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The Guggenheim Aeronautics Laboratory at Caltech and the Creation of the Modern Rocket Motor (1936-1946): How the Dynamics of Rocket Theory Became Reality

by

Benjamin S. Zibit

A dissertation submitted to the Graduate Faculty in History in partial fulfillment of the requirements for the Degree of Doctor of Philosophy, the City University of New York.

1999

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This Thesis is Dedicated to the Memory
of Apollo Milton Olin Smith (1911-1997),
Pioneer in Rocketry and Aviation

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All photographs of the WAC Corporal have been provided through the courtesy of the Jet Propulsion Laboratory, California Institute of Technology, Pasadena, California; JPL Archives, Pasadena, also supplied many of the xerographic copies of original documents immediately relevant to the history, and the Archives' permission to cite from these documents is gratefully acknowledged. The California Institute of Technology granted permission to quote from Frank Malina's Ph.D. thesis, and provided a copy of Hsueh-shen Tsien's Ph.D. thesis. In addition, the Smithsonian Institution Press graciously gave permission to quote from *First Steps Toward Space,* as well as to reproduce diagrams from the text.

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Benjamin Zibit
New York City, 1998.

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Chapter 1

Introduction

The science of rocketry will always retain something of the new and strange at its core. As commonplace as satellite launchings have become in the late twentieth century, as much as the moon has lost its sense of mystery and inviolability, with footprints and tire tracks imbedded in its dusty surface, there will always remain in human consciousness the rootedness to things terrestrial that separates us from the realm of space. Although the names Tsiolkovsky, Oberth, and Goddard are familiar to those who count themselves among the *aficionados* of space science, until the 1920s and 1930s the visionary aspects of rocket development did not hitch themselves to endeavors that would transform dream into reality. [1]

The Guggenheim Aeronautics Laboratory at the California Institute of Technology (GALCIT), created in 1926, was home to one such attempt to turn the theory of rocket propulsion into reality. This dissertation focuses on the accomplishments, the working dynamics, and the conflicts of the small team of scientists and inventors at GALCIT who succeeded in creating the first American JATO (Jet Assisted Take-Off) rockets in the late 1930s and early 1940s. GALCIT engineers invented both solid and liquid propellants for the JATOs of the 1940s. In doing so, they opened the door to modern American ballistic missile development. Whether for good or ill, the development of

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[1] Konstantin Tsiolkovsky (1857-1935), early Russian theorist of liquid-fuel rocketry; Hermann Oberth (1894-1989), Rumanian-German scientist and author of *Die Rakete zu den Planeten Räumen*, 1923 (*The Rocket into Interplanetary Space*); Robert Hutchings Goddard (1882-1945), American physicist and creator of the first successful liquid-fuel rocket.

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both the Polaris and Minuteman ICBMs would have been impossible without the invention of the composite propellants first packed into JATOs by a band of rocket engineers and inventors at Caltech in 1941 and 1942.

There is nothing quite so fascinating as a good story. The tale of how the small intrepid band of rocketeers and engineers at Caltech defied the skepticism of many in the American scientific community and pushed forward to create working rockets is one with many reversals, risks, dramatic moments, and hard work. It is also a story of the dawn of large-scale government intervention in the process of pure research at US universities, an unfolding tale whose consequences for the shape of higher research and its ethical import were not then fully appreciated.

The Guggenheim Aeronautical Laboratory at Caltech, which began its work in earnest after the arrival of Theodore von Kármán at Caltech in 1930, opened a new door to rocket research in the spring of 1936. In a spring season full of tension and imminent conflict for the world, the quiet campus of Caltech saw the creation of a Rocket Research Group—four men strong at the outset—who would explore and design rocket motors, the first ever sanctioned by an major American institute of higher learning. By January 1937, the group, now grown to seven in number, had tested a tiny liquid propellant rocket motor and was on the way to fundamental discoveries in rocket propellants and the physics of rocketry—discoveries which would change forever both the general perception of the rocket's capabilities and its place on the world's political and military stage.

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By early 1939, the Guggenheim Aeronautics Lab Rocket Team (or Rocket Research Group) had secured government funding (paltry at first); but by 1941, military appropriations of more than $115,000 were flowing in. At the end of 1943, the group, now grown to more than 100 in number, founded the Jet Propulsion Laboratory, a Caltech-US Army (later NASA) hybrid which would later design moon probes and, by the late 1990s, a Martian lander. To culminate the preliminary phase of its work, JPL founding engineers and their team designed and flew America's first sounding rocket in the fall of 1945. This vehicle, the WAC Corporal, was the first man-made object to pierce the boundary of space, with a nearly vertical flight of 240,000 feet. The sounding rocket was a tool for scientific and meteorological exploration, built to measure near-space conditions at the rim of the exosphere, where high-altitude balloons could not go.

The aspect of rocket development at Caltech in the 1930s and 1940s which involves the deepest exploration, from the vantage point of this dissertation, is the relationship between theoretical definition of rocket motor construction and the practical fulfillment of this dream. Theodore von Kármán (1881-1963), the Hungarian-born engineer and mathematician, his graduate students at GALCIT, Frank Malina (1912-1981), Martin Summerfield (1916-1996), and the Chinese engineer-physicist, Hsue-shen Tsien (1911—), all members of the Rocket Research Team, conducted ground-breaking research which became the springboard for the launching, not only of sounding rockets, but of earth-orbiting spacecraft. There is almost no aspect of aeronautical theory and rocket flight dynamics these men did not explore.

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But as careful as were their calculations and theories of rocket motor function and design, they still had to contend with the specter of experimental failure. Their theories justified (to themselves, at least) further exploration, but with frequent mishaps the norm, theoretical work desperately sought confirmation in experimental success. Experimentation, in the hands of unorthodox and even renegade members of the Rocket Team, eventually carried the day. Without the maverick work of John Parsons (1914-1952) and Edward Forman (1912-1973), the theoretical explorations of Malina and Tsien would have remained just that: theory. Defining the relationship between the theory and the construction of workable rocket motors, of whatever size and thrust, involves balancing the weight of mathematical explication against the logic—peculiar and unique—of machine construction.

When creating a new technology, inventors unwittingly approach the threshold of an apparent chasm: the division between the craft of construction and theoretical science. A machine in its simplest terms is some constructed system designed to apply or transmit force. [2] It would seem, in its barest definition, that a rocket is among the most straightforward of machines: chemicals are burned within an internal chamber, and the products of combustion—mostly hot gases—stream out the only way available to them, through an aft exit. The reaction principle in its most direct and obvious application, that is what a rocket represents. Gases go one way; the rocket flies in the opposite direction.

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[2] This definition derives from the primary definition of the term, *machine*, found in the American Heritage Dictionary. See:*The American Heritage Dictionary, Second College Edition*. Boston, Houghton Mifflin Co., 1982, p. 751.

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Of course, the apparent simplicity of rocket performance is quite deceptive. Rockets are truly complex systems at work. Even the oldest of rocket motors, the Chinese flying arrow of the eleventh century, only appeared simple in construction and use. In fact, its usefulness depended upon how quickly and efficiently its combustible powders burned. The fire-arrow's creators may not have understood the chemistry of black powder and saltpeter very well, but they intuitively understood the reaction principle. They also knew the importance of balance in flight. In order for a rocket to fly, design and chemistry have to meld in a particularly effective way.

A modern rocket motor, whatever its historical link to its ancestor in Sung Dynasty China, is light years' distant from the primitive incendiary rockets (*huo chien*), with their gunpowder packed into hollow bamboo stems. [3] The modern rocket, despite its adherence to a physical law enunciated more than 300 years ago in Newton's *Principia* (the Third Law of Motion) works as a coordinated system. Its combustion dynamic is old, but its development as a machine dependent on sub-units and sophisticated knowledge of physics, chemistry, and advanced mathematics mark it as a very different innovation than its ancient Chinese predecessor.

The rocket as a systems technology fuses both basic science, applied science, and craft. Proper design of components, such as the combustion chamber and exhaust, or the propellant pump, demands thorough study of their theoretical dynamics. Even simple solid propellant rocket motors do not strictly resemble packed powder fireworks. The designer-craftsman who carefully

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[3] For a basic discussion of early Chinese incendiary rockets, see Joseph Needham's *The Grand Titration: Science and Society in East and West*. London, George Allen and Unwin, 1969, pp. 66-68.

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considers building a rocket motor realizes that he must understand the propellant's thermo-chemistry and packaging before he can assure himself that the rocket will work reliably. If the propellant does not adhere tightly to the rocket's interior wall—a frequent problem in powder rockets—just a few millimeters of open space can result in a burn-through. (Combustion becomes uneven, fills the gap between the chamber liner and the propellant itself, and breaches the container wall.)

However, rocket construction certainly demands craftsmanship as well as theory. At first glance, this might seem an implausible comment, as theoretical science impinges so directly upon rocket design, while craft appears only to play an ancillary role. Certainly a craftsman does not need to understand the physics of a waterwheel in order to build one. He only need know, in a visual and intuitive sense, how this mechanical contrivance works.

But in truth, craftsmen use more than handiwork—skill—in order to build a new machine. Craft involves imagination, specific tools, and arguably a special feel for the work at hand, some indefinable intuitive sense of proportion and of the consequences of each step in the process. In specific instances, for example, in wooden boat construction, careful transfer of design specifications from paper to hull requires knowledge of hydrodynamic shape, wood stress factors, and a keen eye for measure. Both craft and technology share a vision of the completed object.

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In the case of rocket motor construction, the barrier between craft and technology is frequently breached. Scientific discovery, in the sense of mathematical, chemical, or aerodynamic delineation of how a rocket functions, lies at the heart of this technology. Technology involves the birth of the machine. Craft does not take a back seat to theory, for without the translation of idea into design and effective building technique, there will be no rocket motor. Discovery and application have a synergistic effect upon each other, for ultimate vision may well re-shape the initial decisions made about construction; Still, it is the essence of the machine, its motive power, which dictates its frame and end-use.

In fact, defining what a rocket *is* provides a glimpse into the evolution of this technology. Is it a motor or an engine which imparts forward motion to the entire machine? Some rocket designers define the rocket combustion chamber and exhaust nozzle as an engine; others—particularly the Rocket Team at Caltech's Guggenheim Aeronautical Laboratory—generally referred to these key rocket parts as motors. A motor, by strict definition, is a device which "converts any form of energy into mechanical energy," while an engine is "a machine that converts energy into mechanical motion." [4] The key term is *mechanical.* Parts of a rocket motor certainly operate mechanically, particularly pumps and guidance devices such as gyroscopic stabilizers, fin-vanes which direct thrust, gimbels, etc.

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[4] For the purposes of this dissertation, the word *motor* will prevail over the term *engine* when describing rocket propulsion units. The GALCIT Rocket Team preferred the term *motor,* and for consistency's sake, their choice will be the one used here. See: *American Heritage Dictionary, Second College Edition* (1982), pp. 337, 817.

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However, the rocket motor itself does not move, for motion translates directly from it to the entire machine, rather than to some key internal part that carries out mechanical movement. Heat energy converts directly into kinetic energy. Forward motion involves the rocket moving from one point to another, rather than remaining stationary. Still, the question of just how to define the combustion unit remains undecided. Since the rocket is a reaction propulsion device, it may stand by itself in a new class of motors. This puzzle suggests the dependency of the term *rocket* upon social conception, and upon a fundamental linguistic ambivalence over which word best fits the machine's function.

A key issue, then, for the builder of rocket motors is whether to forge a coherent theory of motor functioning and design before actual construction begins, whether to test prior to theorizing, or whether to create a reciprocating dynamic: testing supported by theory, modified after experimental revelation of flaws in the theory, and so on, until the correct design package evolves. Walter Vincenti, in his provocative work, *What Engineers Know and How They Know It*, holds to a model of analysis that strikingly resembles the process of biological evolution.

Vincenti asserts that the accumulation of knowledge is a growth process for engineers, involving two key stages: variation of a current hypothesis through experimentation virtually 'in the dark' ("blind variation" is the term he gives it), followed by a "selective retention" of ideas that work. [5] Blind

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[5] Walter G. Vincenti. *What Engineers Know and How They Know It: Analytical Studies from Aeronautical History.* Baltimore, Johns Hopkins University Press, 1990, p. 241. Vincenti

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variation is not strictly random, but since, according to Vincenti, engineers do not have enough data to construct a successful model initially, they must rely on a trial-and-error process to arrive at a satisfactory result. This definition has developed in the shadow of Natural Selection, for implicit in the model is the notion of progressive modification, eventually leading to a working unit. There is evidently no implication of attained perfection, for no machine is perfect.

Vincenti's model assumes the pre-eminence of theory in engineering. Some pre-formed idea sees the light of day, either as a shaped image, or as a mathematical construct. Although he does not admit it, Vincenti cannot foresee the growth of engineering without the existence of some theory. But machine builders do not always have a complete idea of what they are building; they frequently shape as they build and cannot visualize the final image, or in fact, the precise details of the process they are engaged in. Projects evolve, sometimes very unpredictably.

An essential statement of this thesis is that in the construction of a new kind of technology—specifically, a rocket motor—theory does not precede, but actually follows the lead of experimentation. The two intrude into each other's realm until the process at times seems to meld into one, with the end result the construction of a new machine. Applied science is the driving force and ultimate guide, and most strikingly, actual rocket motor performance even opens unforeseen theoretical vistas to the scientist.

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derived his 'blind variation and selective retention' model from psychologist Donald Campbell, who asserted that knowledge increases, not randomly, but without adequate guidance.

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Rocket technology, then, has led and will continue to lead to exploration, not merely in the field of astronomy, but in areas of mathematics and physics only partially developed to this point. The creation of the rocket has not only altered the political equation on earth; the study of rocket dynamics has extended to a new field in physics and mathematics— the physics of chaos, a discipline that may hold insights into still deeper and more basic mathematical understanding of physical processes. Rocketry is continually on the outer frontier of the known, 'pushing the envelope,' in the jargon of the test pilot. It will remain so until some fundamentally new form of propulsion, capable of acting in a vacuum as well as in an atmosphere, comes to light.

Non-linear physics—the physics of chaos—provides an alternative window into rocket motor combustion processes. The combustion of rocket fuel is a process that *ideally* should unfold at a constant rate; differential calculus certainly describes the burn rate of specific fuels to within reasonable limits of accuracy. But even a solid propellant motor with few moving parts functions at a level of chemical and physical complexity difficult to measure accurately. There just are limits to theoretical accuracy and predictability. The ideal system dissolves into layers of complexity, in which the real behavior of atoms is very difficult to predict. Some of this behavior can be categorized as quantum mechanical. But on an even deeper level, quantum behavior unfolds into a physics of turbulence. This cloud of physical phenomena is now only partially understood. It may reveal a new science unlike any presently imagined.

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Theodore von Kármán and his young graduate students at the Guggenheim Aeronautical Laboratory at Caltech (GALCIT) assumed *constant* burn rates for the solid propellant rocket motors they studied hypothetically in 1939. [6] Underlying this assumption was the model of the propellant as composed of perfectly elastic grains that would collide in linear paths after ignition. The determination of the behavior of the solid propellant was a function of the gross behavior of the entire propellant mixture. However, the actual initial state of the system was unknown at the instant of combustion. If the grains of propellant were not perfectly elastic spheres, some propellant would burn imperfectly, and pressure conditions within the combustion chamber would vary from instant to instant.

As Henri Poincaré (1854-1912), French mathematical physicist and philosopher, wrote in *Science and Hypothesis* (1905), "the state of a system at a given moment depends on two things—its initial state, and the law according to which that state varies....In the kinetic theory of gases we assume that the gas molecules follow rectilinear paths and obey laws of impact and elastic bodies; yet as we know nothing of their initial velocities, we know nothing of their present velocities." [7] Neither von Kármán nor his protégé Frank Malina expected that the rocket motors they were designing might produce dynamic instability during combustion. They uncovered the unsettling evidence of instability within the combustion process during the

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[6] Von Kármán, Theodore, and Frank J. Malina. 'Characteristics of the Ideal Solid Propellant Rocket Motor.' Jet Propulsion Laboratory, California Institute of Technology Report # 1-4, 1940. Published in *The Collected Works of Theodore von Kármán, Vol. IV*, London, Butterworths Scientific Publications, 1956, pp. 94-106.

[7] Poincaré, Henri. *Science and Hypothesis*. New York, Dover Publications reprint, 1952, p. 190.

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course of their experimental work. They were not fully able to explain the source of combustion turbulence. Nevertheless, von Kármán formulated equations that described how an ideal rocket *should* function.

Chaos theory is a recent development in physics and mathematics, although the work of Henri Poincaré at the turn of the twentieth century presaged it. Poincaré was the progenitor of the study of perturbations and of the complexity that lies at the heart of much seemingly straightforward motion. Not only in his *Science et Hypothèse* (1905) and *Science et Méthode* (1908), but also in *Les Méthodes Nouvelles de la Mécanique Celeste* (1892-1899), Poincaré indicated that much of the behavior of complex systems could be understood only by introducing several 'degrees of freedom' to the equations describing them. To understand the probability, for example, that the gravitational interactions of three bodies will result in instability at some future time, Poincaré introduced no fewer than six degrees of freedom to the differential calculus describing their motion. [8] The concept of six separate coordinates describing the motion of an object eventually required new terminology to describe its state: this was known as phase space.

Theodore von Kármán, GALCIT's director, had much mathematical and practical experience with the phenomenon of turbulence and in fluid dynamics, although he seems not to have used a particularly dynamic approach to the understanding of rocket combustion as a form of turbulent behavior. His calculus included the development of several differentiable variables and the calculus of variations (which treats of complex curvilinear

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[8] Poincaré, Henri. *New Methods of Celestial Mechanics, Part I*, in *The History of Modern Physics and Astronomy Series, Vol. 13.* Editor, Daniel L. Goroff. The American Institute of Physics, 1993. See: Chapter 1, 'Generalities and the Jacobi Method.'

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forms), but he did not explore the realm of phase space. Even so, Poincaré's work did not elude von Kármán's, and possibly Malina's eye. [9] Turbulence and complex forms of physical behavior are inherent problems in all fluid and even chemical systems. In the late 1930s, from the vantage point of engineers, they were problems to be tackled from the perspective of design changes, and not by creating *new* mathematical schemata to describe them.

Poincaré's treatment of turbulence, a radical departure from standard differential calculus (including the calculus of variations) might have solved problems plaguing the GALCIT rocket experimenters, if they had decided to follow Poincaré's lead. But few mathematicians picked up on this thread of the French polymath's work until the 1960s. The GALCIT engineers did not see beyond the limits of the mathematical tools available to them. Regular, in fact, nearly constant experiment only permitted them a glimpse of the hidden structure of the puzzle they faced: why so many of the rocket motors they built failed to perform as expected.

Von Kármán knew that rocket test explosions, fuel-line ruptures and fires were not only attributable to mechanical dysfunction. He believed there was a lack of precision in mating mathematics to construction. When von Kármán, in the summer of 1939, presented Frank Malina with a series of four equations designed to describe the "interior ballistics of the rocket motor," he

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[9] In his autobiography, *The Wind and Beyond* (1967), von Kármán wrote of the "profound impression" Poincaré's *Science and Hypothesis* had on his thinking, "especially in my approach to scientific problems." See: p. 24. The effect on Malina was more indirect. As he and von Kármán worked together on the experimental investigation of turbulence, Malina came under the influence of the elder scientist's mathematical study of turbulent phenomena (such as dust storms). More significantly, it was von Kármán's belief that scientific theory is more a matter of convention than of fact that Malina inherited from Poincaré.

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surmised that these equations would describe the solid motor's propulsion dynamics closely enough to narrow the gap between actual function and theory. However, von Kármán also knew that these equations would prove very difficult to solve. The equations were so complex that they did not succumb to integration, but had to be graphically resolved. [10]

The GALCIT Rocket Team made no systematic theoretical attempt to understand how the physics of turbulence might actually affect a rocket motor. The mathematics of Poincaré remained an esoteric unrelated field, even for von Kármán. Differential calculus, including the calculus of variations, became the key mathematical tool of Malina, Tsien, and Summerfield. [11] In fact the project's history reveals that eminently practical solutions to the annoying problem of turbulent combustion became the rule.

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[10] Theodore von Kármán, with Frank Malina. 'Characteristics of the Ideal Solid Propellant Rocket Motor.' JPL Report No. 1-4 (1940). Von Kármán, *Collected Works, Vol.IV* (1956), pp.97 and 100.

[11] The calculus of variations, developed in order to more precisely define the area under curved surfaces, became a separate branch of the calculus in the seventeenth century. Beginning with Newton's investigations of the required shape of a projectile offering the least resistance in the air, this branch of the calculus later evolved with the work of the Bernoulli brothers (James and John) in their study of a class of curves called cycloids, having certain maxima and minima of area or time (1696-1701.) In defining action along such a curve, one can measure the least amount of time it takes a body to fall between two points along this curve. The principle, known as the Principle of Least Action, was explored rigorously by Lagrange (circa 1750-1755.) What Lagrange sought to prove was that "nature always acts to minimize certain magnitudes." Determining the area of curvilinear surfaces using the calculus of variations would be of obvious value to aerodynamic engineers. By maximizing or minimizing the curves on aerodynamic surfaces, such as airplane wings, a designer may streamline them for maximum efficiency and the least drag. The calculus of variations is an essential tool in designing the area of curvature in a rocket's combustion chamber, particularly at the nozzle throat. See: James R. Newman. *The World of Mathematics, Vol.II* . New York, Simon and Schuster, 1956, pp. 886-891. See also: Morris Kline. *Mathematical Thought from Ancient to Modern Times, Vol.II.* New York, Oxford University Press, 1972, pp. 573-591.

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However, solutions were only partial, for the engineers found their mathematical solutions, as complex as they were, inadequate to the task. If their exploration of non-periodic or unpredictable exhaust gas flow on motor performance had been more thorough, new mathematical vistas would have unfolded for them. Sometimes, even the most creative, open-minded thinkers find themselves hamstrung by the very structure of their intellectual system and training. Von Kármán, Malina, and Tsien made inroads in the study of turbulence by using the tools they had at hand. More succinctly, chemical and design solutions to the problem won out.

The work of the Guggenheim Aeronautical Laboratory Rocket Research Team at Caltech, designing and building rocket motors in the 1930s and 1940s, was an effort at problem-solving. The rocket motors the Team studied and invented were created in a defined academic environment, the campus of the California Institute of Technology. The scientists and inventors in the GALCIT Rocket Research Team defined the parameters of their work in isolation from established scientific investigation on the American university scene of the late 1930s. They worked, largely unappreciated by the very institute that sheltered them.

Caltech actually banished them from the campus when their experiments ran amuck and exploded. Other GALCIT professors and graduate students saw them as reckless and even self-destructive. The appellation, the *Suicide Squad* floated around the campus and stuck. [12] Rocketry became the

[12] Some GALCIT students evidently began to use this nickname for the rocket group because they fully expected one of the group's mishaps would result in the demise of the young rocketeers, if not (what was more likely) a gaping hole in the side of the GALCIT

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dangerous, irresponsible pastime of a small coterie of engineering students who could not follow the well-trodden path. Why, after all, did they not study aeronautics and line up jobs with the burgeoning aircraft industry of southern California?

Ridicule and rejection, of course, are powerful shapers of people and their work; to be dismissed by a community implies—in part at least—self-definition by community standards. Tsien and Malina, two highly abstract thinkers, worked in an intellectual *milieu* defined primarily by its focus on aeronautics—a current and practical technology. Rocket development was not a preoccupation of mainstream aeronautical engineering thought in the 1930s or even early 1940s. Only in the Soviet Union, where a small but determined group of engineers, among them Sergey Korolev (1907-1966), labored through the 1920s and 1930s to develop rocket motors—including Jet Assisted Take-off Rockets (JATOs)—and in Nazi Germany, where the Peenemünde program (post 1936) was a closely guarded secret—were there state-supported programs to explore rocket technology. [13]

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building. Professor Hans Liepmann, who worked under von Kármán, recalled how the platform at one end of the GALCIT building was the frequent scene of explosions. (This was a concrete platform at the southern end of the lab.) See: Iris Chang. *Thread of the Silkworm*, New York, Basic Books, 1995, p. 74.

[13] In the 1930s, the Gas Dynamics Laboratory in Leningrad developed small liquid and solid propellant rocket motors, some of them using nitrogen tetra oxide and toluene as fuels. (GALCIT's John Parsons had rejected nitrogen tetra oxide as a fuel in the early 1940s because of its volatility.) The liquid fuel motors were efficient and compact, could be restarted, and were intended to be placed inside airplane fuselages. However, most of the Russian liquid fuel motors were not auxiliary rockets, but were installed inside the tail of an airplane fuselage and meant to be the sole power plant. Small liquid-fuel JATOs designed at the GDL in the early 1930s did find their way on to aircraft wings in 1931 and testing continued throughout the next two decades. Sergey Korolev, who later led the Soviet effort to launch *Sputnik* in 1957, was amongst the few but brilliant young engineers at Moscow's Group for Study of Jet Propulsion (GIRD) working to develop winged rocket planes in the 1930s. See: Y. S. Schetinkov, 'Development of Winged Rockets in the USSR, 1930-39.' In Durant and James (1985), pp. 247-257, and I. I. Kulagin's 'Developments in Rocket Engineering Achieved by the Gas Dynamics Laboratory in Leningrad, ' in the same volume, pp. 91-102.

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The Russian program at the Gas Dynamics Laboratory in Leningrad and at Moscow's Group for the Study of Jet Propulsion (GIRD) made significant technical discoveries in both solid and liquid propellant rocket units. The Russians were mounting JATOs on aircraft wings before anyone else and developing liquid propellant motors which were self-cooling in the mid-1930s. The GDL group, including aerodynamicist Nikolay Tikhomirov (1860-1930), developed a slow-burning solid powder web and several liquid propellant motor variants with truly revolutionary components (such as centrifugal nozzles which sprayed propellant in a circular jet around the combustion chamber.) [14] There seems no question that these early investigations of rocket technology were destined to bear fruit in the first launching of an earth satellite in 1957 and in numerous Russian breakthroughs in space science.

