had been a small group who were trying to develop a method of using an electric arc to heat flowing air; they were largely unsuccessful. Bernard Levine requested the Chicago Midway Laboratory to build two of the Maecker arcs, one for themselves to help in their subcontract to G.E., and one to be sent to G.E. It was quite small, but it did accommodate small samples. It was used to verify the previous results, and permitted some tests of refractory materials such as silica and graphite. During this time period, G.E. had a visit from Pete Rose of the Avco Everett Research Laboratory, but it was understood that the plasma jet was not to be shown to outsiders until G.E. fully evaluated its utility. But our signals got crossed and Dr. John McGinn of G.E. put on a spectacular display of this little plasma jet. Within a year, Avco had a megawatt plasma jet, with which they did most of their ablation testing, and G.E., not to be outdone, soon had a larger one. Many groups eventually built small ones, which led to the next important step, demonstration of the feasibility of conical shaped heat shields, which ablation materials made possible.

Up until then, only blunt shapes were considered to be practical because the stagnation point heat transfer was inversely proportional to the square root of the radius of curvature of a meridian. (Of course, as the radius of curvature went to zero, the heat transfer did not go to infinity, but went to the "free" molecular value which was extremely high.) It therefore appeared that the conical re-entry vehicle was not possible. However, at a small company, Plasmadyne, which had one of these plasma jets, Rolf Buhler noticed that if one started with a blunt graphite specimen, it became pointed.[22] This result was disputed by some who interpreted the result as due to cold flow core in the middle of the jet, but I tried—and succeeded—in predicting the experimental result,[23] (see Fig. 6). (While I was not personally effective in promoting the concept of conical ablation shapes, I learned later that one of the persons who had been skeptical of the result successfully did promote the concept.) This result also provided added confidence for predicting the performance of carbonaceous materials, which were later analyzed in considerable detail by Scala and Gilbert.[24]

The development of pyrolytic graphite attracted considerable interest. This form of graphite is made by placing a high-temperature form in an atmosphere of gaseous

‡I believe these were Gerhardt Heller and William Lucas.

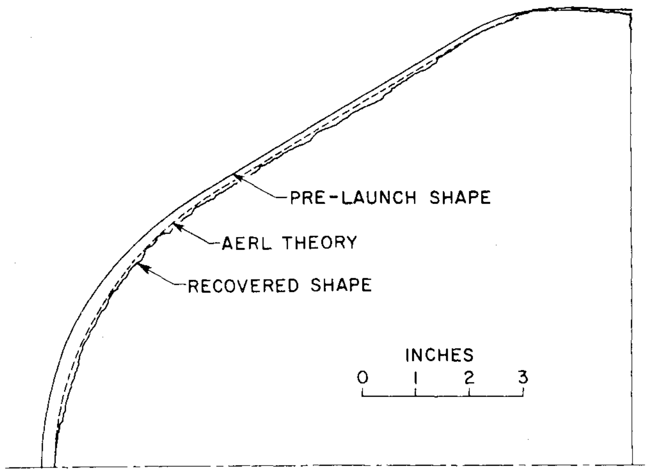

Fig. 5 Cross section of RVX-1 and ablation calculated by Dr. Henry Hidalgo and Mac C. Adams.

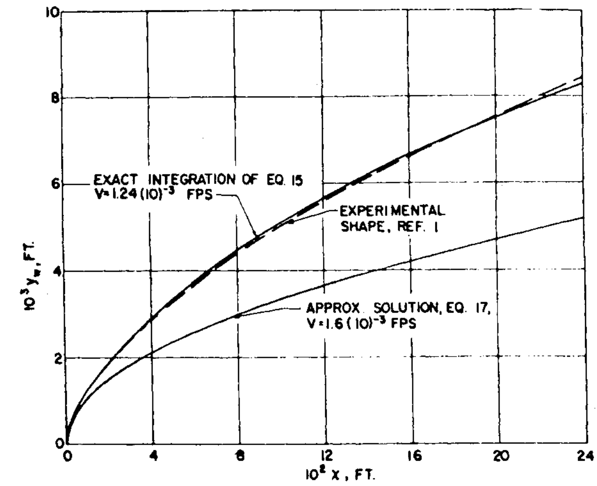

Fig. 6 Calculated and measured graphite shapes.[23]

hydrocarbons. The hot surface pyrolyzes the gas molecules which impinge upon it, leaving the carbon on the surface. The resulting material built up into graphite, with its high-thermal conductivity surface parallel to the surface, and the low-thermal conductivity direction normal to the surface. This material allowed the rear surface to remain cool,[25] but it was still brittle.