Quixotically, however, nearly all the results of Russian rocket research remained within the Soviet Union, virtually unknown to the engineering community in the West. The secrecy of the Russian program, like that of the Nazis, emphasized the hard reality—rocket builders around the globe were often working on their own, with little awareness of each other's accomplishments. This could not help but retard development of the field.

Scientists like Theodore von Kármán, Hsueh-shen Tsien and Frank Malina, for all their desire to maintain an open exchange of information with other engineers, found themselves effectively in a creative vacuum. Political

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[14] I. I. Kulagin, 'Developments in Rocket Engineering....' In Durant and James (1985), pp. 91-102.

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decisions overseas, skepticism, even ridicule in the United States over the value and future of rocket work all meant that the GALCIT rocket investigators were left to pursue their ideas and tests on their own for the most part. Although their effective isolation did not stop Caltech's men from focusing in on the engineering tasks at hand, it did handicap the overall progress of rocketry.

The intellectual engine that drove von Kármán to work into the early hours of the morning until he constructed the four equations he gave to Malina—who solved them within a day—was his quest for a mathematical solution to an annoying and significant puzzle. At the time, von Kármán could not have been thinking that GALCIT would later become a leader in a nascent rocket industry. Peace and quiet, an end to incessantly exploding rocket motors would have been enough for him. Von Kármán became—predictably for him—absorbed by a challenging mathematical problem confronting him.

Despite their partial isolation, the GALCIT Rocket team still operated very much within a specific historical framework. Since the definition of science depends upon the level of insight, the complexity of the tools at hand, and the pyramid of past knowledge built underneath the scientist, scientific effort demands explanation of both the effects and origins of discovery. The only way to accomplish this is to study the environment which nurtures and defines the ends of science.

Although there can never be any complete consensus on the historical role of science, a fundamental mystery seems perenially to surface: how did science become so powerful a force in human life? Scientific discovery destabilizes

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society and forces men to redefine their cherished political and social relations. Yet science and technology have long been intimately linked; in fact, it is impossible to decouple them—one consequence of successfully mating the mathematics and chemistry of rocket motor function to experiment is the intercontinental missile.

In 1965, Oscar Handlin, in a short and provocative essay, 'Science and Technology in Popular Culture,' commented that "a deep underlying distrust of science runs through the accepted attitudes of people in most advanced nations." [15] This is a deeply-rooted belief, born of the uncertainty cast upon the world after the detonation of the atomic bomb. But Handlin astutely recognized that cultures do not regard technology with the same air of quiet malevolence that they reserve for pure science. It is *technology* that turns discovery to good or ill. There is nothing intrinsically evil about science itself. The need to construct historical explanations for the development of scientific concepts arises from the realization of the consequences of these ideas, unveiled before an unsuspecting world.

Certainly, there is still very much an open question as to how deeply humans may probe the inner secrets of creation. The applications and reliability of new technologies have their inherent limitations, and so does man's ability to handle the fruits of discovery. "This accursed Science," said H. G. Wells (quoted in Handlin's essay), "is the very Devil. You tamper with it—and it offers you gifts. And directly you take them it knocks you to pieces in some unexpected way." [16]

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[15] Oscar Handlin. 'Science and Technology in Popular Culture,' in *Daedalus*, The Journal of the American Academy of Arts and Sciences, Winter, 1965, p. 156.

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Quite true, for the story told in this essay, a tale not only of dramatic discovery and invention, but of the birth of what President Dwight Eisenhower called in 1961 the 'military-industrial complex,' bears witness to the truth that science is not free of social and political influence. [17]

Still, the essence of science is the dynamic and the language of discovery, and this process has its own internal logic, whatever the shape of its house. In fact, the correspondence between mathematics as a language and physical process lies at the heart of science. There is often a felicitous conjunction between the structure of mathematics—more a matter of convention and mental effort than anything else—and the existence and operation of natural phenomena in the universe.

As mathematics is another type of human language, there is an almost endless number of possible symbolic combinations which may conform to physical law, but only a tiny number of these have so far been discovered. In other words, deeper levels of understanding still await the explorer. Mathematics as a language is constantly re-inventing itself. A key task of this thesis is to try and elucidate how mathematics as a linguistic tool helped shape rocket theory and how closely the resultant theory fit observed physical process. Rockets may operate on the venerable principle of combustion, but as machines they are wholly artificial contrivances. Like all machines, they

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16 Handlin (1965), p. 164. Handlin does not identify the source of the quote, but Wells' ambivalent attitude towards science is evident in it.

17 Dwight D. Eisenhower. 'Farewell Radio and Television Address to the American People, January 17, 1961.' *Public Papers of the President of the United States, Dwight D. Eisenhower, 1960-61*. Washington DC, US Government Printing Office, 1961, pp. 1035-1040.

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force nature to work in some pre-determined manner. The question is: how effectively does mathematics fuse with actual performance to describe how a rocket works?

Theodore von Kármán was a scientist trained in the tradition of the University of Göttingen, where he studied under Felix Klein (1849-1925) and Ludwig Prandtl (1875-1953). [18] The mathematical expression of ideas came naturally to him. Von Kármán probably never questioned the power of mathematics as a discovery tool, nor looked too deeply into its logical and semantic structure. He used mathematics to define his own scientific interest: the nature of fluid dynamics. Von Kármán was not oblivious to the social and moral dimensions of science; but they did not comprise its essence for him. As an engineer, he devoted his life's work to harnessing scientific insight to the creation of structures, tools, and machines that served social purposes. He believed in the fusion of theory to design and construction of the machine. His training and his own proclivities told him to put theory first.

The GALCIT director encouraged a team possessed of both mathematical insight and technical ability to coalesce around him at Caltech. Von Kármán fully intended for theoretical exploration to lead the way toward successful experimentation of rocket motors. Martin Summerfield, Malina, and Tsien

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18 Von Kármán, Theodore, with Lee Edson. *The Wind and Beyond: Theodore von Kármán, Pioneer in Aviation and Pathfinder in Space.* Boston, Little Brown, 1967, pp. 34-35. Prandtl, von Kármán's teacher at the University of Göttingen, pioneered work in the theory of aerodynamic turbulence, particularly his invention of the concept of the 'boundary layer' in aerodynamic flow. a level of aerodynamic flow near smooth surfaces beyond which turbulence manifests itself. See: Ludwig Prandtl and O. J. Tietjens, *Fundamentals of Hydro and Aeromechanics.* New York, Dover Publications (1957).

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fit the bill of the mathematical scientist envisioned by von Kármán. Each of these men could develop the mathematics of rocket propulsion, or explicate the physics of this new field.

However, a rocket motor is a product of invention and imagination as much as theory. The latter germinated in the mind of a curious and eccentric explosives expert, John Parsons (1914-1952), the inventor of the composite solid propellant that first made the storable JATO possible. An individual without a formal college education, Parsons had a broad, if not deep knowledge of chemistry and was a devoté of the occult—he was also a man who kept jars of highly volatile trinitromethane in his kitchen closets, so that he could experiment with liquid propellants. Unorthodox as he was, Parsons was a natural and prolific inventor. [19]

Malina described Parsons as a man who "had an uninhibited and fruitful imagination." [20] He felt that Parson's impatience with a slow, methodical scientific approach to the problems of constructing a rocket motor was "symptomatic of the anxiety of pioneers of new technological developments," and added: "In order to obtain support for their dreams they are under

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[19] Oral Interview with Frank Malina, conducted by James H. Wilson, June 8, 1973, re: 'Early JPL/Caltech History,' p. 8. Original in the Archives of the Jet Propulsion Labs, Pasadena, California. Oral Interviews in the Archives are catalogued alphabetically by the name of the interviewee, the interviewer, and the date. There is no specific accession number for such transcripts.

[20] Malina, Frank W. 'On the GALCIT Rocket Research Project, 1936-1938.' This manuscript was completed by Malina in 1967 and first published in 1974 by the National Air and Space Museum's *Smithsonian Annals of Flight, No. 10.* Reprinted in *First Steps Toward Space.* Frederick C. Durant III and George S. James, eds. American Astronautical Society History Series, Vol. 6. San Diego, Univelt, Inc. (1985), p. 114. Malina went on to comment on how Parsons "loved poetry and the exotic aspects of life."

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pressure to demonstrate them before they can be technically accomplished." [21] But Malina was himself a pioneer and no less anxious to see his work succeed. [22] From his vantage point, the von Kármán approach, stressing theoretical understanding before experimentation, was preferable. Was it any more reliable and inspired than Parson's imaginative, practically-oriented method? That is an open question, to be addressed within the pages of this dissertation. While Malina, Tsien, and Summerfield stretched the theoretical limits of rocket motor dynamics, experimental verification of their calculations was, for some time, only modestly successful.

Between 1936 and 1941, there were many more explosions than there were successful rocket motor firings. With each mishap, new and daunting problems would unfold before their eyes. At such crucial times, when their work seemed on the edge of failure, imaginative insights brought new avenues of exploration. Moments of creative spark often come when a thinker is deeply immersed in the structured process of thought...in the midst of a complicated mathematical solution, for example. Imagination can bridge the logical gap that confronts the problem solver at such moments.

The sense of the term *imagination* involves an unconscious, yet deeply rational system of thought. Intuition and imaginative creation do not strictly coincide. Intuition may have an irrational basis. Imaginative thought draws upon experiential roots and is an associative process. The imagination may have its irrational or mystical components, but is still eminently logical. The

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21 Malina (1967), p.114.

22 Malina (1967), p.120.

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imaginative faculty systematically assesses prior knowledge and combines it in new ways to solve present dilemmas.

The GALCIT rocketeers grasped a dream and followed its excruciatingly winding path. They devised a plan to realize the dream to a very limited degree through an established method of study (relying on mathematics and rigorous scientific training). Partial failure of this method to achieve its expected goals led them to rely on creative imagination to spark the process anew. Reformulation of their theoretical plan followed, and experiment intensified until they succeeded.

To explore the history of the GALCIT Rocket Research Project is to enter into a labyrinth. At the center of the maze lay a door to a new technology: one that tested the limits of established science in the 1930s and 1940s. The labyrinth, complex as any creation of human experience, possessed pathways leading to political controversy, revelations of personal animosities, the metamorphosis of American academia and the US military into a unique hybrid, and glimmers of a new realm of physics. How far the Rocket Team opened that door will become clear in the pages to follow.

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Chapter 2

The Science of Rocketry in the 1930s: Progress and Poverties

In 1930, American rocketry was no longer quite in its infancy. Robert Hutchings Goddard (1882-1945) had been at work, solitary work for the most part, since the beginning of the century's second decade, designing both solid and liquid propellant rockets. Goddard had achieved limited success in building rockets that would fly, but most of his early designs peaked out at extremely shallow altitudes. Prior to 1916, Goddard had experimented with smokeless powder rockets, (probably with a guncotton or nitroglycerin base), with an eye to determining thrust potential. His rocket motors generated exhaust velocities of approximately 8000 feet per second (5450 mph). In 1926, after several years of struggling to work with liquid oxygen as an oxidizer, Goddard succeeded in firing the first liquid propellant rocket a distance of 184 feet at 60 mph. [23]

In a sign of things to come, Goddard early on established a link between his research and government funding. By September 1916, he was writing to Charles Greely Abbot (1872-1973), the Assistant Secretary of the Smithsonian Institution for financial support. "For a number of years," Goddard wrote, "I have been at work upon a method of raising recording apparatus to altitudes exceeding the limit for sounding balloons...These experiments are now

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23 Durant, Frederick C. III. 'Robert H. Goddard and the Smithsonian Institution,' in *First Steps Toward Space: Proceedings of the First and Second History Symposia of the International Academy of Astronautics at Belgrade, Yugoslavia, 26 September, 1967 and New York, U.S.A., 16 October, 1968.* AAS History Series, Volume 6. San Diego, Publ. by Univelt for the American Astronautical Society, 1985, p. 61. Durant's monograph first appeared in 1968.

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completed, and I feel that I have settled every point upon which there could be reasonable doubt. Incidentally, I have reached the limit of the work I can do single-handed; both because of expense, and also because further work will require more than one man's time." [24]

In 1917, Goddard requested and received $5000 from the Smithsonian to continue his research at Clark University in Worcester, Massachusetts. The somewhat secretive Goddard, who disliked sharing details of his experiments with others, nevertheless received funding from a variety of sources, including the Army Signal Corps (1918). With American entry into World War I in April 1917, Goddard perspicaciously promoted his rocket research as in the national interest. He frequently wrote to Abbot and the Smithsonian's chief, Secretary Charles D. Walcott (1850-1927), of his faith in the military value of the rocket. [25] Abbot and Walcott became Goddard's steadfast supporters.

When Goddard first sought Smithsonian support, he was actually asking for *quasi*-governmental funds. The $5000 initially granted may have come from the Smithson Trust Fund or from Congressional moneys. The Smithsonian's system of sponsoring research was far more *ad hoc* in the early 1900s than it was later to become. Government-regulated grant funding had

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[24] Durant (1968) pp. 57, 59. Goddard received $10,000 from the Signal Corps in 1918. The exhaust velocities of solid -propellants are characteristically higher than those of liquid propellants, if the solid fuel burns in a quick, unrestricted manner. Typically, such solid fuels burn completely in a few seconds, while the thrust of a liquid propellant rocket builds more slowly, as combustion chamber pressure must increase gradually until the rocket motor attains enough thrust to lift the rocket off the ground.

[25] Esther C. Goddard and G. Edward Pendray (eds.) *The Papers of Robert H. Goddard, Vol.I.* New York, McGraw Hill and Co. (1970) See: Letters, Goddard to Walcott, April 11, 1917, and August 20,1917, pp. 194, 199.

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become commonplace during World War I, but the Smithsonian was in the peculiar position of being a privately endowed institution with a Congressional connection. The Smithsonian's discretion in disbursing its endowment depended upon the judgment of its administrators, as much as upon the advice of government experts. The money went to Goddard primarily for two reasons: Abbot, the first to read Goddard's monograph, was an astrophysicist himself; the concept of high altitude rocketry intrigued him. Beyond this, the Bureau of Standards had performed an independent assessment of Goddard's concepts (at the Smithsonian's request), and had endorsed the notion of helping him. [26]

Frederick Durant III (1916-), editor of *First Steps Toward Space,* and one-time president of the International Astronautical Federation, wrote that Goddard's relationship with Smithsonian Assistant Secretary Charles Greely Abbot continued for several years and ended only with Goddard's death. [27] This in itself militates against the common description of Goddard as working in complete isolation from others. The picture is not so simple, for Goddard even asked Frank Malina, a key player on the GALCIT Rocket Research Team, to join his team once Malina had graduated from Caltech in 1940. [28]

By the late 1920s, a new alliance had begun to build. Private philanthropy became a foster-parent to aviation. The aeronautics industry in the United States was young. Yet, Robert Goddard's passion for rocketry was far more

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26 Durant (1968), pp. 57-58.

27 Durant (1968), p. 63.

28 Hunley, J. D. 'The Legacies of Robert H. Goddard and Hermann Oberth.' *Journal of the British Interplanetary Society* Vol. 49, 1996, p.47. This monograph, in its original unpublished version, mentions the Goddard offer to Malina on p. 29.

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unorthodox than the airplane itself. Even so, by 1930, Daniel Guggenheim, (1856-1930), with a fortune built on investment in copper mining, donated $25,000 to Goddard's laboratory in Worcester. [29] Guggenheim's new trust fund would establish the Guggenheim Aeronautical Laboratory at Caltech that very same year. Guggenheim was interested in any research frontier related to flying. His son, Harry (1890-1971), had been an aviator during the First World War; Harry emerged from the conflict an enthusiastic advocate of aeronautics.

The story of the Guggenheim-Goddard connection typifies the way in which air research developed in the early years. Personal ties to donors mattered. In this case, Charles Lindbergh was the catalyst for action. Not more than two years after his successful transoceanic flight (May 21, 1927), Lindbergh energetically exploited his new fame and influence to bolster the fledgling American aeronautics industry. He had become a crusader for the new technology. In 1929, Lindbergh was a trustee of the Guggenheim Aeronautics Fund. He learned of Robert Goddard's experiments rather fortuitously.

Earlier in the fall of that year, Harry Guggenheim's wife, Carol, had read a newspaper story on Goddard's 'Moon Rocket.' Lindbergh and her husband happened to be sitting by her in the living room of the Guggenheim home on Long Island, discussing aeronautics, when she interrupted them with the news of Goddard's work. [30] Mrs. Guggenheim's timely remarks led to a

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29 *Goddard Papers, Vol. II* (1970). Letter, Daniel Guggenheim to Wallace W. Atwood, June 12, 1930. pp. 744-745. On June 23, 1930, Guggenheim forwarded a check in this amount to Goddard (see p. 747.)

30 Walter S. Ross. *The Last Hero: Charles A. Lindbergh*. New York, Harper and Row (1964), pp. 243-245. The newspaper article was clearly lampooning Goddard's research by labeling his rocket a 'moon rocket.' Surprising, Lindbergh became interested in rocketry for reasons

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Lindbergh telephone call to Goddard on November 22, 1929. After a meeting with Goddard the next day, the aviator came away impressed by rocketry's potential. He urged Daniel Guggenheim to underwrite Goddard's work.

On June 12, 1930, Guggenheim formally offered Goddard $25,000 a year for a two-year period, with the door left open to another $50,000, should the Massachusetts physicist's results merit further support. [31] Such largesse proved invaluable to Goddard, who used the funds to relocate his research facility from Worcester, Massachusetts to Roswell, New Mexico, far from the prying eyes of the press. Goddard had also run afoul of his own community in Worcester: the local fire department had banned his tests, leery of brush fires and explosions. Daniel Guggenheim's check was timely. Goddard had decided he had to move to a more secluded test site and the Guggenheim Aeronautical Fund provided the chance to do so. Goddard's luck had held true. But Guggenheim was an unusually farsighted individual. Many other donors were not nearly as generous as he.

In December 1929 Goddard tried to solicit funds from the Carnegie Institution in Washington. In a December 14, 1929 letter to John C. Merriam, President of the Carnegie Institution, he submitted a $50,000 two-year budget request, including $10,000 for 'shop expenses' and $10,000, as an 'amount to meet

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similar to those Frank Malina of GALCIT later professed. He realized the limitations of propeller-driven aircraft and began thinking about jet and rocket propulsion, as early as the 1920s. Lindbergh approached the DuPont Company in the summer of 1929 for information on the feasibility of black powder take-off assist rockets. Dupont chemists told him that such rockets were impractical, as their weight-to-thrust ratio would be too high.

31 *Goddard Papers, Vol. II* (1970), pp. 744-745.

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salary of Dr. Goddard.'[32] Five days later, Merriam offered $5000. Goddard wrote back on December 26,

> The appropriation of $5000 by the Carnegie Institution for the rocket work came as a great surprise, and I wish to thank you sincerely for it. As I understand it, this is made in a way similar to the last appropriation of $5000 by the Smithsonian Institution. It is not quite clear to me, however, whether this grant is part of the larger support which we discussed when I was in Washington, or is distinct from that. [33]

Goddard rarely if ever shared his data in great detail with other rocket scientists, although with Merriam he was forthcoming enough to report favorable news of increases in the exhaust velocity of his liquid-propellant engines. [34] Still, rocketry was very risky business in the 1920s, and Merriam was not ready to overextend himself. Goddard searched for support among established donor institutions, with an eye to his reputation. His meeting with Lindbergh was an unsolicited bit of good fortune. He invariably attracted enough interest to encourage continued funding of his work. Despite his reticence, Goddard clearly impressed donors and aeronautical experts with his command over the physics of rocketry. He could present a strong case for continued funding, even though his experimental results, as of the early 1930s, were still meager. The romance of rocket experimentation held Goddard in good stead—up to a point.

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32 *Goddard Papers, Vol. II* (1970), pp. 721-722.

33 *Goddard Papers, Vol. II* (1970), pp. 727-728. Goddard's disappointment was not assuaged by Merriam's reply two days later: " May I say that the appropriation made by the Carnegie Institution is a contribution toward your work regardless of what other support may come...." Merriam did indicate that more might be forthcoming at a future date.

34 *Goddard Papers, Vol. II* (1970). See: Letter, Goddard to Merriam, May 10, 1930, in which the scientist reports increases in gas velocity from 4000 to between 5000 and 6000 ft./sec. pp. 740-741.

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Goddard's struggle exemplifies the slow path of American science toward accepting rocketry as a full-fledged member of its fraternity in the early Twentieth Century. Goddard had received funds from the US military, from philanthropists, and from a national museum which saw its role as fostering American arts and invention. In a new realm, a triumvirate as old as the republic was again beginning to assert itself. American philanthropy and the US government (both through its military arm and the Smithsonian) combined to help an academic with a bold idea.

This alliance between governmental authority, science, and private philanthropy stretched back to Thomas Jefferson's assumption of the presidency of the American Philosophical Society in 1797, the same year he took the oath as Vice-President of the United States under John Adams. Jefferson personally lent Charles Wilson Peale funds for the excavation of a New York State site known to contain mammoth bones. In 1803, as President, although unable to secure Congressional support for Peale's new Museum of Natural History, Jefferson negotiated financial help from various states. [35] The pattern would repeat itself time and again in the coming years.

In the 1920s, however, the concept of public support for rocket experimentation had not firmly entrenched itself in the ground of American public opinion or among the academic community. Rocketry was not yet a 'true science,' worthy of commitment on a broad scale. Goddard the inventor was not at all interested in publicity. In 1929, there was no community of

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35 Merrill D. Peterson. *Thomas Jefferson and the New Nation*. Oxford, The University Press, 1970, pp. 576, 736-737.

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academics sharing the fruits of their research in the field, no university boasting a department of rocket engineering. The shadow of Jules Verne, the master of science and fantasy, still lay across the field of rocket exploration.

The seed of a new technology was just beginning to germinate in 1930. Rocket builders and experimenters forged fragile links across political, geographical (not to mention academic) boundaries throughout the first decades of the twentieth century. Newspaper and magazine articles about rocket experiments had heightened public interest across the United States, Europe, and Russia. In the 1930s, the consciousness of rapid, unexpected, and dramatic change in historical circumstances was clearly imbedded in the public mind. Futurism, an early twentieth century artistic movement closely associated with the transformative aspects of technology, had found cinematic expression in Fritz Lang's *Metropolis* (1927) and *Frau im Mond* (1929); the latter film, a portrayal of space travel, employed the Rumanian-German rocket engineer Hermann Oberth (1894-1989) as technical advisor. [36] This film helped to legitimize rocket experimentation, at least for the European public.

In New York, the American Interplanetary Society (later known as the American Rocket Society) was founded April 4, 1930. Its first vice-president, G. Edward Pendray (1901-1987), sailed for Europe the next year to visit the famed French aircraft and rocket designer, Robert Esnault-Pelterie. [37]

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[36] Ley, Willy. *Rockets, Missiles, and Space Travel.* New York, The Viking Press, 1958, p.124.

[37] Robert Esnault-Pelterie (1881-1957), early Twentieth Century aeronautical experimenter and pioneer, built an all-metal monoplane in 1907, and designed an airplane engine with axially-mounted cylinders for it. He also wrote extensively on rocketry, predicting interstellar travel, once atomic power could be harnessed to spacecraft. Esnault-Pelterie experimented

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Pendray never did connect with Esnault-Pelterie on this trip. In Germany, however, he met Willy Ley, one of the members of the *Verein für Raumschiffahrt*, an amateur rocket society whose alumni included Wernher von Braun (1912-1977) and Walter Dornberger (1895-1980). [38] Even though Pendray and the members of the *VfR* could not speak each other's tongue, the American witnessed a dramatic test of a liquid-fuel rocket motor at the *Raketenflugplatz* near Berlin. This test provided the incentive for the American Rocket Society's own first attempts to design and fire a liquid-fuel rocket.

Remarkably, neither Pendray nor Ley had any knowledge of Goddard's test firing five years earlier. This gap was clearly the result of the Massachusetts inventor's reluctance to publish the results of his work. Goddard's 1919 Smithsonian report, *A Method of Reaching Extreme Altitudes*, was remarkably detailed, both in its mathematical and experimental investigation of rocket construction and dynamics. But a January 1920 editorial in the *New York Times* ridiculed Goddard's accomplishment, asserting that "after the rocket quits our air and really starts on its longer journey, its flight would be neither accelerated nor maintained by the explosion of the charges it might then have left. To claim that it would be is to deny a fundamental law of

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with rocket motors, and tested the dynamics of rocket combustion extensively. French government officials generally ignored or ridiculed his early work. He persisted, nevertheless, and established a prize, funded by André Louis Hirsch, called the 'REP-Hirsch International Astronautics Prize,' in 1928. GALCIT's Frank Malina sought this prize in 1939, won it, but did not learn of his achievement until the Second World War had ended. For further insights into 'REP's' life, see: Lise Blosset's 'Robert Esnault Pelterie: Space Pioneer,' in *First Steps Toward Space*, (1985), pp. 5-32.

38 Pendray, G. Edward. 'Early Rocket Developments of the American Rocket Society,' (1967). In *First Steps Toward Space*. AAS History Series, Volume 6, San Diego, 1985, p.141. Pendray never did meet with Esnault-Pelterie on this trip. Von Braun and Dornberger were leaders of the German Army's rocket development progarm at Peenemünde, 1935-1945.

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dynamics, and only Dr. Einstein and his chosen dozen, so few and fit, are licensed to do that." [39] This editorial shocked Goddard, and he specifically requested that the Smithsonian not publish his future reports. Goddard's reaction to press criticism was to withdraw from public view. From 1919 until 1936, he held to this policy. He remained content to create and patent his often innovative discoveries. Rarely did the scientific community hear of his results. Then, on March 16, 1936, his second Smithsonian report, *Liquid-Propellant Rocket Development*, appeared. [40]

The credit for his new foray into the public eye belongs, in part, to Lindbergh. The aviator convinced Goddard to present his experimental results at a meeting of the American Association for the Advancement of Science in December 1935. A favorable reaction from fellow scientists strengthened Goddard's resolve, and he published data (accumulated since 1930) the next year. If Goddard had joined his efforts to those of other rocket experimenters, there is little doubt the pace of American rocketry would have accelerated. In his 1936 report, Goddard mentioned progress in developing both pendulum and gyroscopic stabilizers for his liquid propellant rockets. But over the years, reluctance to share his work with the scientific community at large became Goddard's hallmark.