Teflon was another potential ablation material; it had originally been suggested for heat shields by Richard Porter, who had previously been head of GE's missile division. It was subsequently tested at low Mach number. Although its heat of ablation was quite low, I was interested in discovering the physics behind its ablation, primarily to determine its effectiveness for satellite re-entry in comparison to quartz and graphite. Dr. Henry Friedman at GE informed me that Teflon pyrolyzed as a first-order reaction with an Arhennius temperature dependence.[26] I modelled the Teflon such that if depolymerized gas evolved under the surface, then it diffused out through the Teflon, but the remaining polymer shrank to keep the density essentially constant. This, coupled with the heat conduction equation, permitted me to solve for the *square* of the recession speed as a function of the Teflon surface temperature.[27] Thus, the relation between recession rate and activation energy for pyrolysis was that the effective activation energy for ablation was one-half the intrinsic (isothermal) activation energy. It turns out that the surface temperature dependence on ablation rate was weak, so that the ablation rate could be calculated in the usual way, as shown in Fig. 7 in comparison with measurements of Ref. 28. The latter was improved upon by Scala[29] by including the effect of stagnation pressure, and the former aspect was improved upon by Kemp.[30] Scala also analyzed char-forming ablators.[31]

Other methods of fabricating the composite were investigated at G.E., Avco, and others. For example, Fig. 8 shows a cross-section of a tape-wound specimen which contained radial fibers but at an angle to the surface to alleviate delamination.

The Mercury heat shield material was finally selected to be glass-reinforced resin. (The first two flights used beryllium heat shields.) But the later Apollo heat shield requirements were much greater than for satellite re-entry because of the greater speed associated with return from the moon, and also the deorbit would take much longer. I felt that a material of lower-thermal conductivity was needed, for example, a low-density polymer. The Avco Everett Research Laboratory invented a method of structurally fastening such a material to the substructure without the use of refractory fibers, by using a metallic honeycomb to retain the mechanically weak char layer. Each opening in the honeycomb was filled with the

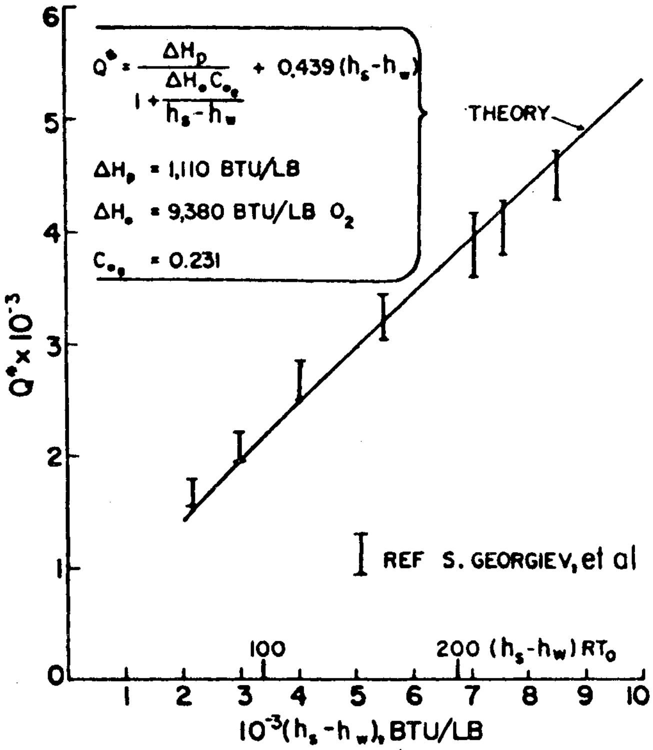

Fig. 7 Heat of ablation of Teflon[27] in comparison with measurements.[28]

ablation material. A key feature was the aspect ratio of the configuration; the depth of each insert was considerably greater than its width, so that the ablation material adhered to the honeycomb in shear which ensured that the ablation material did not fall out. This is in contrast to that of the Space Shuttle tiles in which their depth is much smaller than the width, so that adhesion to the substructure from the backsurface is in tension. The importance of the aspect ratio was pointed out to me by Arthur Kantrowitz.[32]

There were some other items of historical technical interest. After large digital computers were installed at G.E., they were put to use to calculate melting more ablation precisely. Attempts by Dan Tellep at Lockheed[33] to check my predictions showed a small, but persistent difference. My co-worker, Sheldon Blecher, had programmed the momentum equation of the molten glass with a card to permit removal of the convective term from the momentum equation to permit comparisons with the term included. In the run made