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[39] *A Method of Reaching Extreme Altitudes*, Smithsonian Miscellaneous Collections, Vol. 71, No.2. Washington, Smithsonian Institution Press, 1919. Both Goddard's pioneering monograph and the *New York Times* response, January 18, 1920, have been published in their entirety in the volume, *Exploring the Unknown, Selected Documents in the History of the US. Civil Space Program. Volume I: Organizing for Exploration.* Ed., John M. Longsdon. Washington, NASA History Office, NASA SP-4407, (1995) Documents 1-7,1-8, pp. 86-133.

[40] *Liquid-Propellant Rocket Development.* Smithsonian Miscellaneous Collections, Vol. 95, No. 3, 1936. In *Exploring the Unknown, Vol. I* (1995) Document 1-9, pp. 134-140.

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The American Rocket Society shared no such predilection for secrecy. From the start, the ARS sought publicity, issuing a *Bulletin* and attempting regular meetings at the Museum of Natural History. [41] Although the ARS was initially an amateur organization, its members included skilled mechanics, engineering students (one at MIT), and talented inventors like Alfred Africano, who was to win the Robert Esnault-Pelterie-Hirsch Prize in aeronautics in 1936 for an innovative high-altitude rocket design. The ARS had repeatedly tried to enlist Goddard's support, even offering him membership. He steadfastly refused, considering the group too amateurish. Nevertheless, in July 1937, the ARS journal, *Astronautics*, devoted an entire issue to honoring Goddard, calling him the 'Number One Man of Rockets.'[42]

Undoubtedly, the ARS and other early American rocket societies suffered by their amateur status. Despite early contacts between German and American rocket builders, the support network among amateurs and academics was neither widespread nor tightly woven in those early days. As Frank Winter described in his informative study, *Prelude to the Space Age, The Rocket Societies: 1924-1940*, most academicians would not venture to "risk their professional reputations by being associated with a crank cause." [43] A small

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41 Frank H. Winter. *Prelude to the Space Age, The Rocket Societies, 1924-1940*. Washington, Smithsonian Institution Press, 1983, pp. 74-75. The Bulletin actually bore the title of the *Bulletin of the American Interplanetary Society*. Once the group had begun to build rockets in 1931, the terms ARS and AIS became interchangeable. In 1934, at the society's Fourth Annual Meeting, the name *American Rocket Society* became official.

42 *Astronautics*, the Journal of the American Rocket Society. No. 37, July 1937. Goddard contributed an article on 'Rocket Motor Efficiency' to this issue.

43 Winter (1983), pp. 76-77. If rocketry were entirely the 'crank cause' that Winter characterized it as, neither the Smithsonian nor the US Army would have funded Goddard's work. Not all rocket experimentation was amateur, and even amateurs

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number subscribed to the *Bulletin of the American Interplanetary Society,* but no more than three Ph.D.s openly associated with the society.

David Lasser, first president of the American Interplanetary Society and a graduate of MIT, took the enrollment of these three in an optimistic vein and in 1931 proposed the formation of "a Committee on Experiments and the creation of an International Interplanetary Commission." [44] The AIS actually began experiments in the construction of rockets in 1931, and within one year a prototype liquid-fuel rocket had been built, christened 'ARS 1.' The extent to which learned engineering advice and calculation went into its building is unknown; Lemkin (the chemist) contributed articles on rocket fuel to the society's journal, but there is no indication how either of the other two scientists contributed technical advice. [45] With the exception of Goddard's work, most of America's early attempts at rocket construction originated with skilled amateurs. Men with mechanical ability and resourcefulness, fascinated by the dream of space flight, built many of the early rockets.

The plans for ARS 1 were similar to those of the German 'Repulsor' rocket built by the *VfR,* as Pendray had witnessed a 'Repulsor' test firing while in Germany. The 'Repulsor' was a modified stick rocket with its motor mounted on top of liquid oxygen and gasoline tanks. The gasoline tank was only partially filled; gaseous nitrogen pressurized the fuel before its short

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might prove quite skilled technicians.

[44] Winter (1983), p. 77. The three academics were: Dr. William Lemkin, a chemist who taught in a New York City public school, Prof. Horton Sheldon, an NYU physicist, and Dr. Clyde Fisher of the American Museum of Natural History.

[45] Winter (1983), pp. 76-77.

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journey up to the combustion chamber. *VfR* designers had encased the rocket motor in an aluminum water jacket, hoping to cool it down during flight. Since Pendray was familiar with the rocket's design and many 'Repulsor' rockets had successfully flown, the ARS gravitated toward replicating it. Constructed in October and November 1932, ARS 1 never flew. During a November 12 test, the fuel ignited, burning fiercely; but the oxygen supply gave out before the rocket could lift free of the launch stand. ARS 1 then caught fire, fuel spewing over the test site. [46]

In triumph and in failure, the ARS showed considerable ingenuity in the design of its test vehicles and no small degree of courage: the builder of one particular ARS rocket, Bernard Smith, had the pluck to run out and ignite his creation with a gasoline torch after its ignition apparatus failed. The rocket shot off from the launching tower at Marine Park, Staten Island (on May 14, 1933) before Smith could retreat to the safety of a makeshift barricade. [47] Clearly, theoretical knowledge of rocket design was in a rudimentary state in the United States in the first part of the 1930s. The works of Tsiolkovsky and Oberth had not yet been published in America, although by the middle of the decade, writings by European scientists and experimenters began to filter in. Since Goddard was not alone in his fascination with rockets, rocket societies sprang up, not only in New York, Cleveland and in Berlin, but in Liverpool, Buenos Aires and Leningrad.

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[46] Winter (1983), pp. 78-80.

[47] Pendray (1967), p. 144, and p. 145, *nb*. Smith was unhurt in the mishap. The rocket reached an altitude of 250 feet before its oxygen tank exploded. The vehicle then dropped into Lower New York Bay and was retrieved by a fisherman.

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The First World War had not only accelerated the pace of technological change, but had created an intellectual and psychological climate in which the unrealizable suddenly seemed within reach. A profound perceptual re-orientation had taken place, from t ·ealm of physics to the eye of the artist. Time and space were—or a. . .ıed to be—compressible, to an extent never before possible. The wireless radio transmitter remains an obvious symbol of this change. On the other hand, there was the attitude of Bernard Smith, the intrepid mechanic and designer, who had joined the ARS in 1932: "It was a lousy planet; the rocket ship was the only way to get off it." [48]

Of all the accomplishments of the American Rocket Society during the 1930s, the most dramatic was the creation, between 1936-1938, (coincidentally the same years of the GALCIT Rocket Research Project's early struggles) of a regeneratively cooled liquid fuel motor by James Hart Wyld, then a physics student at Princeton University. Wyld had become a member of the ARS in 1936. The principle of a regeneratively cooled rocket motor is straightforward: the fuel passes through a vaporizing jacket that surrounds the combustion chamber. The chamber jacket cools the nozzle as the fuel is preheated by convection just before injection into the motor. The regeneratively cooled motor marked a significant technical achievement and a step toward the 'professionalization' of the American Rocket Society.[49] This rocket motor type is the standard in liquid-fuel rockets of the present day. In fact, GALCIT's

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48 Winter (1983). p. 77.

49 Winter (1983), pp. 83-84.

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Martin Summerfield worked on the design and physics of regeneratively cooled motors himself in the early 1940s.

Wyld's liquid-cooled rocket motor was not the first of its type. The idea of circulating fuel or oxidizer around the combustion chamber as a coolant belonged first to Tsiolkovsky and Oberth. [50] The Cleveland Rocket Society (1933-1937) had built two in 1935. The creation of the Cleveland society's founder, German-born Ernst Loebell (d. 1979), the motors were wrapped in copper coils carrying "both fuel and oxidizer which were then injected...and ignited. " [51] The motors could theoretically produce over 5000 lbs. of thrust. The Cleveland rocketeers never fired them, as the Cleveland Rocket Society's funds ran dry.

The combination of lack of funds for research and construction, coupled with a poorly developed research network led to the demise of many small rocket societies in the 1930s. Rocketry was still considered to be a new, fringe science. Caught in a peculiar twilight area between institutional support for a physics professor who worked largely in secret, and the independent efforts of

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50 Hermann Oberth. *My Contributions to Astronautics.* From: 'The History of Rockets and Astronautics: Materials of the 18th International Astronautical Congress, Belgrade, 25-29 September, 1967.' In: *First Steps Toward Space* (1985), pp. 129-140. As early as 1903, according to Frank Winter, Tsiolkovsky had drawn a model of a regeneratively cooled liquid-propellant rocket motor. See: Frank Winter. *Rockets into Space.* Cambridge, Massachusetts, Harvard University Press, 1990, p. 11.

51 Winter (1983), p.101. See Chapter VIII, 'The Other Societies,' for information on the Cleveland Rocket Society. G. Edward Pendray, first vice-president of the ARS, wrote in an article, 'Pioneer Rocket Development in the United States,' that Goddard actually first "...developed self-cooling motors...." See: *The History of Rocket Technology: Essays on Research, Development, and Utility.* ed., Eugene M. Emme. Detroit, Wayne State University Press, 1964, p. 21.

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amateur societies, there was no firm middle ground upon which American rocket experimentation could rest.

While Americans slogged ahead at a slow pace, in 1934 the German Army actually began to suppress amateur rocket groups, including the *VfR*, and co-opt their members into secret military work. By the mid-1930s, the Nazi government was mobilizing to overcome the difficult hurdles involved in liquid-fuel rocket design, and planning to use the rocket as a radically new weapon. In May 1937, Peenemünde had opened on the Baltic Sea, an entirely self-contained complex devoted to missile research, design, construction, and testing. Although state funding was not always adequate to the demands of the engineers and military leaders at the new scientific center, neither was support ever fully denied.

On October 3, 1942, German researchers successfully test-fired the A-4 (V-2) rocket, and the ballistic missile age was born. [52] In less than ten years, an authoritarian state apparatus had recruited and organized professional staff, enlisted the collaboration of key industries, and constructed a technology that was, in several fundamental areas, groundbreaking. Meanwhile, in the United States, Dr. Jerome Hunsacker, head of the Guggenheim Aeronautics Department at MIT, had just four years previously—1938—declined to accept funds for rocket research, commenting that "Kármán (Theodore von Kármán, of Caltech) can take the Buck Rogers job." [53]

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52 Michael J.Neufeld. *The Rocket and the Reich: Peenemünde and the Coming of the Ballistic Missile Era*. New York, The Free Press, 1995, p. 164.

53 Clayton Koppes. *JPL and the American Space Program*. New Haven, Yale University Press, 1982, p. 8.

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Of course, Hunsacker's comment in no way implies a blanket attitude of derision and skepticism among American physicists of that era toward rocket development. As is common in any field, developing and expanding the knowledge base for existing technologies (such as aeronautics) took precedence over primary research in a new and seemingly impractical technology. Perhaps more fundamentally, it is difficult to transform an idea held in the realm of fantasy for ages into a scientific discipline. After all, one need only think of Johannes Kepler's *Somnium* (1634) to sense the gap between the vision of space travel and its accomplishment. [54]

Well into the 1930s, several scientists believed space flight to be impossible, claiming that no man-made object could escape the gravitational force of the earth, or that a reaction motor could not function in a vacuum, with no atmosphere to 'push against.' Ignorance is not solely the province of the uninformed. Yet, in the 1920s and 1930s, a chasm separated the public imagination from solid accomplishment in the realm of rocket dynamics and spaceflight. At the heart of popular fantasies, tales of flights to the moon and escape from earth, lay a core of disbelief—space flight was still a dream. Goddard's reaction to public ridicule, while extreme, indicated that he sensed this doubt and intended to reject it by shielding himself from open scrutiny. Frank Malina's skin was somewhat thicker: he laughed off newspaper sensationalism on rocket experimentation as 'nonsense.' [55]

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54 Jefferson Hane Weaver, editor. *The World of Physics: A Small Library of the Literature of Physics from Antiquity to the Present, Vol. I.* New York, Simon and Schuster, 1987. Kepler's *Somnium* or Dream, appears on pp. 464-479.

55 'On the GALCIT Rocket Research Project, 1936-38,' in *First Steps Toward Space* , Frederick Durant III & George S. James, eds. San Diego, Univelt, for the American

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The tendency to regard the work of rocket science sensationally is especially keen. There is, admittedly, an inherently dramatic quality to the field. To assume, however, that journalists will automatically over-dramatize or debunk the reporting of rocket flight or experimentation is to bend the truth. When, in October 1933, the fuel tank on one of Ernst Loebell's rocket motors exploded unexpectedly, the founder of the Cleveland Rocket Society asked the reporter present not to write up the mishap. The reporter complied. [56]

Implicit in this story, and in Goddard's, is the power of publicity to adversely affect research. Loebell, in all likelihood, worried about a loss of financial support, reduced membership in the Society, and police interference with the activities of the CRS. Then, too, press reports of rocket launchings attracted a certain 'fringe' element in society, some of whom saw their own fate as linked to the activities of the rocketeers. The human imagination is profoundly stirred by the possibility of leaving the earth. Outlandish ideas and offers of help from the general public, excited about the prospect of rocket travel and sensing its imminent realization, occasionally distracted scientists from their work. From time to time, it became necessary for GALCIT team members to respond to mail from the general public. [57]

Astronautical Society, 1985, p. 117.

56 Frank H. Winter. *Prelude to the Space Age—the Rocket Societies: 1924-1940.* Washington, DC, Smithsonian Institution Press, 1983, p. 101.

57 Several letters to Frank Malina from rocket 'enthusiasts' are on file in the *Frank Malina Collection* at the Library of Congress. Malina took the time to answer many of them, but occasionally expressed annoyance (privately) about the distraction from his work. One Long Island man even volunteered his services as a parachutist willing to ride in a rocket's nose cone. Malina's rejection of the man's services was polite and cordial. See: The Library of Congress (Madison Building), The Frank J. Malina Collection, particularly Box 12, for early reports, letters, and miscellany of the GALCIT Rocket Research Project.

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The question is: did scientists shy away from research as a result of 'fringe' public reactions? The answer is emphatically no. Like many in the general public, established scientists were slow on the uptake. They could not conceive of the reality of successful rocket launches, much less foresee the implications of such exploration. This is the primary source of the lag in research development of rockets, at least in the 1930s. Even in December 1945, Vannevar Bush (1890-1974), Director of the Office for Scientific Research and Development, told the Special Senate Committee on Atomic Energy : "Let me say this: There has been a great deal said about a 3,000 mile high-angle rocket. In my opinion such a thing is impossible and will be impossible for many years....I say technically I don't think anybody in the world knows how to do such a thing and I feel confident it will not be done for a very long period of time to come." [58]

This comment is as revealing of Bush's own lack of awareness of (then) current states of research as it is of his deep skepticism. Von Kármán had just returned in the summer of 1945 from a fact-finding visit to German V-2 factories for General Henry (Hap) Arnold, chief of the Army Air Force. In Germany von Kármán and his investigative team had found evidence that Nazi scientists had, at the least, developed theoretical plans for a modified A-4 capable of skipping across the upper atmosphere and striking targets as far

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58 *Inquiry into Satellite and Missile Programs*, Part I, Preparedness Investigating Subcommittee, Senate Committee on Armed Services, 85th Congress, 1st and 2nd sessions, November 1957-January 1958, p.283. Quoted in R. Cargill Hall, "Early US Satellite Proposals," a chapter in *The History of Rocket Technology: Essays on Research, Development, and Utility.* Eugene M. Emme, editor. Detroit, Wayne State University Press, 1964, p. 68.

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away as New York. His report, entitled *Where We Stand,* was, however, kept secret, and it is quite possible that Bush never saw it. [59]

Public interest in rocket experimentation, coupled with lack of widespread financial support from institutions which could contribute to developing the field made for an odd situation. Most American rocketeers in the 1930s had to dig into their own pockets for funding, or rely upon the generosity of donors—some of them anonymous. Rocket institutes were at least a decade away. The same could not be said for aeronautical institutes, however, which began to appear around the nation by the mid-1920s. Following Lindbergh's dramatic flight in 1927, the power of aeronautics to change the world had finally become a fixture in the popular culture.

In 1925, the Guggenheims, not content to provide seed money for individual inventors such as Goddard, began their sponsorship of aeronautical institutes across America. The Guggenheim family was to play a vital, if indirect role in the growth of rocketry at Caltech. The 1920s were a formative period in the history of aircraft design. Aeronautical institutes in Germany, France, and Italy, and Russia built wind tunnels of higher and higher efficiency, so that wind speeds of over 400 miles per hour could be attained. New designs, such as the cantilevered mono-wing, constructed with internal struts (the idea of Hugo Junkers, the German aviation pioneer), and even jet propulsion, were under study. [60]

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[59] Theodore von Kármán, with Lee Edson. *The Wind and Beyond.* Boston, Little, Brown, & Co., 1967, p. 289. What von Kármán and the Germans called the 'Transoceanic Rocket,' it should be emphasized, was only in the speculative stage. No such vehicle had yet been constructed.

[60] Von Kármán, (1967), p. 75.

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In 1925, only five American universities granted degrees in aeronautical engineering. These were the Massachusetts Institute of Technology, the California Institute of Technology, the University of Michigan, the University of Washington, and Stanford. Then, at New York University, Chancellor Elmer E. Brown "decided to place a (fledgling) aeronautical engineering program on a permanent footing." [61] He formed an organizing committee, which just happened to include Harry Guggenheim, to investigate the raising of $500,000 as an endowment for this purpose. In May 1925, Brown wrote a letter to Daniel Guggenheim, a letter personally delivered by Harry to his father, suggesting that the elder man endow a new 'Guggenheim School of Aeronautics.' Daniel Guggenheim consented, and on June 12, 1925, publicly announced his intention. [62] The school officially opened on October 27, 1926, boasting fifty-seven students, a two-story building and a nine-foot wind tunnel. [63]

The NYU grant was just the start. By early 1926, Harry Guggenheim had established a fund for the development of aeronautics; Orville Wright, Elihu Root, Jr., George W. Goethals, Albert Michelson, and of course, Charles Lindbergh were among its trustees. [64] The Daniel Guggenheim Fund for the Promotion of Aeronautics had an initial endowment of $2,500,000, with

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61 Richard P. Hallion. *Legacy of Flight: The Guggenheim Contribution to American Aviation* . Seattle, The University of Washington Press, 1977, p.28.

62 Hallion (1977), p. 29.

63 The Guggenheim School of Aeronautics at NYU was located on the University Heights campus in the Bronx.

64 Hallion (1977),p. 32.

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another $2,000,000 held in reserve for future donation. The first foundation devoted to aeronautical research at American universities had become a reality.

Even before the creation of the Guggenheim Fund, Robert Millikan (1868-1953), the shrewd and aggressive Chairman of the Executive Committee at Caltech, had written to Harry Guggenheim, urging him to "persuade his father to endow a school of aeronautics at Caltech because of the school's outstanding reputation in physics." [65] Millikan had as keen an administrative bent as he had the capacity for experimental design, and he was determined to bring Caltech into the ranks of the most productive, prestigious institutes of technology in the world. His strategy was to attract the best minds in each scientific field, pay them well, and reap the dividends in significant research accomplished. [66]

Coincidentally, the younger Guggenheim had expressed the wish that Caltech recruit a European scientist of some note as a condition for getting its new aeronautical school. Millikan then set his sights on Theodore von Kármán, then director of the *Technische Hochschule* in Aachen, Germany, whom he had met at a conference in Europe in 1923. The Hungarian, who first visited Caltech in September 1926, refused to accept Millikan's offer for nearly three years. He claimed a number of factors for his reluctance to leave his post in

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65 Hallion (1977), p. 47.

66 Theodore von Kármán made a most astute assessment of Millikan in his autobiography, *The Wind and Beyond*: He wrote that "Millikan believed in forming departments around prominent men who liked the cooperation of younger talents. He did not try to design an organization chart consisting of a number of boxes covering all fields and then try to fill every box, using mediocrities if good men were unavailable. He 'pushed' only those subjects for which he could get the best men." Von Kármán (1967), p. 124.

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Aachen. Von Kármán was almost fifty and he feared that a move to the United States would involve a lowering of his social position. (He believed that professors in the New World were not accorded the same status as those in Europe.) Secondly, his mother felt that only "family black sheep settled in America." [67]

Von Kármán's close bond to his family—and most especially to his mother Helen and his younger sister Josephine (affectionately known as Pipo)—influenced not only his social life, but his professional decisions, as well. Only in 1929, when the rising tide of anti-semitism in Germany reached Aachen and his own aeronautical institute, did the eminent Jewish aerodynamicist reconsider Millikan's proposal. Fortunately, Millikan was quite persistent, and despite von Kármán's continued ambivalence, the offer proved too good to resist.

Helen von Kármán refused to leave the Old World until nearly the day of their departure; she relented only to avoid a painful separation from her son. [68] In December 1929, von Kármán, with his mother and sister in tow, left for Pasadena. And so, both Harry Guggenheim's and Robert Millikan's hopes were fulfilled. Caltech met the philanthropist's expectation that it hire a European aerodynamicist of some renown. Millikan, perspicacious as ever, brought in just the scholar he had wanted. The Guggenheim Aeronautical Laboratory at Caltech was about to be born.

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[67] Von Kármán (1967), p. 141. Theodore von Kármán remained a bachelor his entire life. His sister, Pipo, was his steadfast companion until her death on July 2, 1951.

[68] Von Kármán (1967), p. 145. Oddly enough, von Kármán's two brothers Miklos and Feri elected to remain in Europe. This did not seem to prevent his mother from leaving.

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Chapter 3

The Guggenheim Aeronautics Laboratory at Caltech and the Birth of the Rocket Research Team

Robert Millikan loved to declaim the intellectual prowess of his Caltech staff. In his first interview with von Kármán in 1930, the physicist talked up Caltech's new genetics lab, headed by Thomas Hunt Morgan (1866-1945.) Von Kármán understood the gist of Millikan's remarks immediately: emulate Morgan and break ground in aeronautics and fluid mechanics, much the same way Morgan was doing in his fundamental research into the mechanisms of reproduction. [69] While von Kármán realized how deeply Millikan was devoted to enhancing the reputation and future of Caltech, he nevertheless had some reservations about teaching methods used at the institute, as well as the attitude of his new students toward their education. The Caltech students whom von Kármán had observed learned mathematics by rote, a method he found "somewhat conventional":

> Each day so many pages were assigned from a textbook. The teacher wrote equations on the board. The student copied them fervently in his notebook, while he tried to understand as much of the reasoning as he could. There were frequent examinations in some courses. Therefore, it was memory, not the creative impulses of the mind that was being trained. Of course, under such circumstances, the teacher would be barely remembered. [70]

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69 Von Kármán (1967), p. 147.

70 Von Kármán (1967), p. 148.

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Von Kármán's approach was more visceral, more intuitive. He emphasized basic concepts and hoped that students would "develop a feel for the principle at work." The core idea mattered, not the 'detail.' He asked his students: "How does an electron 'feel' in its environment? What makes it behave as it does? What makes the wing lift in the air? First in each case came the physical 'picture' with only the essentials, like a caricature. Then came the mathematics." [71] In fundamental ways, von Kármán still thought himself a student at his father's knee. He had never forgotten the dynamic lessons of the *Minta*.

The *Minta*, his father Maurice's creation, was an unusual high school in Budapest at which students learned in a open and non-authoritarian environment. At his beloved school, the elder von Kármán tried to show the relationship of learning to everyday experience, and he encouraged direct observation of reality on its own terms by the students, as well as free expression of ideas. [72] This approach, downplaying formality and concentrating on essentials and core concepts, was to give Theodore von Kármán an edge as a researcher and administrator. He had ultimate faith in the mathematical definition of a physical phenomenon and wanted to develop the mathematics of a concept as thoroughly as possible. But he was open to unconventional vantage points and refused to discard new ideas until they were disproved mathematically or failed experimentally.

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71 Von Kármán (1967), p. 148.

72 Among the Minta's graduates were Leo Szilard, George de Hevesi, and Edward Teller. See: von Kármán (1967), pp. 21-25.

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Yet, with regard to American higher education, von Kármán was not without a certain European subjectivity—perhaps bordering on arrogance. "The United States was quite backward in engineering theory related to aeronautics," he wrote in *The Wind and Beyond.* "When it came to scientific design of aircraft it seemed to me that most of the leading engineers of the United States were indifferent to values of theory. The schools did not incorporate into their courses such concepts as design optimization—finding the best design for the given conditions and materials." [73] Von Kármán believed American engineers ignored a direct search for novel materials and the best possible design because they faced no shortage of natural resources. They could afford to experiment until a proper design was discovered by trial-and-error. [74]

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[73] Von Kármán (1967), p. 142.

[74] Von Kármán did not mention the specific shortcomings of aeronautical engineering curricula in American universities during the 1920s. Yet he knew that only a handful of colleges offered such programs. Enlightened military aviators like Billy Mitchell and professional flyers such as Charles Lindbergh sought new designs and urged continued funding for aeronautics after World War I. Still, in the absence of any national consensus for the development of the airplane as an essential combat weapon in an era of shrinking military budgets, there was little incentive for the government to support aeronautical research. The alternative was private sector sources. If anything, the United States military was more interested in dirigible research after the war than in fixed wing heavier-than-air design. In Germany, however, given the restrictions on military development imposed by the Versailles Treaty, airplane designers and manufacturers concentrated on glider development, which provided them with opportunities to test new designs (such as Hugo Junkers' cantilevered mono-wing), without the burden of military expenditures. Of course, there was no lack of dirigible development, either, as the success of the Zeppelin factory near Lake Constance attested. Meanwhile, German designers and military men burned with a desire to rebuild their shattered military apparatus and secret work on aircraft was, even in the 1920s, going on in Germany and the Baltic States, where German manufacturers shipped parts for assembly. The United States had no comparable interest in rearmament and had to be nudged and goaded into further aeronautical research. See: von Kármán (1967), pp. 75, 111.

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Actually, the problem was more complex. American aeronautical research slowly began to develop in the 1920s. American engineers had no special proclivity for wasteful design practices, but aeronautical research facilities were still inadequate to the task of new airframe development. Congress first legislated support for aeronautics with the passage of the Air Commerce Act in May 1926, and funded 'Five Year Aircraft Programs' for the army and navy. Until the 1930s, the most successful commercial airplane flying in the United States was the Fokker F.VII.3m, a Dutch-built tri-engine. The Ford Motor Company designed and flew a competitor, the 4-AT, in 1926. [75] Most research focused on dirigibles, which were considered then to hold a brighter future in aeronautics than heavier-than-air craft. The new Guggenheim Aeronautical Laboratory at Caltech, with von Kármán as its first director, was in a position to take the lead in aeronautical research, if its new administrator charged ahead boldly. By the time von Kármán arrived in Pasadena, the laboratory had a wind tunnel in place, in part designed by him. [76]

The original prototype was the handiwork of two young Ph.D.s at the institute, Arthur L. Klein (1898-1984)—called 'Maj' after his rank in the Caltech student military organization—and Clark Millikan, the son of Caltech's chief executive. The younger Millikan had studied aerodynamics under the school's only professor qualified to teach it in the mid-1920s, Dr. Arthur Bateman. Von Kármán disparaged the influence of Bateman, and of

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[75] Hallion (1977), p. 18.

[76] The wind tunnel was completed by 1930. Von Kármán was involved as early as 1926 when he made a preliminary visit to Caltech to consult with the engineering staff about planning the new aeronautics laboratory. See: von Kármán (1967), pp. 124-125.

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his assistant, Albert Merrill, an instructor who taught airplane design, for Merrill was an inventor, not a mathematician. To make matters worse, according to von Kármán, Bateman and Merrill rarely spoke to one another. A dichotomy between mathematical theory and aeronautical design was the hallmark of aeronautical studies at Caltech in the 1920s. [77] Consequently, von Kármán was determined "to draw mathematics and engineering closer together—an ambition that was to dominate my career in America." [78] The marriage of theory with practical work became characteristic of von Kármán's educational strategy and influenced his guidance of Frank Malina, Hsueh-shen Tsien, and other members of the Rocket Research Team, as they began their explorations into rocket motor dynamics in the mid-1930s.

Although von Kármán approached the shaping of the Caltech aeronautical lab with a reformer's zeal—and a reformer's disdain for past solutions—Clayton Koppes, in *JPL and the American Space Program,* described an attitude among the Guggenheim Laboratory's "very proper academics" in the 1930s which was averse to risky or innovative research. These men "studied such topics as the wind resistance of aircraft and improvement of airplane propellers to boost speeds above 300 miles per hour." [79] Now, these are crucial aspects of aeronautical construction. Understanding how wind, fuselage and wing surfaces interact is key to the development of stable aircraft. Improving the performance of an existing technology is a challenge in itself.

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[77] Von Kármán (1967), p. 124. According to von Kármán, the two men did not speak to each other because one was a theoretician, the other a practical inventor. In his own words: "[Merrill] didn't care for mathematics and was more of an inventor type. [Bateman] seemed to know everything but did nothing important."

[78] Von Kármán (1967), p. 124.

[79] Koppes (1982), p. 2

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The Laboratory was, in fact, fulfilling its primary mission: the testing and modification of machines still quite advanced for their day.

Because it was a Southern California institute, Caltech forged links with the burgeoning aircraft industry in the region. New companies, such as Northrop and Douglas Aircraft, looked to Caltech for a steady supply of young aeronautical engineers and for an ongoing consultant-client relationship with the school. In this respect, the manufacturers were not disappointed, for Guggenheim Aeronautics Laboratory engineers collaborated with Donald Douglas in the creation of the DC-1 and its much more long-lived descendant, the DC-3. [80]

Meanwhile, von Kármán was aware of research in Italy, Germany, and Russia during the 1930s devoted to high-speed flight. In fact, his teacher and occasional academic rival, Ludwig Prandtl, first discussed high-speed wing design in 1922. In Italy, General Gaetano Arturo Crocco (1877-1968) had been interested in the subject as early as 1918, and "had written scientific papers on rockets, ramjets, and even on the possibilities of using atomic energy for interplanetary space-ship propulsion." [81] How was it then that Caltech, even under von Kármán's tutelage, was only pushing the limits of propeller design? The answer lies partly in the very relationship between the

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[80] Jack Northrop, the founder of Northrop Aviation, was a man whom von Kármán apparently admired greatly. Northrup's company built the 'Flying Wing,' a radically designed tail-less plane with optimal speeds of three to four hundred miles per hour. The air force supported, but later dropped the Wing as a viable project in the 1940s. At speeds up to four hundred mph. the Wing was stable, but above this speed, the plane began to shake and lose stability. Von Kármán believed Northrop was a visionary whose work on the Flying Wing failed because its "time had not yet come." Ironically, the air force ended its support for the Flying Wing because it became interested in higher flying speeds. See: von Kármán (1967), pp. 172-176.

[81] Von Kármán (1967), pp. 218-219.

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Guggenheim Aeronautics Laboratory and the aircraft industry in Southern California.

Often what is most novel is the last thing to sell. Aircraft industries in the early 1930s were less interested in radically new technologies than they were in refining existing ones, content to produce airplanes that would perform more efficiently. Such applied technology is dictated largely by production demands and current expectations. There is usually a lag—how much of a lag depends upon the industry—between the vision of a new creator and the implementation of new ideas.

Von Kármán, as the new director of the Guggenheim Laboratory at Caltech, was interested in cultivating links with industry. Not content to attend scientific conferences and remain within the academic realm, he was frequently in touch with leading manufacturers in the United States, Europe and even Japan. Von Kármán enjoyed portraying himself as an innovator at the edge of groundbreaking technology, but the truth is that for all his daring and willingness to consider new ideas, he was still a practical administrator. His own study of supersonic airflow was interrupted in the late 1920s by his departure from Aachen. "It wasn't until 1931, when I was settled in Pasadena, that I set out to study the supersonic airflow around a projectile in a serious way," he later commented..[82]

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82 Von Kármán (1967), p. 220.

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Von Kármán did not—or could not—charge ahead to create a laboratory and curriculum in aeronautics that emphasized the truly experimental. [83] This was in part due to the fact that the Aeronautical Laboratory had to draw upon professors and students in other disciplines within the Caltech engineering department for its academic lifeblood. And no doubt, von Kármán's administrative duties preoccupied him at the beginning. New courses in aerodynamics appeared slowly, many of them taught by von Kármán himself.

"My general procedure at Caltech," he wrote, "was to give the first semester of every new class myself. I started the course in Aerodynamics of Ideal Fluids and then relinquished it to Clark Millikan, while I taught something else that was new, such as Elasticity Applied to Aeronautics, a course which I later gave to Dr. E. E. Sechler. I even taught a semester in the Fundamentals of Dynamic Meteorology, a new concept of weather science, even though I was not a meteorologist, and then handed this course to W[illiam] C. Rockefeller." [84] Von Kármán was evidently not prepared to relinquish control over his new enterprise too quickly. Whether for reasons dictated by ego, concern over the fragility of its status at Caltech, or the still small academic infrastructure of the

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[83] For example, the wind tunnel at Caltech, completed in 1930, had a high 'energy ratio' of 5.6:1, indicating that for each kilowatt of energy required to power the wind tunnel's motor, a large amount of energy was imparted to the airstream flowing past the model in the center of the tunnel. But the tunnel was not equipped to produce supersonic speeds. It could not help Caltech aeronautical engineers exceed the limits of then current technology in airflow design. Its construction also engendered conflict among the engineers at Caltech. Arthur ('Maj') Klein, disagreed strongly with von Kármán's design recommendations for the wind tunnel, and wanted an 'open return tunnel', one in which air is funneled back to the outside atmosphere. Klein's design was a novel one, but von Kármán felt it would prove less efficient than his own 'closed return' system, arguing that the latter would allow less air fluctuation within the tunnel itself. See: von Kármán (1967), pp. 125-126.

[84] Von Kármán (1967), p. 155.

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laboratory, he rarely abandoned the tiller of his new ship in its formative years.

Von Kármán had an informal administrative style which held the new Aeronautical Laboratory in good stead. He was impressed by academic skill, intelligence and boldness. He skirted bureaucratic formalities whenever he could. Believing that form letters from schools to prospective students alienated them, von Kármán encouraged undergraduates to write him personal letters of introduction, and he relied partially on intuition when inviting new members of a class to join the Caltech community. Some of his students, like Duncan Rannie (1914-1988), who later became professor of mechanical engineering and jet propulsion at Caltech, merely wrote to ask whether or not fellowship aid might be available, even before the Institute admitted him. Von Kármán soon recruited him.

When it came to shaping the education of young engineers, von Kármán's perspective was equally unorthodox. The trend in engineering schools was to overload students with science courses. Feeling that engineers should not be abstract theoreticians, von Kármán insisted that his students get hands-on technical knowledge, thereby developing the "creative capacity to design new hardware." [85] He also wanted to create a new Masters program—calling it the 'Masters in Scientific Engineering'—which would emphasize practical design applications more than technical scientific work. Despite the academic environment in which he had been steeped back in Hungary, von Kármán felt the traditional track of BS.-M.S.-Ph.D. was too constricting to fit the demands of engineering design in the Twentieth Century.

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[85] Von Kármán (1967), p. 157.

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Oddly enough, von Kármán was less emphatic about the relative merits of practical versus theoretical education for engineers in his discussion of the founding of GALCIT. "Overly practical engineers," he noted, "can often be very impractical. They worry about things that do not come to pass." [86] Several of the key members of the GALCIT Rocket Research Group—Frank Malina, Martin Summerfield, Hsueh-shen Tsien, Clark Millikan—had or were pursuing their Ph.D.s while working under von Kármán, and it is these men whom he hoped to mold to the image of the new engineer he envisioned. In his autobiography, he wrote of these young engineers as if their education were his personal responsibility.

There is more than a hint of frustration with engineers who lack theoretical vision in von Kármán's autobiography. On November 7, 1940, the Tacoma Narrows Bridge, a new suspension bridge over Puget Sound, twisted and fell into the sea just four months after it opened. Von Kármán consulted with the bridge's designers after the catastrophe. To a man, they remained convinced that the static load on the bridge was not enough to drop it into the water. Von Kármán felt annoyed at their myopia. The problem wasn't weight; the bridge had succumbed to aerodynamic forces on its frame. He later confessed: "I hadn't reckoned on the depth and long-standing of the prejudices of...bridge engineers." [87]

Von Kármán approached the bridge collapse from the vantage point of an aerodynamicist; like a wing, the bridge responded to lifting forces of the wind

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86 Von Kármán (1967), p. 126.

87 Von Kármán (1967), p. 214.

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flowing across it. Under certain conditions, the bridge's design promoted turbulent airflow (*Kármán vortices* they are now called) causing the bridge to oscillate in tune with the rhythm of the wind flow. Nor did wind speed have to be high to create such vortices. At just the right wind velocity, the bridge, like an airplane's wing, would begin to undulate to the rhythm of the 'Kármán Vortex Street.' If the wind persisted just long enough, the bridge would twist like rubber.

Listening to the bridge engineers, von Kármán became exasperated. They asked incredulously whether or not he expected them to put a bridge model in a wind tunnel, as if it were a model airplane. But this was just the answer. He suggested that even small dynamic loads can be very destructive. Commenting on the experts' stubborn self-assurance, he wrote: "That was exactly the kind of thinking I was trying to overcome." [88] In this case, the engineers had *not* worried about something that "might not come to pass." They had failed to break free from the constraints of their training. Over-specialization in a field of expertise can lead to ossified thinking. This might, of course, hold true for an aerodynamic engineer as well. What von Kármán may have wanted to inject into the training process of the engineer was a knack for more spontaneous creativity.

Understanding materials was not enough for von Kármán. Still, any theoretician might make the mistakes of the Tacoma Narrows bridge builders. The solution which von Kármán sought was a mode of teaching which would balance expertise in the how-to's of construction with a certain

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[88] Von Kármán (1967), p. 214.

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inspiration or openness to new ideas. In practice, however, GALCIT's first director frowned upon *avant-garde* experimentation until a sound theoretical foundation had been developed. He did not forbid experimentation, but questioned its premature value. Practical tests in aerodynamics began with model building and wind-tunnel testing.

However, a rocket motor cannot be tested in a wind tunnel. It may be constructed in a laboratory; testing it indoors is entirely another matter, as the young GALCIT rocket engineers were to learn to their chagrin. The early history of rocketry is replete with stories of builders and experimenters who lacked a basic understanding of the theory of their craft. The cadre of engineers—dreamers all of them—who calculated the thrust necessary to propel a vehicle from a stationary position to a high speed and altitude within a matter of seconds were few in number. Tsiolkovsky, Sänger, Oberth, Esnault-Peltrie, and Goddard, divided as they were by national borders, time, and personal preoccupations, hoped to develop an international network for the dissemination of their ideas. The pace was exceedingly slow. Von Kármán and the new laboratory at Caltech were at a relative advantage. A new leader, eager to make his mark upon the academic scene in his adopted country, a young institution resting its reputation upon talent and prestige—both hoped to attract the brightest stars among the theoreticians, including Einstein. GALCIT was poised to make its mark upon aeronautical research.

A chance to do just this came in the mid-1930s. Through the doors of the Guggenheim Aeronautical Laboratory stepped some bright young men who went to Caltech specifically to study aeronautics, or who drifted into the field while there. William Bollay (1912-), a von Kármán assistant who earned his

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MS in mechanical engineering in 1934 and his Ph.D. in aeronautical engineering in 1936, was to play a key role in founding the GALCIT Rocket Team. Sometime early in 1935, Bollay read Eugen Sänger's (1905-1964) "*Neure Ergebnisse der Raketenflugtechnik,*" first published in the journal *Flug* in December 1934. [89] He then began to carry out "independent design and performance studies of rocket-powered aircraft." [90] Sänger's article mentioned maximum velocities of nearly 13,300 kilometers per hour (8250 mph) and a range of 5000 kilometers for a rocket airplane, performance far exceeding anything possible in a propeller-driven plane. The Austrian rocket engineer even wrote of utilizing centrifugal force created by the rocket-plane's flight at high altitudes (taking advantage of the curvature of the earth) to slingshot the vehicle higher and faster. [91] This surely impressed Bollay.

The detail and technical expertise with which Sänger covered not only propulsion dynamics, but supersonic flight and rocket-plane design, create the impression in the reader's mind that rocket propulsion is quite feasible. In the early 1930s, when the highest speeds attained by propeller-driven aircraft

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89 Sänger s article in *Flug*, translated as 'Recent Results in Rocket Flight Technique discusses the results of the Austrian rocketeer's theoretical and experimental investigations into liquid propellant combustion and ballistics. Technical in nature, Sänger's work presented formulae for combustion burning rates, types of fuel mixtures, the relationship between the nozzle size and internal pressure within the combustion chamber, even the design of rocket planes. The mathematics, if not overly complicated, were still quite detailed and based upon Sänger's knowledge of thermodynamics. There is no question that it would have been the kind of serious study of rocketry that would have attracted an aeronautical engineer such as Bollay. *Flug. Sonderfheft 1*, December 1934. See: National Advisory Committee for Aeronautics (NACA): *Technical Memorandum No. 1012* (April 1942). Pasadena, JPL Archives. Document # HC 5-427. The JPL document is an English translation.

90 Frank Malina. 'On the GALCIT Rocket Research Project, 1936-38;' in *First Steps Toward Space*. AAS History Series, Volume 6. Frederick C. Durant III, George S. James, eds. San Diego, published for the American Astronautical Society by Univelt Inc. 1985, p. 113.

91 Eugen Sänger. '*Neuere Ergenisse der Raketenflugtecknik,*' (1934). See: NACA Memorandum (1942), p. 32.

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were no more than three hundred miles per hour, this kind of performance must have seemed startling. Suddenly, rocket-propelled flight had appeared to leave the realm of fantasy. [92]

On March 27, 1935, Bollay presented a paper based upon his reading of Sänger's research at a GALCIT seminar on rockets. Entitled "Performance of the Rocket Plane," it immediately attracted the attention of a local paper, the *Pasadena Post*. [93] The *Los Angeles Times* carried the story as well, which indicates more than passing interest in the seminar. [94] On October 4, 1935, the Institute of Aeronautical Sciences in Los Angeles invited Bollay to repeat his lecture. The Caltech seminar had attracted enough publicity to warrant a second presentation in a more public venue. [95] Bollay was to continue his work in aerodynamics at Caltech until 1937, when he left the West Coast for Harvard. [96]

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92 Most likely, Bollay was unaware of the technicalities of Goddard's work in the 1920s on liquid-propulsion motors, since Goddard published little between 1919 and 1936.

93 Bollay's paper is catalogued as *GALCIT Rocket Research Project, Report 5*, 27 March 1935. The date of this report is out of synch with the creation of the Rocket Research Project itself, which did not get underway until formal approval was granted by von Kármán in 1936. The assumption is that the report was later issued under the rubric of the Rocket Research Group, although its original date preceded the founding of the group.

94 Malina (1967), p. 126, *nb*.

95 Malina (1967), p. 113. Malina gave *different* dates for Bollay's talk at Caltech in an oral interview with R. Cargill Hall, conducted in October 1968. There he noted that the Bollay lecture at Caltech took place in "December of 1935 or January of 1936." Since Malina extensively researched his 1967 paper and delivered it at the International Academy of Astronautics Symposium in Belgrade in September 1967, the March 1935 date for the Bollay seminar probably is most accurate. The Cargill Hall interview with Malina (October 1968) can be found at JPL Archives, listed alphabetically in the Oral History Archive under 'Malina.'

96 By 1947, Bollay was director of the Aerophysics Lab at North American Aviation. In 1951, he founded the Aerophysics Development Corporation, later purchased by Studebaker. Bollay also worked for Rocketdyne.

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In the audience for Bollay's talk at Caltech in March 1935, was Frank Malina, a transplanted Texan who had come to the institute in the fall of 1934. Malina was much less inclined than Bollay to migrate eastward in the 1930s, especially as he had received a scholarship from Caltech to study engineering after his graduation from Texas A & M University. Malina became a prime mover in shaping the direction of research and the success of the GALCIT Rocket Research Team. In fact, over the years, von Kármán and Malina developed a deep personal affinity for one another. Even though Malina's own father was very much alive in the 1930s, the young Texan came to regard the sixtyish Hungarian professor as his surrogate father. Their intellectual and personal collaboration lasted until von Kármán's death in 1963. [97]

Frank Malina had a previous interest in aeronautics and rocketry, ever since he had read Jules Verne's *Voyage from the Earth to the Moon* in Czech at the age of twelve (1924). His parents, Frank and Caroline, emigrated from Czechoslovakia to Brenham, Texas, approximately 50 miles northwest of Houston, joining other Czech emigrants there who came to work the fertile land near the Brazos river. [98] Malina was born in this southeastern Texas town, October 2, 1912. His father was a music teacher. As a young man, Frank Jr. found himself in the American heartland, sequestered in a small, somewhat isolated world. [99] This claustrophobic environment may in fact

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[97] Interview with Mrs. Liljan Darcourt Wunderman, Frank Malina's first wife, New York City, April 22, 1996.

[98] Frank Malina's father was born In 1881 and died in 1959. His mother, Caroline Sophie Marek , was eight years younger than her husband. The two married in Brenham Texas in 1911, and Frank was born just over one year later. Information courtesy of Marjorie Duckworth Malina, Frank Malina's widow.

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have stimulated young Malina's imagination. Rather than pursue a career in music, Malina put his mathematical talent to good use and entered a technical college. He studied Mechanical Engineering at Texas A & M (1930-1934). In his senior year, writing an essay for his English class, he even mused on the practicality of space flight. [100]

Malina entered the Guggenheim Aeronautical Laboratory with his plans in a somewhat confused state. He had misread the Caltech School of Engineering catalogue. When he arrived in Pasadena in the fall of 1934, with a scholarship in Mechanical Engineering, no one had told him exactly what courses he had to take in order to qualify for his scholarship.

> When the time came to register, I noticed that mechanical, aeronautical, and electrical engineering were all listed under the Division of Engineering. So I thought that since they didn't have departments like they did at A & M, it didn't matter which one of these I registered for, so long as it was the Division of Engineering, so I went and registered for Aeronautics. [101]

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[99] In her discussion of Frank Malina's early years, Liljan Wunderman could not fathom how an intellectual European family had ended up in a small hot Texas town with few relatives close by. However, Brenham lies in a particularly fertile farm belt, and land there was affordable, especially early in the 20th century. Czech and German émigrés had already settled in the region. In fact, although Brenham is not a large town, the Czech community there was close-knit. Mrs. Wunderman remembered family luncheons at which Frank's mother and his aunt cooked and baked together from morning until midday.

[100] In 1933, Malina wrote an essay for a Technical English course at Texas A & M on the possibility of interplanetary space flight. He asked: "Can man do what he can imagine? Now that man has conquered travel through the air his imagination has turned to interplanetary travel. Many prominent scientists of today say that travel through space to the Moon or to Mars is impossible. Others say,' What man can imagine, he can do.' Many difficulties present themselves to interplanetary travel. The great distance separating the heavenly bodies would require machines of tremendous speeds, if the distances are to be traversed during the lifetime of one man....If a machine left the earth, its return would be practically impossible, and those on earth would never know if the machine reached its destination. " Malina (1967), p. 113.

[101] Cargill Hall, *Interview with Dr. Frank Malina, October 29, 1968, Concerning JPL History, Early Years*. Pasadena, JPL Archives. Document listed alphabetically in Oral History Collection. See also: Washington, DC, NASA Historical Archives, Malina Collection, LEK 1/12/3. p. 1.

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A month after registration, a professor of Mechanical Engineering at Caltech, Robert Dougherty (1885-1978), called Malina into his office for a chat. Malina told him that he was "having a very nice time" teaching at the Heat Engine Lab and taking courses in aeronautics. Dougherty hesitated: "But don't you have a scholarship in Mechanical Engineering?" Malina answered, "Yes, is anything out of order?" Dougherty noted dryly that it was the first time he had heard of anyone receiving a scholarship in one engineering area but taking courses in another. He added, "It's too late to do anything about it, so just go ahead. However, I'm afraid we won't be able to renew your scholarship at the end of the year." [102] Nevertheless, in the spring of 1935, Malina received a scholarship in both Mechanical and Aeronautical Engineering, enabling him to continue his studies in aeronautics.

Still, it was not until the end of his first year at Caltech that Malina joined the staff of the Guggenheim Aeronautical Laboratory. First, he began working part-time as a crew member for the GALCIT ten-foot wind tunnel. Then he became a full-time graduate assistant at GALCIT. The following year, Malina's research led him into the study of propeller design. Along with another young graduate assistant, William W. Jenney, he conducted wind tunnel experiments on the characteristics of propeller design and operation, for which Caltech awarded him his Master's degree. [103] But this work left him ill at ease. The Guggenheim Laboratory was already carrying out high-

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[102] Hall, *Malina Interview*, October 29, 1968. p. 1.

[103] Malina (1967), p.113. Malina earned twin Masters Degrees in Mechanical and Aeronautical Engineering in 1935.

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speed wind tunnel research, in part pushed by von Kármán, who was then competing with Ludwig Prandtl to see who would first publish a more complete theory of turbulence. [104]

Turbulence is a serious problem in aerodynamic design as air velocities approach and exceed the speed of sound. A theory of how turbulence develops around supersonic or 'transonic' airframes is essential to understanding the effects of the sound barrier on aircraft. Malina felt uncomfortable working on propeller design when research horizons in the area of high speed flight seemed so much more challenging. Von Kármán himself was working on the theory of supersonic flight. Hoping to develop a new approach, Malina kept thinking about rocket propulsion.

Why he did not focus on ramjet design rather than rockets is not clear. A ramjet is a logical 'next step' advance over propeller-driven aircraft. The ramjet operates on the theoretically simple principle of the compression and superheating of air. The ramjet engine sucks air in through its cowling, compresses and heats it, forcing a pulse of hot air out the rear. Malina never mentioned jet propulsion as an alternative to his decision to the study of rockets. It was as if the memory of Jules Verne were still with him. Bollay's talk on the rocket plane doubtless prompted Malina to reconsider the direction of his research. Although Malina could not remember the exact timing of his decision, Bollay's seminar took place early enough in 1935 (March 1935) to influence him during the period of his work on propeller design.

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104 Von Kármán (1967), pp. 134-140.

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Bollay's presentation proved serendipitous for the future of rocketry at Caltech. There were relatively few students on the Caltech campus interested in the scientific aspects of rocketry and little money was available for research. Malina was looking for new avenues of study more adventurous and stimulating than propeller design. Knowing propeller aircraft would someday become passé, he wanted to help forge new technologies and develop new theories of aircraft performance.

Bollay's Caltech seminar on the "Performance of the Rocket Plane," was one of several regular meetings of institute engineers and graduate students designed to share the fruits of new research. But talk of rockets and space travel leapt easily from the campus to the world beyond and the *Pasadena Post* article attracted two outsiders as if a magnet had drawn them to Caltech. Neither Ed Forman nor John Parsons had a college education. Forman was a 'skilled mechanic,' Parsons, an explosives expert and a walking encyclopedia of chemistry. Both had been busy for some time building and firing rockets. They sought out Bollay; he was too preoccupied with his research to talk with them. Bollay referred them to Malina, who was evidently much more receptive, for by February 1936, Parsons, Forman, and Malina had worked out a program of study whose objective "was the design of a high-altitude sounding rocket propelled by either a solid or liquid propellant rocket engine." [105]

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105 Malina (1967), pp.113-114. A sounding rocket is a small vehicle, with relatively limited thrust, capable of arcing into the ionosphere. The rocket would usually be usually loaded with meteorological instrumentation or a camera; it was meant to return to earth shortly afterward by parachute.