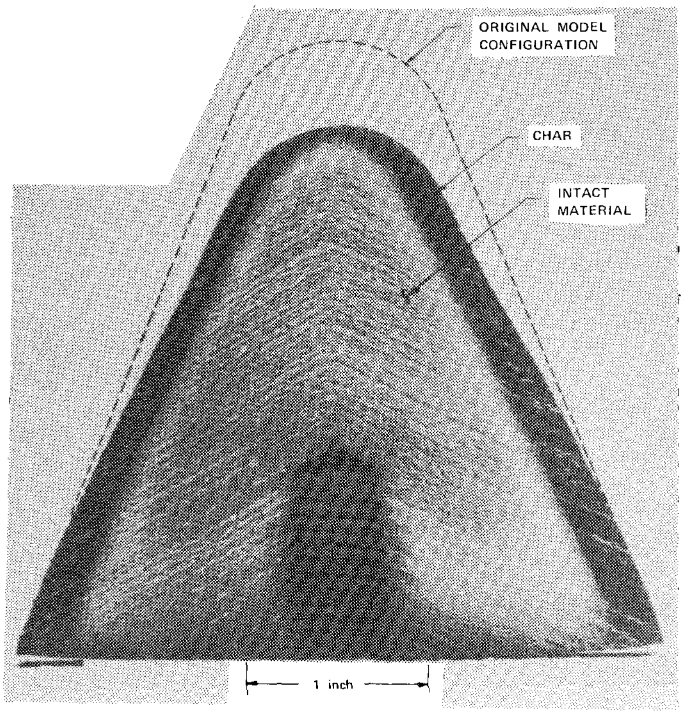

Fig. 8 Tape-wound silica fabric—phenolic model after exposure in a water-stabilized arc, 1958. Courtesy of the Chicago Midway Laboratories.

available to Tellep we had inadvertently omitted the convective term. This led me to be much more careful in the future and to read the listing and perform a hand check whenever feasible.

I also attempted to make contact with the rocket nozzle community. The switch from liquid propellants to solid propellants eliminated the nozzle regenerative coolant. My efforts were futile. The rocket people were geared to a schedule which did not permit methodical development at that time, hence they were searching for a quick solution.

On the other hand, Fred Whipple, the meteorite expert, was a consultant to G.E. at that time beause of the relation between meteor and man-made vehicle re-entry. This encouraged me to apply my theory to the prediction of meteor ablation. Some meteors are metallic, and molten metal has a low and almost constant viscosity, so vaporization was unlikely. The molten layer turned out to be so thin that the problem reduced to just an energy balance between the rate of meteor melting and the instantaneous heat transfer to the meteor for its remaining size. It is not a surprise that meteor dust with its deceleration at high altitude, and very large meteorites, with their reduced heat transfer and large mass, survive. I presented this at a meeting of the American Physical Society, but it did not attract much attention, so I never followed it up with a publication. Ernst Opik[34] gave a similar model at his lectures at the University of Maryland in the spring of 1957, see Fig. 9.

Conclusion

The general acceptance of the concept of a polymer charring resin held in place by an ablating refractory structure concluded my personal involvement with its development. The use of digital computers with finite difference techniques permitted the calculation of the ablation rate over complex shapes by the design group. Interest later shifted to planetary entry, in which radiative heating became important for glassy ablating heat shields.[35] (Although my involvement was over, embedded reflectors in the transparent ablating material appeared to be an attractive concept for this application.)

I was left with three major impressions or lessons. First, progress depends upon results from previously accumulated

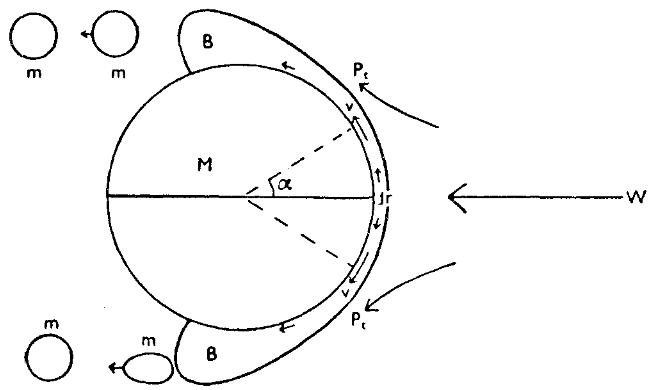

Fig. 9 Opik's figure of melting ablation of a meteorite.[34]

understanding. Second, progress, particularly for new areas, requires the synergism of disciplines; that is, the basic principles from several disciplines require combination to evolve a new solution. Third, my formal education had taught me primarily how to learn, think, understand, and innovate. It had successfully overcome the barrier which is initially created in the education process, that all useful knowledge is extant, and that it is presumptuous for anyone to try to increase it.

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