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Despite differences in academic status and training, the members of the new group were initially a 'good fit.' They immediately set to work reading the available literature on rocket performance, most of which appeared on the pages of the American Rocket Society's monthly *Bulletin*. There was much European research available, including Tsiolkovsky's *The Rocket into Cosmic Space* (1903), translated into German in 1924; Oberth's *Die Rakete zu den Planeträumen (Rockets to Interplanetary Space)* (1923), and Esnault-Pelterie's *L'Exploration par Fusées de la Très Haute Atmosphere et la Possibilité des Voyages Interplanétaires (Rocket Exploration of the High Atmosphere and the Possibility of Interplanetary Voyages)* (1928). [106]

In his 1967 memoir Malina was very vague on the specifics of what the small rocket group studied. He could read German (but no French) in 1935, but von Kármán, of course, was fluent in both languages. Forman and Parsons spoke only English. So when Malina wrote that "we reviewed the literature published by the first generation of space-flight pioneers," he no doubt meant more than Goddard's work. But most likely any work by Tsiolkovsky or Esnault-Pelterie would have crossed von Kármán's desk for translation first. [107]

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[106] See Willy Ley (1958), pp. 505, 507, and 511. In addition to Sänger's work (mentioned above) and American Rocket Society reports, there was an important work on rocket propulsion by the Yugoslavian rocket theoretician, Hermann Noordung (né Potocnik),*The Problem of Space Travel: the Rocket Motor*, in print only in its German version in the 1930s (*Das Problem der Befahrung des Weltraums.) Das Problem der Befahrung des Weltraums* has been published in English by NASA. See: NASA History Series Document # SP-4026. eds. Ernst Stuhlinger, J. D. Hunley, and Jennifer Garland. Washington, NASA History Office (1995.)

[107] Malina (1967), p. 114.

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Initially, the construction of a sounding rocket did not seem an unattainable goal. Designing a small rocket, perhaps liquid fueled, capable of reaching the upper levels of the ionosphere was theoretically possible. Packed with a few simple scientific instruments, it might even bring back significant data on the nature of the upper atmosphere. Balloons could ascend to at least 75,000 feet. A small rocket might go even farther. The group did not set unrealistic expectations—or so its members thought. After all, Eugen Sänger had computed how a working rocket motor with an exhaust velocity of 4000 meters per second (over 8900 mph) might work. A motor with this much power would seem astounding for 1930s vintage rockets. However, as Malina himself commented, he and the other members of the small working group at Caltech were never able to understand how Sänger obtained his data, using the calculations published in his article. [108] Malina did not dispute Sänger's conclusions, but seemed a bit incredulous, remarking obliquely: "We were never able to understand the method Sänger used for presenting his experimental results." [109]

Parsons and Forman approached Malina eager to begin rocket experimentation. Malina, already assuming the role of leader, prepared a highly realistic program. The paucity of scholarly literature in English had taught them all to anticipate—at the very least—skepticism from the American scientific community about the feasibility of their goals.

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[108] See: Eugen Sänger: *Neure Ergebnisse in der Rakentenflug Technik* (1934), p. 13. Orbital velocity is approximately 18,000 mph, and a rocket with an exhaust velocity of almost 9000 mph would have had no difficulty reaching the upper atmosphere.

[109] Malina (1967), p. 114. In an interview with Dr. A. M. O. Smith in July 1996, Smith reported that Malina and Smith believed Sänger's calculations to have been 'fudged.'

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Accepting professional rejection of their project was part of the risk the GALCIT rocketeers assumed. In fact they soon realized that it would not be possible as yet to build a rocket motor which would "meet specified performance requirements for a sounding rocket." [110] Instead, they modified their goals and pushed ahead. Malina admits to "much argument" over setting their sights lower, But in the end—by February 1936—the men agreed to concentrate their efforts on designing and producing a "workable engine with a reasonable specific impulse." [111]

A clear two-phase plan became the road map for the group's work. Both parts of the plan involved difficult mathematical and physical hurdles. First, the thermodynamics of a working rocket motor had to be thoroughly understood. The men would have to properly design a nozzle, determine the correct volume of the combustion chamber, and choose the best fuel mixture for the motor. They would need to accurately calculate burn rates and exhaust-gas velocity, for these factors were crucial to improving thrust. The second stage would involve experimental work. Malina and the rocket crew planned static tests of liquid and solid propellant rocket motors, so as to evaluate the problems inherent in their construction.

Implicit in the goals of the new GALCIT Rocket Research Project—for so it came to be known, whether officially or not—was a recognition of the tension

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110 Malina (1967), p. 114.

111 Malina (1967), p. 114. The specific impulse, I_{sp}, of a rocket motor is the equivalent thrust of an ideal rocket motor which consumes one pound of propellant each second. It is thus a measure of the motor's efficiency in imparting thrust to a rocket vehicle.

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developing between Malina and Parsons over the role of experimentation in their work. Parsons and Forman wanted to fire off rockets as frequently as possible, regardless of the theoretical problems involved. Malina viewed this as hot-headed and insisted upon theoretical understanding of the underlying physical principles before building motors. This tension persisted throughout the history of the group. Although it was a factor to be recognized in the group's dynamics, it usually did not interfere significantly with the progress of their efforts.

Nevertheless, Malina and Parsons had very different temperaments and working styles. Parsons was impetuous, Malina more detailed and methodical in his approach. There were many practical and theoretical issues to be faced by the fledgling project; personality contrasts were occasionally an irritating factor within the group. All along, there was a common goal, but little money. Nor, as yet, had they received any professional recognition of their efforts. Despite friction, their goal took precedence and slowly a mutual respect for each other's expertise and doggedness grew.

Malina took credit for keeping the group together during its initial phase. As he said in his memoirs, after the birth of the Rocket Research Project, "the initiative rested with our group, and it fell to me to hold the group together." [112] The burden must have been a troubling one at times, considering that Malina was about to hitch his entire wagon to the design of a working rocket motor. His Ph.D. thesis, which he did not complete until 1940, a full four years after the start of the Rocket Project, would be devoted to the theoretical study of rocket propulsion.

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[112] Malina (1967), p. 115.

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In March 1936, Malina proposed to Clark Millikan, a professor at GALCIT, that "I continue my studies leading to a doctorate and that my thesis be devoted to studies of the problems of rocket propulsion and of sounding-rocket flight performance." [113] The younger Millikan demurred. Evidently believing that rocket science had little future, he told Malina to think instead of getting a good job with one of the local aircraft manufacturers. After all, GALCIT cultivated a synergistic relationship with the Southern California aircraft industry, and this seemed the most practical course for a bright Caltech graduate student. Malina, not deterred by the put-off, ignored Millikan's advice. It was not the last time he would do so. [114]

The next door to be opened was, of course, Theodore von Kármán's. Here, the reception was not nearly so discouraging. Malina regarded the elder statesman of GALCIT highly. He thoroughly agreed with von Kármán's research methodology: study the theoretics first before building the model. Mathematical underpinning for experimentation was a must with von Kármán. In addition, von Kármán had a fondness for the young Texas graduate student. Malina had done well in aeronautics courses in his first year at Caltech. Von Kármán's door was usually open to bright creative graduate students.

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[113] Malina (1967), p. 115.

[114] Clark Millikan later softened his initial skepticism and contributed to the work of the GALCIT Rocket Team, especially its understanding of the aerodynamic design of rockets. Malina welcomed him graciously, yet never quite forgot Millikan's early doubts. The two men often clashed over administrative decisions at the Jet Propulsion Laboratory in 1944 and 1945, revealing deep differences over policy.

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Characteristically, the GALCIT director took a chance. Malina's earnestness reminded him of a student at Aachen, in earlier days before political troubles drove von Kármán out of Germany. In the 1920s, when the Weimar Republic, under the watchful eye of the French and British military, could not develop new military airframes, one of his protégés had ingeniously come up with ideas for a new glider. [115] Malina's request to study rocket theory touched a resonant chord in von Kármán. In appealing to von Kármán as the court of last resort, Malina may have sensed that he would not face rejection. [116]

Malina had made a wise choice. According to von Kármán's memoirs, Malina did not appear alone at his office; he had Jack Parsons and Ed Forman in tow. The three young men struck von Kármán as earnest and capable. In his own words,

> [They] set forth modest aims. They wished both to build and test liquid and solid rockets which could be propelled perhaps twenty to fifty miles into space. This was not flying to the moon, the vision of most space pioneers, but a small rocket when properly instrumented could bring back information about cosmic rays and weather at the edge of space, an altitude which balloons had not been able to penetrate. And I felt this was worthwhile." [117]

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115 Von Kármán (1967), p. 238.

116 One year later, in 1937, Malina's artistic talents would come in handy for von Kármán. William Bollay, who had been illustrating an engineering textbook, *Mathematical Methods in Engineering*, turned over the responsibility to Malina. The book, published by McGraw Hill, appeared in 1940. Malina's illustrations were entirely of a technical, geometric nature. Curiously enough, it was Bollay who personally introduced Malina to GALCIT's director. This was undoubtedly in 1935, following Bollay's talk on the rocket plane.

117 Von Kármán, (1967), p. 238.

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Von Kármán apparently made up his mind the very same night and approved the project the next day, along with Malina's proposed thesis topic. In truth, the proposal Malina, Parsons, and Forman made to von Kármán was not so modest after all. Propelling a rocket of any size to an altitude of twenty, much less fifty miles, was not a task to be lightly undertaken. Robert Goddard's rockets, for example, rarely achieved altitudes of more than a few thousand feet in the 1930s. Even so, Malina was no wide-eyed dreamer, but instinctively felt that the GALCIT team could accomplish ambitious goals.

Von Kármán agreed to Malina's proposal, but his approval of the GALCIT plans was definitely conditional. Malina and his crew could only use the Aeronautical Laboratory in off hours, and absolutely no funds were placed at their disposal. The group would have to scrounge around for money and supplies and make the best of it. Beyond this damper on their enthusiasm, von Kármán may have sensed the political capital to be gained, should GALCIT and the young men succeed.

At the time, a professor of meteorology at Caltech, Irving P. Krick (1907-1996), was making a name for himself as a "commercial weather forecaster for movie studios." Krick looked favorably (at least initially) upon the possibility of receiving meteorological data from space. Then again, there was always Robert A. Millikan, Clark's father. He, too, hoped that a sounding rocket might provide him with data on cosmic rays from the fringes of space. Von Kármán, always a shrewd administrator and perhaps a bit of a gambler, was not entirely alone in his support of rocketry at Caltech. If the gamble paid off,

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GALCIT would be the first institute of advanced study in the United States to have a program in rocketry.

Whatever the political ramifications of his decision—and von Kármán may not even have considered them—his was still a bold move. While Clark Millikan had not refused Malina's request outright, he had certainly not endorsed it in ringing terms, either. Knocking on von Kármán's door was risky: how could Malina know that GALCIT's director would decide in his favor? After all, Robert Millikan's son was no ordinary professor, but one with immediate access to Caltech's chief. Malina may well have wondered whether or not von Kármán would (or could) act independently, regardless of the consequences.

Malina had correctly assessed the director's tenor: "[Von Kármán's] decision was typical of his unorthodox attitude within the academic world," he was later to write. [118] Both he and von Kármán were then aware of the limitations of propeller-driven aircraft. Von Kármán's investigations into the aerodynamics of high-speed flight and the recent completion of Malina's own wind tunnel studies of propeller design dovetailed nicely. A rather fortuitous set of coincidences found them both on a new road at the same time, a road which began with their vision that aerodynamics had reached another experimental threshold.

Beyond academic and political considerations, a relationship was developing between the two, as von Kármán more and more assumed the role as Malina's mentor. It was to be a complex, sometimes perplexing rapport that

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[118] Malina (1967), p. 115.

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grew and lasted until Von Kármán's death. Von Kármán was an engaging but absent-minded man who inspired the deep loyalty of many of his graduate students. They came to respect him; but with Malina, the tie involved a bond that transcended professional academic links. Von Kármán could inspire anxiety in Malina, the graduate student, typical of a young man facing his oral examination in aeronautical engineering before a recognized expert in the field—especially one with an exacting, probing mind. Malina developed great affection for his teacher and even married Liljan Darcourt (1921-), his first wife, in von Kármán's house in Pasadena. [119] The fact that GALCIT's Director had quickly approved of Malina's rather unorthodox thesis project must have deeply impressed the 24 year-old student.

Frank Malina always considered himself a multi-dimensional individual. Within the realm of science, he wrote, "I was sort of straddling between the two [worlds of theory and experimentation.] "[120] He knew the details of airfoil design, the tensile strength of materials; he could visualize and adroitly sketch complicated plans for rocket motor test stands. But he preferred the theory, the elegance of mathematics. Not so, his colleague, John Whiteside Parsons. Parsons had little inclination for defining the 'equilibrium pressure' of a burning gas inside a rocket combustion chamber, or determining the burn-time theoretically. He understood the properties of many explosives concretely; he was as facile with details as an expert chemist.

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[119] Interview conducted with Mrs. Liljan Darcourt Wunderman, April 22, 1996, New York City.

[120] Interview with Frank Malina, conducted by James H. Wilson, June 8, 1973 (transcribed May 1974.) JPL Archives, catalogued alphabetically under 'Malina, ' p. 26.

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Parsons had learned his craft by trial-and-error, and by voluminous reading in the field of chemistry.

John Parsons, a long-time resident of Pasadena, was born in Los Angeles on October 2, 1914. In January 1915, four months after his birth, Parsons' parents separated and his father left for Massachusetts. His mother, Ruth Virginia Parsons (1891-1953), had discovered her husband's infidelity; Jack Parsons grew up with her in Pasadena, while his father, Marvel Parsons (d. 1947), returned to Boston, where his family had a wholesale egg business.

Parsons clearly led an unsettled life. His parents were not wealthy. In Pasadena, Marvel Parsons sold automobiles. After his return to the East Coast, he joined the army and eventually rose to the rank of captain. [121] Between 1914 and 1950, John Parsons listed eight residences, three of them in Pasadena, others in Manhattan Beach and Redondo Beach, California. One home in particular, 1003 South Orange Grove Drive, became the scene of black powder rocket launches—many of them abortive—in the mid-1930s. [122]

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[121] This information on John Parsons' background, courtesy of Dr. John Bluth, JPL Archivist. Electronic mail letter, April 13, 1998.

[122] *Federal Bureau of Investigation File on John Whiteside Parsons,* Investigation conducted in Los Angeles, California, fall 1950. Report dated November 2, 1950. FBI File No. 65-59589. Pasadena, JPL Archives, p.13. (As there is no document number on this file, it is listed under the FBI file number.) The FBI investigated Parsons, ostensibly because he removed classified material on jet engine propulsion from his office at the Hughes Aircraft Co., Culver City, California. Parsons also claimed in this FBI interview that his parents briefly reconciled, taking him to Europe in 1929.

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When Parsons appeared at Caltech in early 1936, he came looking for an environment which would nurture his habit of building and igniting rockets. But this he was not to find. As Malina wrote in his memoirs:

> Parsons and [Edward S.] Forman were not too pleased with an austere program that did not include the launching, at least, of model rockets. They could not resist the temptation of firing some models with black powder motors during the next three years [1936-1939.] Their attitude is symptomatic of the anxiety of pioneers of new technological developments. In order to obtain support for their dreams, they are under pressure to demonstrate them before they can be technically accomplished. [123]

Since Parsons was no mere dreamer, but a doer, Malina was in a quandary: just how could he fit Jack Parsons into the project? Parsons remained something of an enigma to Malina. He did not know very much about Parson's educational background, yet he accepted him as part of the team that intended to build rockets at Caltech. The young graduate student saw Parsons as "at most [a man] who had finished high school. When I met him [1936] he already had a certain amount of experience with the manufacture of explosives. He acted as a chemist." [124]

Malina went on to admit Parsons' expertise in the realm of explosives manufacture, adding that Parsons possessed an unusual capacity to think flexibly. As various experimental snags developed in the creation of rocket motors at Caltech, Parsons' imaginative mind rescued the team from dilemmas which otherwise might have halted its work.

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123 Frank Malina (1967), p. 114.

124 Wilson Interview with Malina (1973), p. 16.

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Even though Parsons' success was narrowed by a lack of a theoretical underpinning in the dynamics of rocket propulsion, his enthusiasm for firing black powder rockets eventually produced results. Many rocket builders begin as amateur inventors, and a fire within drives them to design and build, rather than to sit in front of a blank sheet of paper, writing out calculations that may not actually bring dramatic and immediate experimental results.

There is a thrill associated with constructing a rocket, not only because the materials involved are inherently dangerous, but because rockets pierce the boundaries of the everyday world. They shatter the basic notion that humanity is earth-bound. For Parsons and his mechanic-colleague, Edward Forman, the attraction was both practical and esoteric, as if they were exploring the edge of reality. If Malina were not cut from the same mold, he probably would have scoffed at Parsons' interest in working at Caltech. After all, at first glance, why would a young graduate student, mathematically inclined, be receptive to the overtures of two unlettered strangers who shared little with him except his budding interest in rocketry?

Two factors, however, militated against Malina rejecting Parsons and Forman outright. First of all, Malina was already leaning toward research in this clearly exotic field. Secondly, Parsons and Forman had been conducting tests in rocket construction since the mid-1920s and were not novices at the

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craft. [125] Both men knew how to build rockets, and their skill could only complement Malina's work. [126] Beyond practical considerations, Malina himself possessed a certain aesthetic refinement. He was the child of musicians. Music demands discipline, both of the performer and the listener, but it also invites a certain emotional abandon. Parsons loved the lyrical, the odd. Malina admitted to friction between himself and Parsons due to their different approaches to rocketry, but he could not help but reveal a certain liking for, if not outright affinity with the amateur rocketeer.

Perhaps, had he not been a graduate student but already an established professor at Caltech, Malina would not have encouraged Parsons' hopes to affiliate himself with the prestigious institute. It was, in fact, strange—and a bit fortuitous—that William Bollay, who had sparked the birth of the GALCIT Rocket Team with his 1935 seminar, brushed Forman and Parsons aside. Malina, however, told Parsons and Forman that he "was also interested in this field and that perhaps [they] could work together." [127] So, he took the credit for initiating the working relationship. Malina obviously saw the opportunity presented to him and grasped it. At the time (early 1936), there was no one else at GALCIT who was willing to risk his graduate career entirely on the investigation of a new academically unproved field, not even Bollay, who signed on to assist in the project shortly after February 1936. [128]

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[125] According to Dr. John Bluth, JPL Archivist, Parsons began tinkering with rockets while in junior high school. He experimented in Ed Forman's backyard and learned more about rocketry from a skyrocket maker in Glendale, California.

[126] Koppes (1982), p. 3.

[127] *Interview conducted by* R. Cargill Hall with Frank Malina, October 29, 1968, 'Concerning the Early Years of JPL History.' Washington, NASA Historical Archives, LEK.1/12/3, p. 1.

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Frank Malina and John Parsons formed a unique duo, although neither would have admitted it. Each served as a foil for the other. Malina kept his distance from Parsons personally, but would not let him leave the Rocket Team. He sometimes grew exasperated when Parsons and Forman drifted off, as they frequently did in 1937 and 1938, to conduct experiments on their own or engage in work (at explosives factories in Southern California) which distracted them from their Caltech commitments. But Malina never seriously considered banishing John Parsons or Forman from the group. No doubt, there were times when he would have liked to.

Nor did Parsons quit. Although he seems never to have publicly revealed his thoughts about collaborating with Frank Malina, Parsons must have regarded Malina as the link to Caltech, and in no way did he want this link severed. [129] It was his ticket to respectability in rocket work, to a steady source of material, as well as to a test site. He could not have held Malina, a struggling graduate student, in awe; rather Parsons and Malina were equals in the effort, and both needed each other. Parsons was as open and unpretentious around Malina as he was with nearly everyone. In effect, like two planets revolving around the same star, Malina's and Parsons' orbits often intersected. Their striking differences would soon fuse their lives together.

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[128] Koppes (1982), p. 3.

[129] Some of Parsons' notebooks and writing survive, and have been placed in an archive of his magico-religious sect, the *Ordo Templis Orientalis*, in Texas.

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Chapter 4

The Rocket Team's Circle Widens

By autumn 1936 the GALCIT Rocket Research Team had begun to take shape. In merging the talents of engineering graduate students and practical experimenters with little formal training, the fledgling group faced internal conflicts which Malina usually finessed. Tension arose out of fundamental differences: should the essentials of rocket motor performance be mathematized, in the expectation that reality would conform to the theoretical construct, or should the realities of rocket performance be forged from hard metal and explosive powder alone?

Malina was trained to value the quantification of reality; the corroboration of theoretical fundamentals followed as a result of successful experimentation. Since Parsons and Forman would be working with Malina under the loose supervision of von Kármán at Caltech, it might be assumed that the theoretical approach would dominate. Although this was generally so, the GALCIT Rocket Team's working dynamic fostered a rough egalitarianism. There was no time for artificial divisions among them. It was the power of personal will, adherence to academic standards, and a bit of serendipity that shifted the focus from experimentation to theoretical investigation and back again.

Malina insisted upon theory and mathematics. Parsons and Forman would wander off on their own to launch rockets. Malina was no less interested in creating rocket motors than was Parsons, but the theoretician in him

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struggled against blind experimentation. There was a twinge of arrogance in his approach, although he rarely disparaged Parsons' talent. Malina had enough magnanimity and good sense to realize the unique contribution Parsons and Forman could make to the successful completion of the common task.

> I think that one could say that perhaps [Parsons'] general grasp of the theory of chemistry was sort of global rather than detailed; I mean, he was not mathematically talented. On the other hand, it seems to me that at this [early] phase of any kind of a development, a person with that kind of freewheeling brain is an extremely valuable person....We had many sympathies. He was very interested in the so-called cultural activities in the sense of music and especially in poetry which I think was sort of tied into his sort of mystical streak that he did have. [130]

Personal sympathies aside, Malina could not completely hide his annoyance at Parsons and Forman for their insistence that the new Rocket Group begin their exploratory work by firing rockets. Von Kármán had given the group no funds, and Malina was sure that a premature schedule of rocket launches would result in failure, discouragement, and the denial of any monetary help from Caltech or any other interested donors. Malina was right—a shortage of funds for equipment dogged the GALCIT rocketeers in the early years.

Beyond the risk of the Rocket Team limping along cash-poor, Malina's concern about over-zealous experimentation had another intuitive root: physical danger. Exploding rocket motors could wreak havoc on the quiet Pasadena campus. Rocket fuel was quite volatile. Neither von Kármán nor Millikan enjoyed the prospect of metal shards flying around the GALCIT laboratory. Perhaps Parsons would be just a bit too cavalier with his

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130 Wilson Interview with Malina (1973), pp. 16-17.

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experiments. As luck would have it—again—Malina's hunch was not off the mark. This is exactly what happened.

Mishaps became, if not quite commonplace, then certainly part of the fabric of the team's explorations. Parsons was usually not far from the scene of explosions, although his chemical concoctions were not necessarily to blame for them. Rocket motors are machines which generate tremendous operating stresses. Oddly enough, with all the misfirings which plagued the team's years of trial, not one serious injury resulted among any of the team's members. But one day early in the 1950s, all that changed for Parsons.

On June 17, 1952, John Whiteside Parsons died violently, a victim of his own carelessness with explosives. His death was the result of a flukish accident. The GALCIT phase of his life was over; the original group would, by the mid-1940s, transform itself into a government-run research arm—the Jet Propulsion Laboratory. After enduring the 1950 FBI investigation and working at various jobs, many of them involving pyrotechnics, Parsons prepared to move to Mexico, where he had just received a contract to work in an explosives factory. He was busy transferring chemical paraphernalia into a trailer near his home at 1071 South Orange Grove Drive in Pasadena when a coffee can containing fulminate of mercury slipped from his grasp and exploded, mortally wounding him.

It is not altogether surprising that Parsons should have died handling explosives. He often stored them around his house, sometimes leaving barrels of black powder open in the backyard. Malina had himself commented on how nervous stepping into Parsons' kitchen made him,

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knowing that glass jars in the cabinets were filled with tetranitromethane. [131] Parsons experimented with nitromethane as a possible rocket fuel, and had mentioned it in his first GALCIT Project report in 1937. [132] The volatile fuel was later discarded by the group in favor of a nitric acid-aniline combination.

Parsons' tragic and even foolish demise came after nearly twenty five years of rocket motor construction and experimentation of all sorts. Jack Parsons had devoted himself to disciplined, encyclopedic, yet unguided study of chemistry. Parsons even took an occasional college course in the field, but his formal education after high school was anything but systematic. Then there was the other side of his life, his love of the bizarre. He was a devoté of the occult and after 1939 joined the Church of Thelema, which followed the practices of English occultist and poet Aleister Crowley. [133] Parsons indulged in sexual experiment as part of black magic rituals. He was a man whose libido liberated him from conventionality and whose thought ranged adroitly from the mystical to chemical energetics. He freely crossed the bounds of

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131 Tetranitromethane, formula: $C(NO_2)_4$, a highly unstable organic compound, has a boiling point just over 125° C. For a short while, the GALCIT group considered using it as a rocket fuel, but decided its instability ruled out any practical use.

132 John W. Parsons. 'A Consideration of the Practicability of Various Substances as Fuels for Jet Propulsion,' GALCIT Rocket Research Project, Report #7, 10 June, 1937. Washington, Library of Congress (Madison Building), Malina Collection. Section II, Box 9.4 , 'Technical Reports. These reports, known collectively as the GALCIT Rocket Team's *Bible*, include papers by Parsons, Forman, Malina, *et al.* Caltech Archives publishes a *Guide to a Microfiche Edition of the Frank J. Malina Collection*, eds. Judith R. Goodstein and Carol H. Bugé (1986). This guide corresponds well to the location of material in the Library of Congress collection.

133 According to the 1950 FBI File on Parsons (see above, *nb.*), the Church of Thelema, alternately spelled Thelma, or Thelna, was incorporated in California on April 14, 1934, by a Wilford Talbot Smith. The report went on to count twenty members of the 'religious organization' and noted its headquarters at 1003 South Orange Grove Avenue, in Pasadena—Parsons' home at the time. Parsons himself described the Church as organized on the model of "the order of Oriental temples, an English society." He added that the Church was devoted to "the freedom and liberty of the individual." FBI File Report # 65-59589 (1950), p. 9. For references to Aleister Crowley's works, see the bibliography of this thesis.

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experience accepted by polite society, and, having been largely self-taught, adhered to no established system of chemical theory. What he read in textbooks he stored away in some inner mental file, ready to draw forth information and re-fashion it as he needed to.

Parsons' supple, undogmatic intellectual style, combined with his imagination, meant that he was invaluable to the GALCIT Rocket group. He chafed at the limits imposed by accepted scientific methodology, and as a result, was both an inspired thinker and a man difficult to control. The fact that his somewhat unruly mind injected itself into the nucleus of a budding group of engineers interested in rocketry led to several key discoveries in rocket propulsion. His impact on the GALCIT group was significant, although he fit into no clearly definable scientific mold.

The key to John Parsons' successful contribution to rocketry at GALCIT lay in his dedication to the belief that frequent experimentation would eventually lead to a breakthrough in the design of a workable rocket. His unconventional manner of thinking and his ability to shift gears led him to try new approaches as soon as his imagination and the failed results of a recent experiment suggested new avenues of thought. He knew the properties of hundreds of chemicals and mixtures, and combined this knowledge with an inspiration that allowed him to make connections between mundane aspects of reality and the work he was doing.

One nearly mythical story about Parsons' chief discovery—the castable solid propellant—centers around a walk that Parsons took one day late in 1941 or early 1942. There were problems with the first Jet Assisted Take-Off Rockets

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created by the GALCIT Rocket Research Group. JATO units were compressed black powder canisters which burned slowly like cigarettes, rather than in a few seconds, as one would expect with black powder. When installed under the wings of aircraft, they worked reliably in most cases, boosting the plane into the air and shortening its take-off run. This was a major achievement, both from a technical and a military point of view.

JATOs installed under the wings of US Navy fighter planes could quickly catapult the aircraft from a carrier's deck. Take-offs would be faster and more efficient; but there was one major problem. Early JATO units (that is, those built in 1941 and the winter of 1942) could not be stored at room temperature for more than a few days. Even overnight, cracks would develop in the compressed propellant and the entire unit would explode upon ignition. If the rocket motors were to be mass produced, as intended, such instability in the propellant was quite unacceptable.

Parsons had been experimenting with black powder rockets since the early 1930s, but now it seemed as if his research had led him to a dead-end. The future of the JATO was at stake, and so were the hopes of the struggling GALCIT Jet Propulsion Research Group—soon to become the Jet Propulsion Laboratory. Only a clear break with current technology might work. This is what Parsons sensed, and he went with his hunch. According to the version of the tale which appeared in Clayton Koppes' *JPL and the American Space Program: a History of the Jet Propulsion Laboratory,* Parsons was walking down a street in Pasadena one day, watching a group of roofers tar the roof of

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a house with asphalt. [134] At some point during his stroll, he had a flash of insight and realized that asphalt would make a practical solid propellant.

Asphalt is a common construction material and most of its uses are quite pedestrian. But Parsons knew its composition and understood it was both combustible, as it was a petroleum-based product, and castable. A castable substance was one which could be poured at melting temperatures (about 350° F, for asphalt) into a variety of shapes. It was malleable until set. If it were mixed with an oxidizer, poured into a shell, and a nozzle attached, it could become the staple of a solid propellant rocket motor.

And so it was to be. But if Parsons ever told anyone how he thought the problem through, or exactly when he first made the discovery, he never emphasized it. As far as is known, he did not fully describe the circumstances of his epiphany. Nor did anyone else in the Rocket Team recall a single version of the story which fully tallied with any other.

Martin Summerfield, in a 1994 interview, claimed that Parsons told him he had been following an asphalt truck as it was mixing its molten ingredients, and that this image gave him the idea for casting an asphalt-perchlorate mixture (potassium perchlorate was to be the oxidizer.) [135] The answer to this mystery may lie in Parsons' notebooks—or in his grave. Still, the story is remarkable, as the plasticized descendants of Parsons' solid propellants are

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134 Koppes (1982), p. 12.

135 NASA: Oral History Interview: Dr. Martin Summerfield, conducted by J. D. Hunley, September 27, 1994. Capitol Hill Reporting, Inc. Washington, DC, 1994. p. 20. NASA has no copy of this interview, but it may be obtained directly from Capitol Hill Reporting, Washington, which transcribed the interview.

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presently in use on many American missiles, including Space Shuttle boosters, as well as Polaris and Poseidon intermediate range ballistic missiles (IRBMs). Parsons' discovery mushroomed into results far overshadowing the creation of the small JATO unit.

Of course, the ultimate evolution of Parsons' invention was an unknown quantity to the graduate students and experimenters who had just formed the GALCIT rocket project. A significant bit of logical and imaginative thinking brought about a major redirection in an experimental pathway. This new discovery was not made without prior theoretical groundwork. The work done was not in physics but rather in chemistry. The originator of this new type of solid propellant was not well-versed in mathematics but was an inventor who was accustomed to create with his hands as well as with his mind. This was not unusual for the GALCIT men, although the impression Frank Malina gave was that a thorough grounding in theory preceded most of the group's successful rocket discoveries.

The passion for construction and experimentation so much associated with Parsons and Forman was infectious. The true excitement of rocket work came in the testing and firing, not on the drawing board. Apollo Milton Olin Smith (1911-1997) joined the Rocket Research Group for the most practical of reasons: a New Deal program actually assigned him to work at GALCIT. After January 1937 'Amo' Smith tested materials for rocket motor exhaust nozzles. Over more than a year (until the spring of 1938), he and Frank Malina also collaborated on at least three papers. [136] Smith was a young graduate student

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[136] Koppes (1982), p. 7. Koppes wrote that Malina and Smith distilled hundreds of hours of work into their paper, 'Flight Analysis of the Sounding Rocket.' In a 1986 interview, Smith admitted that he only worked occasionally on the GALCIT Rocket Research Project, "a few

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in aeronautical engineering when he first heard of the rocket group at GALCIT. The group had certainly not attracted much attention on campus in 1936, until the men conducted a rather comical and ill-fated series of liquid propellant motor tests in a dry abandoned canyon, the *Arroyo Seco*, that winter. These tests created a 'name' for the rocket group around the Caltech campus. The fiery experiments caught Smith's attention. Struggling financially, without a scholarship, he was an employee of President Franklin Roosevelt's New Youth Administration. NYA jobs were akin to work-study. The student worked for a small stipend, enough to pay his fees and perhaps the rent. The rest of his time was devoted to his studies.

GALCIT had assigned Smith to the laboratory wind tunnel and it was there that he first heard of the GALCIT Rocket Research Group. He was best known as a bright young graduate student with an odd hat, which he wore to ventilate his head while helping to prepare rocket tests at the *Arroyo Seco.* The hat sported a small propeller on top. Smith did not remain with the rocket group very long, however. After mid-summer 1938, he left the rocket research group for an engineering job at Douglas Aircraft in southern California. He returned to work with Malina and the team for a time in 1942 and 1943, but finally moved on for good after then.

The extent of Smith's interest in and commitment to rocketry as a field of study was somewhat limited. In a 1996 interview, Smith demonstrated considerable knowledge of rocket motor dynamics, but was clearly more enthusiastic about discussing aeronautical design. [137] Yet, in January 1938

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afternoons a week." See: JPL Archives, Oral History File, Interview with A. M. O. Smith conducted by Dick House, September 23, 1986.

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von Kármán sent Malina to New York to deliver a paper on the theory of sounding rocket performance—a paper Smith had helped write ('Flight Analysis of a Sounding Rocket.') [138]

Smith's chief task in 1937 lay in discovering a material suitable for exhaust nozzles. [139] Heat resistant materials, of course, are crucial to successful motor performance. An uncooled nozzle will burn through in seconds, if not made of an appropriate metallic or ceramic material. From the winter of 1937 into the spring of 1938, Smith tested several metals and ceramics with high melting temperatures. Molybdenum was among them, and he watched a block of it go up in smoke as he applied an acetylene torch to the metal.

Ceramics and carborundum didn't work very well, either. Finally, Smith arranged a visit to the Vitrefax Corporation in Huntington Park, California, a company which manufactured a substance known as mullite. [140] He watched the manufacturing process and saw how graphite electrodes survived dipping into pots of boiling "super refractories." A material made up of one the most basic of elements—carbon—just might work.

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137 Interview with Apollo Milton Olin Smith, July 24, 1996, San Marino, California.

138 Malina delivered the paper at the annual meeting of the American Astronomical Society, January 1938.

139 'Flight Analysis of a Sounding Rocket' (January 1938) was published in the *Journal of the Aeronautical Sciences, Vol. 5.* (1938), 199-202.

140 This researcher has discovered little on the actual composition of mullite, but it was likely a graphite compound, perhaps with another chemical added and marketed under a brand name.

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Back at Caltech, 'Amo' persuaded the group to try graphite and lined a nozzle with it. The nozzle tolerated the high temperatures of rocket motor combustion, often in excess of 1000° C. Just before he left Caltech in June 1938, Smith tried an acetylene torch on a bar of copper, which also withstood searing heat without a dent. "The torch could not hurt this piece at all and this test opened our eyes to the possibilities of massive copper for resisting heat," Malina wrote years later. [141]

For some reason, neither Malina nor Smith mentioned in their letters and memoirs the development of James Hart Wyld's regeneratively-cooled motor, which was first described in the March 1936 issue of *Astronautics*. [142] Wyld's motor was ready for testing by January 1938. Why didn't the GALCIT Rocket group attempt to build a similar motor? Had they failed to read the article on Wyld? This seems unlikely, since Malina and the rest of the group made it a point to cull much of the available literature on rocketry. At least, they had done so when the Rocket Research Group was first founded. The truth is that the GALCIT crew just did not have the technical expertise to build such a motor in 1938.

Investigating regenerative cooling in liquid propellant motors at Caltech was to wait more than two years—until 1941, when Martin Summerfield began his own experiments with this type of cooling system. After the early—and relatively unsuccessful—tests of a liquid propellant motor at the *Arroyo Seco*,

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141 Malina (1967), pp.123-124.

142 Winter (1980), pp. 83-85.

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the group turned more to experimentation with solid propellants. A 44-second burn of a liquid-fueled motor on January 16, 1937 encouraged Malina to continue building liquid propellant motors. But regenerative cooling would require complex die-casting and machining tools. The GALCIT crew possessed no sophisticated machine shop in which they might fashion such a motor in the late 1930s.

Malina, Parsons, and Forman first tried building new motors both inside and just outside the GALCIT Laboratory building. [143] Two rather dramatic explosions at the laboratory in 1938 and 1939 ended this practice; in fact, the explosions resulted in the group's permanent banishment to the sagebrush of the *Arroyo Seco*. In the meantime, Parsons and Forman went on with their experiments in solid propellant rocketry, with black powder and nitrocellulose as component fuels. Malina delved more into theoretics. Smith was drifting away, attracted by the potential of higher income working for the Douglas Aircraft Corporation. He had received his MS in engineering from Caltech and would remain at Douglas for most of his professional life.

A motley and diverse set of characters was coalescing around rocket-building at Caltech. One of the strangest characters to enter this small world was Weld Arnold (1895-1962), a graduate student in meteorology who volunteered to join the group in 1937. Caltech meteorologist Irving Krick had given modest support to the idea of rocket research there. Arnold, who was then a laboratory assistant in the Astrophysics Laboratory at Caltech, attended a Malina seminar on the GALCIT rocket group's progress in April 1937.

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[143] On the south wall of the building, there was a concrete platform where Malina and Parsons constructed a test motor.

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Arnold was from the East; he had gone to Harvard as an undergraduate. [144] His interest in the GALCIT group piqued by this talk, Weld Arnold approached Malina and offered to give the nearly destitute team $1000, if in return, he could serve as the project's official photographer. Arnold's photographic skills were an unknown to the rocket team, but Malina certainly did not refuse his offer. The group's cash reserve was dangerously low, and the men seriously considered disbanding, in order to concentrate on their own economic survival. At the time Arnold lived in Glendale, California, and bicycled regularly to Caltech from there—a five mile trip. Arnold came to Malina with a bundle wrapped in local newspapers. Inside were $500 in one and five dollar bills, all for the GALCIT rocket group. Arnold never mentioned how he acquired the money, but he was as good as his word and soon ventured an additional $500.

A stroke of good luck had arrived at the rocket group's door, but it was good fortune shrouded in mystery. This was no atypical occurrence. Had Arnold returned home and borrowed the money from his family in the East? Why would he donate a substantial sum to a small group of graduate students, when he himself had little academic interest in rocketry? Perhaps his work in the Astrophysics Laboratory had energized him to think about the potential of space flight. At any rate, his generosity saved the rocket team from more desperate measures at a time when experimental success was short-lived and no support from Caltech was forthcoming.

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144 According to a telephone interview with JPL Archivist, Dr. John Bluth, Weld was a member of the Harvard Club in New York City. Telephone Conversation with Dr. Bluth, July 11, 1996. Arnold attended Harvard from 1914-1917, belonged to Harvard's 1918 class, but actually graduated in 1919, after World War I ended.

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Malina and Parsons were just then concocting the plot of a novel about a group of anti-war rocket scientists which they hoped to sell to a Hollywood screenwriter, in order to revitalize the project. As they were writing in Parsons' kitchen, Malina was continually nervous, thinking about the tetranitromethane in the cabinets a few feet away. [145] Arnold appeared as a peculiar *deus ex machina* and saved the day. Malina promptly deposited the money on Clark Millikan's desk and asked, "How do we open a fund at Caltech for our project?" Millikan, not particularly sanguine about the Rocket Team's chances of success, was shocked by this dramatic gesture. [146]

When Malina left Caltech in 1947 to work for UNESCO in Paris, $300 still remained in this fund. A Caltech accountant requested that the sum be transferred into the Caltech General Fund for accounting purposes, but Malina refused, feeling sentimental about the money. [147] It was an odd stroke of luck that twice saved the rocket team just after it had begun its work. In 1936, a GALCIT seminar had drawn in Parsons and Forman, who appeared with just the right construction skills and experimental inspiration; then, one year later Weld Arnold materialized, saving the group from financial extinction. Strangely enough, Arnold disappeared from the Caltech scene late in 1937, and in fact dropped completely out of sight for several years. He next

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145 Malina (1967), p. 120.

146 Malina (1967), p. 120.

147 Telephone Interview with Dr. John Bluth, July 11, 1996. The $300 was still in the fund because, by 1940, the Army Air Force had begun to fund the GALCIT project, and the original money was no longer needed. The fact that some was left in the fund stands as testimony to the frugality and resourcefulness of the group in obtaining materials.

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surfaced in the late 1950s as a member of the Board of Regents of the University of Nevada. [148] Arnold died in 1961.

In 1937 'Amo' Smith shared his small GALCIT office with Hsue-shen Tsien, an applied mathematician with a facile mind and a penchant for passing mathematics exams with perfect scores. Tsien, born into an intellectual family in Hangzhou, China on December 11, 1911, could trace his descent from the family of the tenth century emperor Qian Liu. [149] Tsien's personality was shy and retiring. Yet he had a reputation for impatience with minds less sharp than his. Perhaps his reticence, especially in fields other than mathematics, gave the impression of arrogance to his fellow students. He enjoyed working through ideas in solitude, had reasonable mechanical aptitude, but felt most at home in the theoretical realm.

Tsien came to the United States in 1935 on a Boxer Indemnity Scholarship. The United States and Chinese governments had established the Boxer scholarships in 1909, following negotiations between President Theodore Roosevelt and the Chinese imperial government. China was due to pay $46 million in indemnities to the United States as compensation for the destruction wreaked on American property in Beijing during the rebellion of 1900-01. Roosevelt believed that amount too high and offered to return half of it to China in the form of scholarships for study in the United States.

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148 Von Kármán (1967), p. 239.

149 Chang (1995), p. 1.

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Tsien won his Boxer Scholarship in October 1934 and sailed for the United States in August 1935. His destination was MIT. [150] Tsien could have remained in China and taken up a position with the Ministry of Transportation, which would have given him a comfortable job designing railway engines. But Tsien wanted to study aeronautics, which he saw as a more intellectually challenging field. However, as Iris Chang notes in her biography of Tsien, *Thread of the Silkworm,* "Tsien and MIT were simply not a match." [151] Tsien was unsuited for the MIT curriculum, which emphasized hands-on design as well as the theory of flight. Tsien had come from an aristocratic family in China; his family had not encouraged him to work with his hands, and so he was not particularly skilled in draftsmanship or laboratory techniques. Nevertheless, he tried his best.

Tsien teamed up with another young Master's Degree student, W. H. Peters, in an attempt to study the phenomenon of turbulence in the boundary layer next to an airfoil. Ludwig Prandtl had surmised that air separated into discrete layers above an airfoil surface and that the layer closest to the airfoil formed a unique boundary between the smoother flow of air rushing above (or below) the airfoil surface and the surface itself. This layer was susceptible to turbulence, especially if the design of the airfoil did not promote smooth, unobstructed air flow.

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150 Chang (1995), pp. 37-39.

151 Chang (1995), p. 41.

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Tsien and Peters wanted to study the nature of this boundary layer turbulence, using MIT's wind tunnel. However, the tunnel was balky. Small, constructed of plywood, and powered by a noisy fan motor, the tunnel actually promoted turbulent air flow of its own, rather than a smooth current of air. It became impossible to study the phenomenon of turbulence created around the airfoil (a wing or fuselage) itself because the design of the experimental apparatus itself interfered. Tsien actually got into the tunnel several times, trying to improvise additions and changes to the tunnel that would smooth the air current, but to no avail. Tsien finally had to conclude that he would get no "meaningful results" from the tunnel, and his thesis summation remained inconclusive. [152]

Beyond the frustration of not successfully completing the experimental portion of his Master's thesis, Tsien may have come to dislike the isolation he experienced at MIT, even though he loved intense, quiet study himself. At some point in 1935, probably after the disappointing results of his wind tunnel tests had become all too clear, Tsien went to discuss his problems in the Aeronautical Engineering program with its director, Jerome Hunsaker. He rankled Hunsaker with his complaints, for Hunsaker told him: "Look, if you don't like it here, you better go back to China."[153]

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152 Chang (1995), pp. 43-44

153 Chang (1995), p. 44. The story of Tsien's meeting with Hunsaker is related by an old friend of Tsien's, Andrew Fejer, who allegedly heard it directly from Tsien. Chang does not insist on the accuracy of the story, however. It appears that a number of events convinced Tsien that he could not fit in at MIT, and that his department may have been trying to ease him out.

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This, however, was not an option for Tsien. Returning to China before the completion of his graduate studies would have been a disgrace, both for the young graduate student and his family. He began to search for another graduate school in the United States, one better suited to his interests and temperament. He found Caltech and von Kármán. Rather than write the director of the Guggenheim Aeronautical Lab at the California Institute, Tsien decided to pack his bags, take the train to Pasadena, and speak with von Kármán himself. It was late summer or early fall 1936, Tsien had completed a year at MIT, and the Rocket Research Group at GALCIT had just been born.

Tsien, with his Master's Degree from MIT, went to Caltech and appeared at von Kármán's door. The elder man relates the first encounter:

> One day in 1936 [Tsien] came to see me for advice on further graduate studies. This was our first meeting. I looked up to observe a slight short young man, with a serious look, who answered my questions with unusual precision. I was immediately impressed with the keenness and quickness of mind, and I suggested that he enroll at Caltech for advanced study. [154]

As was the case with Frank Malina, Tsien found himself quickly absorbed into the GALCIT program. No long formal process of application was necessary for Tsien prior to admission. Von Kármán had the authority to sweep any promising student into the program whom he wished and he rarely missed an opportunity to exercise his astute judgement about intellectual capacity. Tsien accepted immediately and was in. There was no need to return to China.

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154 Von Kármán (1967), p. 309.

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Von Kármán had taken the measure of his new student with remarkable clarity. Watching Tsien's work in applied mathematics, GALCIT's director summarized the aristocratic young man's abilities succinctly: "I found him to be quite imaginative, with a mathematical aptitude which he combined successfully with a great ability to visualize accurately the physical picture of natural phenomena." [155] It was undoubtedly Tsien's sharp mind and his ability to *visualize* phenomena which put him most in tune with von Kármán. Von Kármán himself liked to see the essence of a phenomenon in his mind's eye before he worried about the mathematical details describing it. John Parsons also shared this knack for imaginative visualization.

Tsien and von Kármán worked out the ideas they were creating in their minds somewhat differently. Von Kármán often worked on two mathematical ideas simultaneously. He could leave one behind after initially sketching it, go on to another complicated mathematical string, then abruptly return to the first one and complete it at the end of a classroom lecture. Von Kármán's mind evidently worked on several deep levels—he could follow one idea all the way to its conclusion while devoting his mental energy to an utterly different problem. [156] According to Chang, Tsien labored in another vein. Tsien would sit down at his desk and write out an entire string of mathematical ideas in one sitting, and then present them to his class in completed form. But he needed quiet and concentration to do so. He was

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155 Von Kármán (1967), p. 309.

156 'Amo' Smith described von Kármán's mental feats in class during his 1996 interview with this researcher.

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apparently an expert at erecting the detailed structures of mathematical events, sometimes laid out only in sketchy form by von Kármán. [157]

Tsien tended to work alone, sometimes all through the night. He was a systematic and methodical thinker. Tsien was known to turn in chapters to books exactly on time, while co-authors delayed their own contributions for months or years. Clearly, he was quite tenacious. His work revealed a beauty and tight logic which could only come from having worked out every detail of every stage of a given problem. Nothing was left unexplained. He was a mathematical perfectionist. Tsien often looked upon von Kármán as his intellectual father and undoubtedly revered the thinking style and teachings of the master Hungarian aerodynamicist. But Tsien's career in mathematics started long before he met von Kármán at Caltech in 1936.

At Jiaoting University in Shanghai, where Tsien enrolled in 1929, he studied "fundamental physics, chemistry, and mathematics." [158]

> As the year progressed, Tsien's peers began to note the grades of this small reclusive boy from Beijing....At the same time they remembered the boy himself as an odd sort, even by Jiaoting standards. He did not talk much, nor did he study with others. The collegiality of the lecture hall and laboratory held few charms for him, they remember. Tsien, it seemed, wanted only solitude and silence—a blissful atmosphere in which to think and do his own work. He seemed happiest when alone. [159]

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157 Chang (1995), pp. 64-66.

158 Chang (1995), p. 23.

159 Chang (1995), p. 24.

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Tsien later developed a reputation as a cold, introverted, arrogant professor, even more at MIT than at Caltech. He was known to terrorize and dazzle students at both institutions: terrorize them with his fiercely difficult exams, where the high grade might be 22 out of 100, and dazzle them with his mathematical erudition. Sometimes (in the 1940s) he appeared in class at Caltech without any lecture notes at all. Then, during a class break, "Tsien would rush up to his office on the third floor [of the GALCIT building] and slam the door, so he could look at his notes for the second hour and come back down and lecture again for an hour!" So remarked a former student, Holt Ashley, rather amazed at the performance. [160]

In the spring of 1937 Tsien, iconoclastic and introverted, fell in with the Rocket Team. Here was a group of young engineers and amateur rocket builders, some of whom were far more demonstrative than he, some, in fact, quite unreserved and impulsive. Tsien met Frank Malina through 'Amo' Smith, Tsien's office mate. Since Smith had not joined the team out of any burning desire to build rockets, his presence at GALCIT was, at least initially, a matter of chance. [161] Coincidence thus brought Tsien and Malina together. Malina, having committed himself to a Ph.D. on rocketry, approached the problems of rocket dynamics and design with a burning desire to work on engineering problems that were on the cusp of current research.

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[160] Chang (1995), p. 127.

[161] This being the case, Smith 's contribution to the early theory of rocketry with his 1937 paper, 'Flight Analysis of the Sounding Rocket,' is even more remarkable.

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Tsien, too, seemed enthusiastic about the prospects of doing *mathematical* research on rocket dynamics. This was a new, unexplored field and he could truly make a contribution to it. Tsien did not lack ambition. He was willing, as had been shown by his move from MIT to Caltech in 1936, to put down new roots wherever opportunity led him. Yet, in discovering rocket dynamics, Tsien inadvertently stumbled onto a field to which he would devote much of his life's work, and in which he would make significant and original contributions. In his own way, Tsien was to become an intellectual gold mine for the rocket research team.

All in all, the GALCIT Rocket Research Team, born in 1936 of a very diverse group of young scientists, was to share several characteristics: intellectual curiosity of a high order; a solid knowledge of chemistry, particularly that of pyrotechnic compounds; and, with the exception of Parsons and Forman, a particularly high level of mathematical expertise.

Most significantly, the small knot of men who came together at Caltech quite fortuitously in two short years (1936-1938) shared a willingness to take risks that was positively uncanny. These men were not unaware or unafraid of the dangers they faced. They knew that physical injury or death might result from their experimentation. But their desire to explore the theoretical and experimental unknown loomed larger than their fears. They were ostracized, ridiculed, and later banished from the Caltech campus following some disastrous explosions of their experimental apparatus. This did not stop them.

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What was the essential glue that bound the GALCIT Rocket Research Team together? Certainly, the presence and tacit support of GALCIT's director, Theodore von Kármán, provided the base of support around which the team's efforts could coalesce. Von Kármán was a man of extraordinary talent and a peculiarly light-hearted personality. He was freed from many of the daily cares of life by the attentions of a doting sister and mother. He had no wife or children. Yet, he adopted a fatherly tone with at least two of the members of the Rocket Team, Frank Malina and Hsueh-shen Tsien.

These two young men revered and even adored him, intellectually and personally. Frank Malina had living parents in Brenham,Texas, both musicians and intellectuals. Tsien's father, emotionally removed but deeply concerned about the scholarly well-being of his son, still lived in Shanghai. Perhaps in part because the young scholars were far from home, they became increasingly attached to the elderly Hungarian scientist, himself so full of ebullience and insight. Risk taking became a natural endeavor to them.

The imprint left by von Kármán's *persona* on Tsien, who called GALCIT's director his 'revered teacher' was especially profound. Tsien seemed at times to hang on von Kármán's every word and certainly was assiduous in copying every formula von Kármán wrote on the classroom blackboard. [162] Malina's relationship with von Kármán was equally complex, but from a different perspective. Malina did not come from a culture in which educators were viewed as demi-gods. Malina relied on von Kármán for an emotional sustenance whose roots lay in collaboration with a well-known scientist. Von

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[162] Chang (1995), p. 64.

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Kármán could be a very affectionate, if a bit absent-minded father figure. As long as his students accepted his rules of the game, the doors to his inner intellectual sanctum were open.

Yet the close attachments von Kármán formed with Tsien and Malina did not extend to Forman and Parsons, both of whom failed to fit the mold of the bright, insightful young graduate student. Parsons and Forman dealt with von Kármán as their administrator, rather than their intellectual guide.

Martin Summerfield, who joined the GALCIT Rocket Team in 1940 after earning his Ph.D. in physics, was, according to Liljan Darcourt Wunderman, awkward: he frequently seemed to feel out of place among others, at least socially. [163] This in no way prevented his making some seminal discoveries in his nearly ten years at Caltech. After hunting through the Institute's library, sometime in 1940, Summerfield discovered in an old chemistry text that hydrocarbons can be made to burn completely and efficiently in a small combustion chamber, thus inaugurating the effort to build a higher thrust liquid propellant motor for the JATO.

Summerfield could also take credit for recognizing that instantaneous ignition of a liquid propellant (that is, reducing the lag between ignition of the propellant and its actual combustion to the bare minimum) would enhance stability of propellant burn. [164] Summerfield formed an intense attachment to his work and proved loyal to the Rocket Team that helped

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[163] Interview with Mrs. Liljan Wunderman, April 1996.

[164] Koppes (1982), pp. 13-14.

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bring about these groundbreaking results. He does not seem to have formed a deep attachment to von Kármán in the process.

Apollo Milton Olin Smith saw the Rocket Team's work as a friendly intellectual challenge in a new area of aeronautical engineering. He greatly admired von Kármán, but felt his teaching method was rather disorganized. In any case, following the publication of his joint paper with Malina in 1938, Smith gravitated toward his true life-work in the design of aircraft, holding a position with Douglas until 1975. He lost immediate touch with the Rocket Team after 1938, and then re-appeared, as a consultant to the Aerojet Company, for eighteen months in October 1942, on leave from Douglas.

Weld Arnold's loyalty to the Rocket Team probably had its source in his desire to play a peripheral role as photographer for the group. He was neither a mechanical nor an aeronautical engineer, and his disappearance from the scene in 1937 left him in a curious position: he descended upon the group, providing sustenance in the form of five and ten dollar bills. Then he returned to the East Coast, leaving virtually no trace behind, as far as the group was concerned.

Clark Millikan, on the other hand, whom 'Amo' Smith admired much more for his teaching skills than he did von Kármán, was to play an important role in the development of the team. Millikan initially saw the Rocket Team's struggles as irrelevant to the career goals of a young engineer, but he became more involved as the experiments with rocket motors developed in earnest and the group began churning out a series of sound theoretical papers. While not quite attaining respectability in Millikan's view, the group had at least

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achieved a modicum of academic success. Clark Millikan's relationship to Frank Malina turned from that of skeptic to co-worker, and finally, to antagonist.

Millikan disliked Malina's progressive political inclinations, and as the son of Caltech's Chief Executive Officer and a GALCIT professor in his own right, he embraced the role of conservative foil to Malina's more *avant garde* political style. The friction between Malina and Clark Millikan rarely abated and even broke out, once JPL had been founded, into outright hostility. There was a certain degree of impatience and perhaps even jealousy on both sides. After all, Millikan viewed even Malina's chief, von Kármán, with respect, rather than reverence. His own guide was his father, Robert Millikan, a Nobel laureate.

Cohesion among the Rocket Team's members ebbed and flowed—it cannot, in retrospect, be reduced to one particular quality. Loyalty to von Kármán certainly was key; but the intellectual stimulus of research and experimentation in an uncharted field played upon the men like a magnet. Collaboration, rather than isolated intellectual work, was a hallmark of their efforts (Tsien's insular personality notwithstanding.) Ambition also had its influence on the effort, for the GALCIT graduate students scientists craved recognition from their peers for their intellectual efforts, as do most scientists. A scientist without a record of experimentation or publication is one whose career is short-lived.

And yet, the most perplexing aspect of the Team's work together was the men's utter disregard for the impact their work had on the very community

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of scientists with whom, of necessity, they lived. They wistfully bore the appellation of the *Suicide Squad*, following the series of explosions that rocked the south side of the GALCIT building. Their experiments annoyed other Caltech scientists, who rarely took them seriously. But it was not only the Caltech community's reaction to the Rocket Team that solidified their rapport. Rather, their dedication and professionalism, the curious blend of their personalities, and their desire to expand aeronautical knowledge welded them together—even if, at times, this bond seemed stretched to the breaking point. In fact, the Rocket Team's peculiar personal chemistry soon began to play a decisive role in its accomplishments.

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Chapter 5

The Social and Political Milieu: How Domestic and International Crises Shaped the Direction of GALCIT Rocket Research in the Nineteen Thirties and Forties

Today the Caltech campus exudes a particularly quiet and insular tone. Surrounded by residential avenues with palm-graced lawns, it is home to fewer than a thousand students, and its lush quadrangles and pathways are bordered by semi-tropical plants. The school seems far-removed from Los Angeles to its southwest, and even separate from the surrounding community of Pasadena. It is quite possible for a student to pass many semesters on the verdant campus, losing track of the rhythm of the outside world. Such must have been even more the case in the 1930s, when autos were more sparse and traffic moved more slowly. To the north and slightly west of Pasadena sits the *Arroyo Seco,* the present home of the Jet Propulsion Laboratory. Today, JPL sprawls across the upper edge of the *Arroyo,* but in 1936, the canyon was nothing more than a bleak, barren, sage rolling gulch with one road circumscribing it.

It would be hard to conceive of an academic haven better suited to intense exploration of the physical sciences than the environs of Caltech. And so it is not surprising that even in the mid-1930s, with the world of Asia and Europe percolating like a too-watched pot filled to the brim with tragedy, that Caltech engineering students could devote themselves cleanly to science. Yet the Pasadena campus was not immune to the repercussions of the outside crisis. GALCIT's students were not so one-dimensional that they excluded political passions from their repertoire of interests. (Of course, there were

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undoubtedly still some young intellects on campus whose sole interest was formulae, the principles of physics, and wind tunnels.)

For Frank Malina, Hsueh-shen Tsien, Martin Summerfield, and in his own inimitable way, Jack Parsons, life was a balancing act, swaying between professional scientific endeavor and the pleasures (or concerns) of the day. The intensity of their collective devotion to the tasks they had assumed remained the guidepost of their active lives. Tsien, in particular, had the energy and tenacity to study, attend class nearly all day, and after a brief respite for dinner, return to his small room and labor over the details of a problem—usually sketched out by von Kármán in class or in the study of the director's home on Marengo Avenue. Malina, too, was frequently absorbed by the theoretical problems with which he struggled. His young wife, Liljan Darcourt, rarely was privy to the details of his work. The *Suicide Squad's* days revolved around calculations at the laboratory, or attempts to design and assemble experimental rocket motors at GALCIT or in the *Arroyo Seco*.

Nevertheless, military and political upheavals in distant regions of the globe reverberated even at Caltech. Tsien was a Chinese national, a student with a special, conditional dispensation to study in the United States. The Chinese Nationalist government knew of his efforts at Caltech and of his talent. He could be called back to serve his country at any time. As the Japanese incursions into China grew more probing and threatening, Tsien himself wondered whether or not to return to Shanghai and serve his homeland by working with the tiny Chinese Air Force. Perhaps he could be of greater help there. Malina, coming from an intellectual European family ensconsed in a small southeast Texas town, was cosmopolitan in his interests. He was

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developing into a young engineer with political interests that tended toward the support of democratic forces struggling against dictatorship. Malina identified strongly with the efforts of the Spanish Republic to maintain itself against Franco's onslaughts. At the very least, he was growing increasingly concerned over the drift toward the defeat of democracy in Europe. Parsons, whose political definition was by no means clear or complete in the mid-1930s, lived out a belief in the renunciation of the influence of all authority over his life. [165]

Most certainly there was much to preoccupy all three men, should they begin to follow in earnest events across the Pacific and Atlantic. Even closer to home, the Depression touched their lives. Malina earned eighty cents a day in 1935 working in GALCIT's wind tunnel. Tsien was dependent upon his Boxer Indemnity Scholarship for his survival in the United States. Fellow students at the Guggenheim Aeronautical Laboratory, looking for work with nearby aircraft manufacturers, were met by rejection after rejection. 'Amo' Smith, dependent, as were many Americans, on the New Deal for his economic survival, saw little to look forward to outside of government-sponsored work. Professionally, Caltech remained a haven during the Depression. The struggles of other working people in southern California did not touch their lives in an immediate way, but neither were GALCIT students so inured to the grind of their intellectual work that they could ignore the sacrifices of millions, near and far.

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165 According to Wilson's June 8, 1973 interview with Malina, Parsons had relatively little interest in politics in the 1930s. "Parsons came into...worries about the social questions more...from the poetic side." Malina Interview (1973), p. 32.

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Their interest, however, was largely intellectual—these men were not wholly devoted to a particular political passion. In mid-summer 1936, while Malina was preparing the theoretical groundwork for the first GALCIT rocket motor tests (to come in the fall and early winter of that year), the Spanish revolution took a turn sharply to the right, with the fascist invasion of southern Spain and consequent army uprisings all over that peninsular nation. Malina and Tsien regularly met at their homes to listen to music. Sometimes they met at von Kármán's house for the same purpose. Von Kármán was a European expatriate and was very concerned about rising Nazi power, having experienced anti-Semitism first-hand in his days as a graduate student at Göttingen, and again at Aachen.

According to Malina, "We'd get into arguments. We would get together at night. Sometimes we'd be arguing about rockets and the next thing you'd know we'd be arguing about what's happening in Spain, et cetera. We were a very peculiar, closely knit group with very many cross interests which threw us together, in which the discussions could swing from music to rocketry to political questions to social organizations and to all these kind of things." [166] Malina was an anti-fascist and ardently sympathized with the Spanish Republic. Since no transcripts of the discussions, which often ran late into the night, exist, there is no indication of who might have opposed Malina at these collegial meetings, or what the specific content of their 'arguments' was.

Malina had also taken a course in philosophy at Caltech which had greatly impressed upon him the need to integrate his mathematical studies with the

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166 Wilson Interview with Malina (1973), pp. 33-34.

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history and philosophy of science. He had studied logic, which "made a tremendous impression on [him]." Malina added that Tsien and even Parsons had devoted a certain amount of effort to such studies .[167] However, according to Iris Chang, Tsien was more absorbed by Classical and Romantic music, his second passion, than by politics. During soirées at the von Kármán home, he would sit in a corner, listening to Beethoven recordings. Tsien was a highly private person and it is unlikely that he shared his personal opinions about politics with many others.

There have been intimations that Malina and Tsien were, at the least, subtly persuaded to participate in a Communist Party cell organized at Caltech in the late 1930s.[168] But Malina never made important decisions without due deliberation. Liljan Darcourt Wunderman contended in a 1997 interview that Malina joined the US Communist Party sometime after 1939 and may have remained in it at least through 1941. There was a small cell in Pasadena, Unit 122, led by another Caltech graduate student named Sidney Weinbaum, and Malina and she attended the cell's meetings. [169] Wunderman stressed Malina's loyalty to the United States, and was firm in her belief that his membership in the Communist Party was a sincere intellectual attempt to grapple with the problems of the Depression. She emphasized repeatedly that she and Malina only went to meetings in Pasadena, never to Los Angeles, where the Party had larger organizational units. Wunderman believed that Malina's short tenure in the Communist Party presented him with the

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[167] Wilson Interview with Malina (1973), pp. 33-34.

[168] See: Chang (1995), Chapters 16-19, pp. 149-172.

[169] Interview with Liljan Darcourt Wunderman, New York City, October 16, 1997.

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opportunity to participate in highly theoretical discussions, as if he were in a chess game. He wanted to solve the formidable problems the United States and the world faced in the late 1930s, but took no steps to organize opposition to the government. After the mid-1940s, Malina became disillusioned with the communists and drifted away from the Party.

Malina's flirtation with communism as a young man cost him dearly some years later. The FBI persistently harassed Malina in the mid-1940s for suspected affiliation. Chang (1995) has dealt extensively with Tsien's brush with the same (Unit 122) Communist Party cell. It is not likely the young Chinese scholar ever joined the Party—he was too reclusive, knew the history of his own government's battle with the communists, and would not have done anything to jeopardize his student status in the United States. Malina and Tsien were open, as were many other young intellectuals, to the political and philosophical ferment prevalent in the 1930s.

Disgusted by an increasingly acrimonious relationship with Clark Millikan, troubled by his own involvement in military-sponsored research, and scrutinized by the FBI, Malina left in 1947 for Europe and a position at the United Nations Educational and Scientific Organization. [170] In an oral interview, Liljan Wunderman described how in 1945, FBI agents actually broke into the tiny Malina home in Pasadena and partially ransacked it. [171]

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[170] In 1945, Malina was the first Acting Director of the Jet Propulsion Laboratory. Clark Millikan was Caltech's liaison officer at JPL. Friction between the two men heightened as a consequence of their ill-defined professional relationship.

[171] Interview with Mrs. Liljan Darcourt Wunderman, April 22, 1996. Apparently, the agents were looking for suspicious literature. Mrs. Wunderman alleged in her second interview with this researcher that another member of the Caltech community alerted her and Malina to the impending FBI raid, telling her that the Bureau had already called at the homes of others in the Pasadena area. She drove around Pasadena the entire afternoon of the raid with papers

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In 1936, all this was in the future, and these young men were probably entirely unconscious of the danger their late-night political perambulations were to bring. The world was in a very precarious position in 1936; Malina, who was Czech in origin, and Tsien, quietly agonizing over his relatively secure life in California while Japanese troops slowly swallowed up Chinese territory, could not help but be politically aware. The political thread wove a persistent pattern through the lives of several of the Rocket Team, sometimes altering their existence dramatically. Yet, any assertion that politics dominated their lives in the 1930s would be an overstatement. The preoccupation of these young engineers and dreamers was rocketry, not politics. Intellectuals tend to be complex, multifaceted individuals. To sum up their interests by categorizing them according to one political stripe or another would be inaccurate and a distortion of their story.

The impact of Depression era political events on the work of the Rocket Team, and on rocketry as a science cannot, however, be denied. On two continents, the military was to play a decisive role in its development. Yet, America's commitment to rocket research in the mid-1930s could not match the Nazi government's investment of resources in its liquid-fuel missile program. The American military and the US Congress could not resist the influence of Isolationists within their midst and refused to gear up for war. The result was that the army and navy's capabilities, prior to 1939, remained at peacetime levels. If Franklin Roosevelt were having a difficult time convincing some members of Congress that war with Germany was just

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from their attic on the front seat of her car, eventually stopping at a friend's house to burn them. Coincidentally, while Malina was wrestling with his conscience over his involvement in war-related missile research, etc. in 1945, his marriage Liljan Darcourt was breaking up.

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around the corner, only a miracle could convince elected representatives—much less scientists at many American universities—to devote money to rocket research.

In 1936, three years before the start of war in Europe, the United States preoccupied itself with domestic political imbroglios. The Supreme Court had just invalidated several of the Roosevelt Administration's innovative remedies, especially the National Recovery Act. The Congress passed two Neutrality Acts, in 1935 and 1937, in response to the Italian invasion of Ethiopia and the outbreak of civil war in Spain. Military expenditures were low ($932 million in 1936), although in Depression dollars, this actually proved something of a boon to aeronautics research; NACA (the National Advisory Committee for Aeronautics) built new wind tunnels with funds supplied by the WPA and actually increased its staff from a low of 181 in 1930 to over 520 in 1939. [172]

Although von Kármán himself was conducting basic research into the behavior of projectiles at supersonic speeds in the 1920s and mid-1930s, no government initiative to explore the potential of rocket research came before 1938. In May of that year, General Henry H. (Hap) Arnold (1886-1950), a farsighted advocate of air power, came unexpectedly to Caltech to speak with von Kármán about the possibility of rocket boosters for Army Air Corps bombers. Arnold had the perspicacity to realize that Caltech and its unorthodox director of aeronautics were on the cusp of flight research in the

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[172] Anderson, Frank W. Jr. *Orders of Magnitude: a History of NACA and NASA, 1915-1980.* Washington, the NASA History Series, 1981. pp. 4-5. The source of the figure for US military expenditure (in current dollars) for 1936 is Paul Kennedy's *The Rise and Fall of the Great Powers.* New York, Random House, 1987, p. 296. For the same year, Nazi Germany spent $ 2,332 million dollars on armaments.

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United States. Arnold was also conveniently stationed at March Air Base, near Pasadena, in 1938.

While enlisting von Kármán's collaboration in a military research project, Arnold rattled a hornet's nest of controversy with the civilian director of NACA, George Lewis, who decried the budding relationship between academia and the military. According to von Kármán, Lewis bluntly informed him that NACA expected Caltech to serve as a feeder school for NACA engineers. Lewis "opposed military sponsorship of research altogether," thus exposing a conflict that was to simmer among GALCIT Rocket Team engineers, and within the larger aeronautical community, in the coming decades. [173] Actually, Lewis wanted exclusive access to the military as the developer of Armed Services wind tunnels, etc. In 1915 Congress created NACA by attaching a rider to a navy appropriations bill, and ever since the agency's link to the military had remained strong. Lewis probably believed that von Kármán was doing an end-run around him.

In reality, von Kármán was only heeding the call of an old siren within him: in the First World War, he had been in the officer corps of the dying Austro-Hungarian Empire—in the artillery corps, no less—and he had loved the pomp and prestige of military life. Questions of the propriety of relying on military funding for civilian academic research did not seem to trouble him as much in 1938 as they were to in the late 1940s, when the Jet Propulsion Laboratory was defining its relationship with Caltech and the extent of its involvement in Air Force projects. [174] Thus, it was perhaps natural that an

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[173] Von Kármán (1967), pp. 226, 243-244.

[174] For a detailed and insightful discussion of the military-academic research dilemma see:

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early team effort at rocket construction sought out the military, and was—if not immediately successful—at least propelled forward by this association.

National Academy of Science and then Army Air Corps funds resuscitated the GALCIT Rocket Research Project just when it seemed about to fade away, and throughout the 1940s, the military and Caltech were to play out an elaborate dance, tugging at each other's skirts for control of the organization and direction of experimentation in rocketry. [175] As the import of rocket development dawned on those few in government with their eyes open to the future, an alliance formed that was to outlast its originators.

Yet, even when army money began to funnel in, the GALCIT Team did not forget its scruples and plunge full-speed ahead into military rocket research. There is little question that the Caltech rocket team was not likely to turn down a source of munificent funding—this, especially after struggling through two early years of penury. But, military funding evoked ambivalent reactions among several of the Rocket Team's members. They were troubled by the realization that the military would quickly and inevitably turn their dream of building a sounding rocket into some other practical military project. However, in 1938, war lay ahead and at least some of the team's members felt an urgent need to take part in the struggle. It is probably accurate to say that the group was grateful for the chance to continue its

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Koppes (1982), pp. 18-29.

[175] The last and largest of the NAS Air Corps Research grants to GALCIT began on July 1,1939, and was labeled NAS Contract W535-ac-15690. The grant lasted through 1940, and was actually dated September 26, 1940. The grant amount was $10,000. R. C. Miles, 'The History of the ORDCIT Project up to 30 June 1946.' Research and Development Service Sub-Office (Rocket), California Institute of technology, Pasadena, California, 1946. JPL Archives, Document # HC 3 326, Part 1 of 2, p. 3.

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research; its members rationalized the difficult aspects of receiving money from the military and sincerely believed they were contributing to the struggle against fascism.

Frank Malina identified strongly with the plight of his relatives in Central Europe. The 1938 Munich Accord between Germany, Italy and the western democracies must have greatly shocked him. In 1938, the Army Air Corps began to offer research funds to the GALCIT Rocket Project. Under the circumstances, whatever qualms Malina had about accepting military money were quelled by the realization that the US military would likely be in conflict with Germany within a few years. The real issue lay not in the immediate result—the production of small rocket units to help boost aircraft into the air more quickly—but in the institutional bonds created by a link to the armed forces. The army, sooner or later, would begin to intrude organizationally and administratively into the team's work. This was the underlying problem. Nor was there any way to avoid it, once the military laid claim to funding the research. Priorities would be established by military necessity; secrecy would be imposed on the work; the regulations governing contracts with the armed forces would take effect.

In reality, the question of GALCIT—and Caltech—independence from the military would rear its head by the mid-1940s. In order to facilitate the growth of JPL, the army exerted pressure on the Pasadena City Council to renew its lease on the land in the *Arroyo Seco* to the burgeoning research center. The city had complained of the noise and disruption of rocket tests as incompatible with the residential area. The army responded by threatening to

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condemn the area of the *Arroyo* and the City Council capitulated, signing a new twenty-five year lease in 1945. [176]

The problem was not so much a moral dilemma in the late 1930s and early 1940s as it was an incipient conflict between a powerful arm of government and a small nucleus of applied research. Forms, regulations, oversight inspections, increasing control over the direction of research: these were real issues. Behind them lay a particular ethos, with which one might agree or disagree—that of working on behalf of the security of a large national state. Frank Malina slowly became uncomfortable with military research in the 1940s. In the 1930s, however, the danger appeared to come, not from his own massive modern state, but from the existence of machine-like totalitarian states which mobilized resources and molded opinion, usually through coercion, to their own distinct ends. The organization of research to serve the interests of a well-organized state, with its own intrinsic and insatiable demands for security and power, was not of concern to most American scientists in the 1930s.

The situation was far different across the Atlantic. Since autumn 1929, German Army Ordnance had shown interest in developing a new type of surprise weapon, one that could replace conventional artillery. This interest, of course, had to remain secret, as the Versailles Treaty forbade the weak

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176 Miles (1946), pp. 17-18. See also: Koppes (1982), p. 21. In 1943, Pasadena's city manager, C. W. Koiner, had protested renewing the army's lease on the land in the *Arroyo Seco*, saying that the city had an obligation "to the people who built their homes in the vicinity away from the din and the noise of city life." Koiner had promised Pasadena residents that, at the war's end, the Jet Propulsion Laboratory plant would be "removed from the city's property." Instead, by 1945, two years after the dispute began, a new lease for 25 years was signed by Pasadena, after the army threatened to begin property condemnation proceedings against the city. The end result of this legal action would undoubtedly have been confiscation of the *Arroyo Seco* property by the military.

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German republic to develop its armed forces. The very restrictions of the peace accord spurred some in the military to circumvent it. Lieutenant Colonel Karl Emil Becker (1879-1940) of Army Ordnance, a Ph.D. in engineering with a particular eye on solid-fuel rockets capable of carrying poison gas to enemy lines, received permission from the Weimar Republic's Defense Minister in late 1929 to investigate such rockets on his own. It so happened that Becker was also an ultra-nationalist who yearned for the day when a new right-wing authoritarian regime could overthrow the treaty's onerous limits on German military power."[177] Becker had participated in the development of Big Bertha (the Paris gun), an 8.25 inch howitzer with a range of 80 miles. He realized the technical limitations of an artillery piece this large and began dreaming of a ballistic system that would eliminate the need for stationary barrel-type guns. A missile would just suffice.

Working under Becker in Ordnance was a young veteran of the fighting on the Western Front, Walter Dornberger (1895-1980.) Dornberger, who had read Hermann Oberth's *Wege zur Raumschiffahrt* (*Ways to Space Flight*) in 1929 and who was much taken by the concept of iquid-fuel rocketry, was destined to play an essential role in the development of the GermanV-2 missile. Eventually, Dornberger became the head of the German Army's Rocket Program and the director of the experimental missile works at Peenemünde on the Baltic Coast (after March 1936.)

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177 Michael J Neufeld. *The Rocket and the Reich: Peenemünde and the Coming of the Ballistic Missile Era*. New York, Free Press, 1995, p. 6.

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German Army Ordnance's promotion of men preoccupied with developing a military rocket program proved a solemn harbinger. Once the Nazis had wrested power from the moribund Weimar Republic, the new totalitarian government could move to choke off, not merely dissent, but amateur and potentially suspicious science. Army Ordnance had, of course, known about the *VfR* (*Verein für Raumschiffarht*). The most flamboyant proponent of the Raketenflugplatz Berlin, Rudolph Nebel, (a former World War I fighter pilot) tried several times to insinuate himself in the early 1930s with Ernest Röhm, head of the powerful SA, the Nazi Stormtroopers. [178] He rarely succeeded, but Nebel proved a tireless promoter of the *Raketenflugplatz*. Nebel gambled that close affiliation with the SA would result in government support for the *VfR*. He was wrong. After the June 1934 murder of Röhm and other key figures in the SA, carried out by SS execution squads backed by army logistic support, the death knell of amateur rocketry in Germany sounded.

The *VfR* had not counted on complete government suppression of its independence. In fact, however, Army Ordnance tried to eliminate the amateur organizations, even those allied to prominent businessmen, as early as 1933. The army itself, influenced by men such as Becker and Dornberger, wanted to control rocket development. As Neufeld wrote in *The Rocket and the Reich*, the SA purge had effectively given the army a rocket monopoly." [179]

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[178] Neufeld (1995), pp. 11-16.

[179] Neufeld (1995), p. 31.

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The contrast between the fate of German rocket experimenters in the 1930s and that of American researchers at Caltech could not be more laden with irony: while a technical institute in southern California would not even give financial backing to rocket investigation in the mid-1930s, the German Army had already committed itself to full control over rocket testing by 1934. The issue of state control over experimental design and research was not even to become important in the United States for fully eight to ten years after the German Army swept away amateur rocket groups. While Caltech engineers and experimenters struggled to build a rocket motor under three feet in length, the German Army began production of the world's first ballistic missile, capable of reaching a target nearly 200 miles from its launch site.

What is even more troubling is the insight that there was relatively little if any dissent from the German rocketeers over the ultimate use of their creations. Dornberger and Wernher von Braun, both of whom led the effort at Peenemünde on the Baltic coast, were wholehearted supporters of the Nazi regime. Von Braun was technically an SS officer. Despite frequent friction between the Army High Command and the Nazi Party over the slow pace of development of the A-4 (known in the West as the V-2 or Vengeance-2 missile), there was little thought given at Peenemünde to the ethics of state control over science. The state demanded performance. Scientists had to produce tangible results. Similarly, few scientists worried about the morality of targeting civilian populations with the new weapon. Nor did Dornberger or von Braun lose much sleep over the employment of slave labor in the construction of production models of the A-4.

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In the 1930s, the nation-state seemed to manifest itself as a living organism, fusing the individual to larger aims of an apparatus far more compelling than any personal destiny. Whatever hopes of academic independence might be lurking in the minds of German scientists were squelched by the demands of the state. National security was to become an overriding concern in the funding and design of scientific research in the United States, too, once the Second World War was well underway. [180] In 1942, few in the American scientific community thought of building missiles that could destroy civilian populations. The ultimate fruit of the labors of GALCIT research—the solid propellant submarine-based IRBM—was then two decades away.

Yet German Army Ordnance control over rocketry in the Third Reich raises ethical questions which cannot be easily dismissed as the quirky demands of a totalitarian state. There still remains the question of where the individual scientist draws the line between his own hopes and his duty. The national state fashions scientific vision to its own ends. Some members of a scientific community adopt the ethos of destruction and see a positive good in creating weapons of mass destruction. In such cases, motivations differ. Scientists, like anyone else, can squelch the rumblings of their conscience and devote themselves completely to the rigors of their work. It is sometimes easy to subordinate troubling ethical issues to the successful completion of an experiment.

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[180] One might also argue that the War Industries Board of the World War I era served as the prototype for the shaping of engineering research to national security aims.

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In even more extreme cases, the will to destruction fuses with the creative impulse and produces scientists who take pride in building weapons. Somewhere in the gray area between the perverse and the merely mundane lies the individual who consciously works on experiments destined to bring about destruction, but who denies—even professes to reject—his role in the larger aims of the state. Science and government are difficult to divorce in an era when the state apparatus is powerful. This is one of the legacies of Peenemünde.

The issue for the individual scientist is one of justification. Participation in research for other than altruistic aims can be a very unsettling experience. Each of the participants in the GALCIT Rocket Research Project had his own particular rationale for working on rockets destined for military use. Hsueh-shen Tsien looked upon his work through the purest of lenses, seeing only the mathematical challenges and exploration inherent in it; yet, he was deeply distressed by his own lack of direct involvement in the Nationalist Chinese war effort against Japan. He has since built ICBMs for the Chinese People's Republic, some of which are undoubtedly trained on the city where he once studied in Southern California.

Frank Malina, perhaps the most conscientiously moral thinker of the group, never denied his distaste for military research. By 1945, his underlying distress at having to meet army regulations and timetables in the development of rockets was stronger than his enthusiasm for building them. His conflict revolved around the troubling recognition that in the post-World War II era, nuclear weaponry would place the sovereign national state in a

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position of unprecedented power. If this virtually unlimited power should sit in a missile's nose cone, then only a strong international organization might limit the destructive expansion of Twentieth Century nation-states. In 1945, it was altogether unclear to Malina whether the United Nations would be able to fulfill that role and actually rein in competing nations.

German scientists working on the V-2 ostensibly suffered from no such distasteful moral conflicts, but still had to contend with the overarching power of Nazism and the rivalries of competing bureaucracies within the fascist state. These rivalries could prove maddening to scientists working at Peenemünde. Although the Nazi government had the outward appearance of a monolithic totalitarian regime, in reality, inter-service competition and intense Nazi Party suspicion of the established Army Corps and civilian bureaucracy disrupted the normal logistic web. When Army Commander-in-Chief General Walther von Brauchitsch decreed the Peenemünde project to be "particularly urgent for national defense," he spoke on his own, "without consulting either Hitler or the Armed Forces High Command." [181] Placing the army strategically at the vortex of rocket construction, von Brauchitsch took advantage of a poorly defined chain of command, riddled with intra-Party jealousies, incompetent administrators, and competing priorities.

The Army Commander saw the A-4 as a weapon whose war-winning potential the Nazi hierarchy was only just beginning to grasp. Convinced by Walter Dornberger of the future value of the long-range rocket, von Brauchitsch wanted, in all likelihood, to get in on the ground floor of a big technological change in weaponry. However, Hitler did not see it that way in

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[181] Neufeld (1995), pp. 119-120.

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the fall of 1939. There were other demands for steel production then pressing upon the German industrial apparatus; Hitler only ordered that A-4 testing continue, without accelerating its pace. [182]

The struggle to create new technology in Nazi Germany provides a high-relief by which to view the growing dilemma for rocket engineers working under government contract in the United States in the 1930s. A lack of support at the very top of the administrative hierarchy stymied the development of the A-4 in the early 1940s, but did not halt it. In a situation of confused bureaucratic priorities, with various ministries stealing resources from each other, the success of a new venture depended upon how resourceful and influential its backers were. Having the right advocate to make the case for the V-2 could make all the difference.

The German missile program was a long-term creation of fertile scientific minds. So long as some funds were coming in to Peenemünde work could continue. A skillful manipulator of the war-psyche of a dictator (such as Walter Dornberger) proved crucial in preventing the demise of the A-4 program. In the United States, a perspicacious general—Hap Arnold—teamed up with a somewhat adventuresome engineering school professor—Theodore von Kármán, to prevent rocketry at Caltech from sinking into the backwater of neglect. In both Germany and the United States, wide institutional support for rocketry was initially lacking. What the new field needed was not popular acceptance but the right backers. These, the A-4 found in certain elements of the German Army. Only in 1942, when the

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182 Neufeld (1995), p.122.

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military situation for Germany was becoming more desperate, with the Soviet Army dealing reversals to Hitler's forces, did the German dictator and his SS chief Himmler begin to see the development of the technology as a potential savior of the Nazi state.

The most troubling aspect of state sponsorship of rocket technology under the Nazi regime was its open-ended future. There was theoretically no limit to the use of this new and devastating weapon beyond the inherent limitations of the technology itself. Even so, it is unlikely that the German Army would have been able to develop a long-range version of the A-4, or to deploy more Me-262 jet fighters, even if the war had lasted until 1947 or 1948. Allied bombing of German factories and of Peenemünde (beginning in August 1943) had crippled Germany's ability to mass-produce new technologies, except in underground bunkers.

There was no sense among Peenemünde scientists that the new weapon was merely to be the fruit of wartime exigency. What lay ahead was wider use, greater destructive power, and longer range. Since the military controlled funding, personnel procurement, and production of the missiles, the new V-2 was meant to be harbinger of an awesome standard of military technology destined to alter warfare forever. Oddly enough, it was left to the United States and Russia to realize the potential of the ballistic missile as an instrument of strategic policy—a world-shaper.

In the United States, the prospects for military applications of rocket technology remained tenuous even in the late 1940s, because the American military was (predictably) reluctant to fund new projects after the war's close.

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Frank Malina commented in his memoirs (1971) that Captain Robert B. Staver of Army Ordnance, along with his Army Air Force counterpart, Colonel W. H. Joiner, liaison officers at Caltech during the war years, both feared "the historic tendency of the government to drop potentially important research for military purposes when a war ended." [183]

Malina himself was more reserved and circumspect in his support for continued military research of the rocket. His deeper misgivings about the military link to academe took decades to mature. Only in 1971 did he write:

> I have pointed out before that there was a conflict of opinion as to the kind of work that could or should be undertaken by an academic institution of higher education and research. Von Kármán and I were of the view that Caltech was an appropriate organization to take responsibility for basic engineering research, but not for the prototypes of engines and of vehicles that involved problems of meeting production and end-result specifications. [184]

Malina could have been pointing toward separate development of missilery by private industry—of which his own Aerojet Corporation was one crucial part in 1944. But it is clear that on some level he opposed university involvement in research directly leading to the production of new missiles for the armed forces. How he would have made the distinction between research on a technology with obvious military applications and direct participation in its mass-production is not clear at all, however. Malina must

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183 Frank Malina. *America's First Long-Range Missile and Space Exploration Program: the ORDCIT Project of the Jet Propulsion Laboratory, 1943-1946: A Memoir*, presented at the Fifth History of Astronautics Symposium of the International Academy of Astronautics, Brussels, Belgium, September 1971. Washington, DC, NASA Archives. LEK 1/12/3. pp. 343-344.

184 Malina (1971), p. 344.

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have realized that there was no sure demarcation line between weapons-related research and development.

It is possible to design a rocket motor strictly for upper atmospheric research, but not possible to accept military funding for that research and expect that the armed forces would hesitate to turn the fruits of the research toward war aims. In 1942, no less than in 1938, Malina, the intellectual, saw the end results of his research in moral terms: a 'right' cause was engaged in the defeat of a greater 'evil.' But had Malina deeply considered the implications of his research for the future, when the war against fascism was over? This is not at all clear, although the Pandora's box he and the GALCIT team had helped to open could not be so easily shut.

In the early 1940s, the GALCIT Rocket Team evidenced little discomfort over its military research work. In 1944, when a $3,000,000 Army contract with GALCIT for the development of a long-range rocket looked promising, Malina's reaction was that the letter announcing the army's intentions "threw us into a proper dither!" [185] Such an extravagant promise in 1944 surprised Malina and the others; for a small rocket-development project this sum seemed to exceed their wildest hopes. In 1938, they had received $10,000. Even in 1942, the GALCIT Rocket Research Project's budget was only $115,000.

In his 1971 memoir of the ORDCIT Project (Ordnance, California Institute of Technology), Malina did not mention qualms he may have had about Army generosity. On the contrary, his history of the project leaned toward the technical aspects of building a long-range rocket. Malina's proudest moment

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[185] Malina (1971), p. 345.

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came in 1945 with the development of a true sounding rocket, the WAC Corporal. Ironically, in 1949 this first rocket to reach well into outer space would sit atop a V-2 missile built by the Nazi war machine. (This two-stage vehicle was known as the 'Bumper WAC.') But by then Malina had left behind the fledgling Jet Propulsion Laboratory and was working for the United Nations in Paris. Did doubts simmering in him for some time finally compel him to change his professional situation?

There is no question that personal considerations prompted Malina to consider leaving JPL in 1945. His marriage to Liljan Darcourt was on shaky ground. Then there was his acrimonious relationship with Clark Millikan. And—the world was intruding on his private life. In August 1945, two nuclear weapons detonated over Japan. Malina, like Robert Oppenheimer, had serious questions about the power now evidently resting in the hands of one nation—in fact, in humanity's hands. An internationalist, he saw dangers ahead if the national state were to possess unlimited power and advanced technology. But his research had helped that very state gain its postwar technology and thus, immense power.

In 1945 and 1946, Malina was clearly a man in conflict. He had succeeded in his life's work. His professional gamble had paid off. But at a cost, for as proud as he was of his technical accomplishments, he also knew that he had helped to hand an exotic new weapon to the US government. The American military had just changed the rules of warfare with its attack on Hiroshima. GALCIT rocket engineers fashioned, by the fall of 1945, a potential delivery system for nuclear weapons. If Malina had foreseen, as a young Ph.D. in 1940, the consequences of his work just five years hence, perhaps he would have

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halted it. His work had gone on. He and the GALCIT crew became so enmeshed in the technical aspects of their research and so flush with their emerging success that they failed to recognize—or perhaps admit—the full import of their inventions.

If anything, Malina quite enthusiastically wrapped himself in the war effort and was glad to have the money necessary to carry out his research. In 1938, the GALCIT Rocket Team was a group of hard-pressed, struggling young enthusiasts. Funding from the Army Air Corps renewed their hopes. Malina was elated over the government support the group was about to receive. He wrote:

> Parsons and Forman were delighted when I returned from Washington with the news that the work we had done during the past three years was to be rewarded by being given government financial support....We could even be expected to be paid for doing our rocket research! "[186]

There is a touch of incredulity in his remarks. All these young men were to spend the war in a university setting doing research. Their work was considered important to the war effort and none were drafted. Malina, Tsien, and Summerfield were men of conscience, with political sensitivities. They did not shirk their duty in the effort against fascism; they grasped it wholeheartedly.

What had begun as work for a noble crusade and a technological adventure in the late 1930s became a far murkier moral dilemma for Malina and possibly other members of the GALCIT Rocket Team by 1945. Yet, the feeling he

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186 Malina (1967), p. 126.

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expressed after the success of WAC Corporal mirror only pride at the culmination of a long-sought-after dream.

> I was the sole member of the original GALCIT Rocket Research Group of 1936 to experience the culmination of its hopes after the many vicissitudes in rocket development over the ensuing period of 10 years. It is difficult to describe my feelings as I watched the sounding rocket soar upward. One can think of many things in a few minutes. One of the thoughts was that I could now turn my mind to other goals in a world full of both fascinating technological possibilities and of desperate social problems. In the astronautical fraternity, the tradition of lighting up a cigar had not as yet begun. We contented ourselves by letting out whoops of joy, shaking hands, and patting each other on the back. [187]

Malina later described the first meeting of JPL personnel and "members of the German V-2 development team who had been brought to the USA to be incorporated into the [Army] Ordnance Department's long-range missile program." [188] In 1945, von Kármán and a team of aeronautical engineers, including Tsien, had toured Germany, meeting both with Adolph Busemann—developer of the swept-back wing and proponent of supersonic flight—and Ludwig Prandtl, von Kármán's teacher at Göttingen, who had continued his research, oblivious to the destructive predations of the Nazi regime around him. Von Kármán also toured Nordhausen, where the SS forced slave labor to construct V-2s in underground tunnels. Yet, Malina had little overt reaction to reports of German atrocities associated with the V-2 project—his only written comment was: "By this time (late 1945) we were quite well-acquainted with the V-2 missile." [189]

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[187] Malina (1971), pp. 365-366.

[188] Malina (1971), p. 369.

[189] Malina (1971), p. 369.

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Malina's long association with military-related research had somewhat shifted his priorities or at least dampened his expression of the conflict simmering within him. There is no discussion of the ethical vortex into which he, von Kármán, and other JPL scientists had descended by using captured German 'war-booty,' even though the elder statesman of American aeronautics knew first-hand that the Germans had produced V-2s at a cost of many lives. Both Nazi rocket scientists and the V-2 were fruits of war and proved a boon to the American rocket program.

Malina's predicament mirrored that of any scientist who commits to working for the benefit of society through his efforts in government-funded research. Malina operated under a dual system of values. His more ethical side expressed recognition of a distinction between university research and ends-directed production of a new technology. For Malina, mixing the two caused considerable internal friction. The second value Malina and other GALCIT engineers embraced was that of fostering a noble social goal by harnessing the power of government to the refined methodology of the research laboratory. The result can only be productive for the researcher, although he risks losing sight of the ultimate use of his new knowledge. The same issue shadowed physicists at Los Alamos during the Second World War, although not all of them had misgivings about the ultimate ends to which their new marvel would be put.

From penury in 1937 to plenty in 1942—it took five years for the fortunes of the GALCIT Rocket Team to reverse themselves. In this short span of time, the world situation matured from Depression to full-scale war. Nations were

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busy harnessing devastating technologies for the second time in less than 30 years. By 1942, the GALCIT Rocket Team had learned to focus its energies on completing its task: the creation of a rocket propellant which could be stored on a shelf for months. In striving toward this goal, Malina, Tsien, Summerfield, and Parsons gained deeper theoretical understanding of rocket combustion. Somehow, while trying to achieve a balance between the pursuit of science and attention to their social and political lives, the center of gravity kept shifting for these men.

For Malina and Summerfield, political ideals occasionally became the justifications for scientific endeavor: but science came first. In a revealing comment, Malina's first wife, Liljan, spoke of how Malina brushed aside her concerns about the different paths their lives were taking in the early 1940s with the comment: "I am very involved in something that is very important, and all of this is simply going to have to wait." [190] Jack Parsons, however, had little problem pursuing the dual goals of his creative inventive self. He usually thrived on the very edge of experience. All the while Tsien maintained a discrete boundary between his duty as a Chinese national and his passion for science. Usually, the latter took precedence.

The men who proudly identified themselves with the GALCIT Rocket Team in the 1940s sensed their destiny and followed it. For Malina the more pressing issue remained, not merely experimental success, but the increasing stress his various administrative roles were to place upon him. In the mid-1940s he was simultaneously doing research at Caltech, helping to administer the new Jet Propulsion Laboratory, (built with military funds), and serving as

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[190] Interview with Mrs. Liljan Wunderman, April 22, 1996.

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an executive at Aerojet, Inc., the company he had helped to found in 1942. (Aerojet was then under contract to build JATOs for the navy and Army Air Force.) So Malina stood to profit by military contracts, even while he worked directly for the Armed Services at JPL. Whatever doubts Malina may have had about the morality of wartime research for the military surely did not surface in 1942 and 1943. [191] After nearly five years of such work, the outlook of the GALCIT team had matured. They were, in fact, ambitious for further success.

Tsien, who returned to MIT to further his academic career in the fall of 1946, again found the Massachusetts environment unsuitable and, after a visit to China and an auspicious marriage to a Chinese opera singer, returned to Caltech by 1949. [192] Two years before, Malina's had permanently departed for Europe. His marriage to Liljan Darcourt was over; she, in fact, had moved to New York City (1946) in order to pursue an independent career in art. In June 1947, while at UNESCO headquarters in Geneva, Malina met Marjorie Duckworth (1918-), the woman who would become his second wife and the mother of his two sons. [193]

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[191] Malina (1969), p. 196. Malina went on to write: "The more strenuous objections to possibilities of our having conflicting interests, however, were soft-pedaled because there were so few qualified persons in the country to deal with the required expansion of rocket propulsion development and production."

[192] Chang (1995), pp. 121-148.

[193] Marjorie Duckworth Malina— personal correspondence with this researcher, April 22, 1998. Mrs. Malina was a personnel officer for UNESCO at the time, helping to recruit for the Department of Natural Sciences, where Malina worked. The Malinas' two sons are Roger (1950-) and Alan (1952-).

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In the meantime, the war years had shaped the careers of GALCIT Rocket Team engineers, and the men themselves had, through their research, changed forever the technological equation for the United States. The propellants they discovered and produced were to lead to the creation of long-range ballistic missiles tipped with nuclear warheads. They had helped to alter the balance of power in the postwar world. No longer were they scrounging to find bits and pieces of rocket material in Southern California junkyards. They were accomplished scientists and creators, complex men who had changed the world.

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Chapter 6

The GALCIT Rocket Project: A Portrait of Dedication, Intensity, and Innovation.

Designing rockets, building rockets, understanding the theoretics of rockets: these are exotic tasks, even in the late Twentieth Century. In 1935, they were the purview of a handful of men, some of whom worked alone, some of whom were amateurs. Among them were established chemists and engineers. Few worked as part of an academic team. Herein lies the uniqueness of the GALCIT rocket group. An odd mélange of characters, their early contacts were quite fortuitous. Some of them were at dead ends in their research before they turned to rocketry. Others were dissatisfied with their professional and academic environment. Still others, notably Parsons and Forman, were adventurous experimenters looking for the right home for their unconventional ideas and ways.

Looking back through time, it is remarkable that such a group of disparate students of science could have worked together at all. The glue that bound them together was Caltech and their enthusiasm for rocket building. The Institute was the magnet which attracted them. Caltech provided a milieu for these men which allowed them to range beyond the pedestrian concerns of engineering: creating and designing for established corporations, holding to limited visions. While GALCIT aggressively forged links to the aircraft industry in the 1930s, the laboratory also permitted experimental thought to blossom. In this environment tolerant of unconventional ideas, young graduate students felt they could extend the limits of their explorations into

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aeronautics. Students like William Bollay and Frank Malina, Hsueh-shen Tsien, Martin Summerfield, and 'Amo' Smith, whatever their motives for joining in the development of rockets, decided to investigate an area of engineering design and theory that had not been systematically explored before. In 1935, little was known of the theoretical dynamics of rocket propulsion. Only a small number of men (Goddard, Esnault-Pelterie, General G. Arturo Crocco in Italy, Louis Damblanc, Sergey Korolev, among them) were attempting to define the mathematics of rocket propulsion while conducting controlled experiments with static rocket motors. [194] Writing a dissertation on the theory of rocket propulsion was outlandish and risky, although it did present concrete challenges to the student well-versed in calculus, the structure of materials, and chemistry.

The years 1936 to 1939 were difficult, tentative, fluid years for the GALCIT Rocket Research Team. These three intense years were also a crucial period of definition for the group. New members joined; others drifted away. The team nearly broke apart under the stress of Depression economic realities. From the time in March 1936, when von Kármán approved of Malina's thesis in rocketry to the beginning of liquid propellant rocket motor tests in the barren *Arroyo Seco* later that year, a nucleus of men—Malina, Parsons, and Forman—had formed and remained together at least until 1943.

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[194] G. Arturro Crocco was an aeronautical engineer in the *Genio Militare Italiano* in the 1920s and 1930s, a general in the Italian Army under Mussolini. He began research into supersonic flight and rocket propulsion soon after World War I, and even received army funds for his research into the military possibilities of rockets as early as 1927. Louis Damblanc, a French aeronautical researcher, invented and tested multi-stage powder rockets in the 1930s. See: *First Steps Toward Space* (1985), pp. 33-48; 49-55.

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The next year, 'Amo' Smith and Tsien joined the group, while William Bollay prepared to leave for Harvard. Smith and Malina wrote their first (and only) collaborative paper on sounding rockets, Weld Arnold's generosity saved the group from financial disintegration, and with von Kármán's blessing, Malina prepared to enter the academic circuit: by early 1938, he had traveled to New York to discuss the fruits of his and Smith's work before a nationally recognized conference of aeronautics experts.

This promising start turned into near disaster for the rocket team by the summer of 1938. Tsien and Smith began to turn their interests elsewhere, Malina struggled to stay alive economically, working with von Kármán on seemingly unrelated research (how the wind creates sand dunes.) But 1939 proved more felicitous, for despite Smith's absence, the bond between Tsien and Malina strengthened, Parsons' and Forman's incessant experimentation with solid propellant rockets began to reveal important insights, and the National Academy of Sciences saw fit to grant $1000 to the team for continued research. Three years filled with drama, hard decisions, and hope: 1936-1939 would prove the foundation stone of success in the coming decade.

The GALCIT Rocketeers had coalesced around Frank Malina in 1935 and 1936. Malina imagined himself as the founder of the GALCIT project and as the one team member who held the group together when argument or competing interests threatened to dissolve it. In his recollections of the first collaboration with the Army Air Corps (GALCIT Project No.1, 1939-1946), Malina evaluated his early role in the group's success: "The work I initiated

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with the GALCIT Rocket Research project in 1936 had blossomed within eight years into a major activity of the Institute. "[195]

Was Malina the originator of the GALCIT Rocket Research Project, as he claimed to be? There are several instances in which Malina spotlighted his own role in the survival of the group, although he was in no way self-aggrandizing. In fact, his comments more often than not appear to be modest and rather quietly self-assertive. Malina frequently became the point-man for the GALCIT team: it was he who visited Robert H. Goddard in Roswell, New Mexico, September 1936 (while heading home to Brenham, Texas.) This short tour of Goddard's experimental station in the desert followed a Pasadena meeting between the two men on August 28, 1936, a rendezvous arranged by Robert Millikan. It was Malina who traveled to New York in January 1938 to present 'Flight Analysis of the Sounding Rocket' at the sixth annual meeting of the International Astronautical Society. Malina was then a Ph.D. candidate, a claim that Smith could not make.

But to assert the predominance of any one individual as the catalytic force in the survival and success of the GALCIT Rocket Team would be presumptuous. In fact, if any one individual were to be credited with that pivotal role, it would have to be von Kármán, who was the only Caltech administrator in a position of authority willing to support the group's work. The Rocket Research Group's failures and successes were the result of collaborative efforts. More precisely, the pyramidal effort of several bright

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[195] Frank Malina. 'The US. Army Air Corps Jet Propulsion Research Project, GALCIT Project No. 1, 1939-1946: A Memoir,' (unpublished.) Third International Symposium of the International Academy of Astronautics, 1969. Washington DC, NASA History Collection, LEK 1/12/3, p. 196.

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and energetic individuals, each of whom wanted to make his mark in the field, created the JATO. Some in GALCIT's band of rocket engineers came to the work entranced by the almost spiritual and certainly dangerous nature of rocketry in the 1920s and 1930s. Some came out of love for the purely theoretical side of discovery. Some, perhaps all, were personally ambitious, and one (Clark Millikan) jumped on board when it appeared the work was turning interesting and successful.

Much of the administrative work fell to Malina, once the project became successful in the 1940s. He naturally inherited the role of group standard-bearer in the late 1930s, as one of the original actors, and as Caltech graduate student. Even Malina himself admitted how bleak prospects for the group were in 1938:

> During the summer of 1938, Smith began working in the engineering department of the Douglas Aircraft Company.... Arnold left Caltech for New York, and completely vanished as far as we were concerned....Tsien was able to devote less time to the work of the project, as he was completing his doctorate under von Kármán. I struggled on with Parsons and Forman....We also had less time to devote to rocket research, for we had to support ourselves. Parsons and Forman took part-time jobs with the Halifax Powder Company in the Mojave Desert, and I began to do some work on problems of wind erosion of soil with von Kármán for the Soil Conservation Service of the US Department of Agriculture. [196]

In fact, the only event that saved the Rocket Team from an early demise was the burgeoning interest of the Army Air Corps and the National Academy of Sciences in rocket-propelled boosters for aircraft. From early 1936 until the summer of 1938, the group struggled to survive financially, even when, in its

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196 Malina (1967), p. 124.

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initial year and a half of existence, energy and commitment peaked. Clearly a pattern of spontaneous interconnectivity, a forging of intellectual links based upon shared hopes and visions, manifested itself early. This is the dynamic core of the GALCIT Rocket Group's 'internal engine.' The circumstances for rocket research were right at Caltech in early 1936, and the pieces of the montage that made up the working group began to fall into place. Despite so much enthusiasm, neither Malina, Parsons, nor von Kármán could be sure in 1938 that the ravages of the Depression would not take their toll on their efforts. The drive was there, but financial security had vanished.

Recalling the rocket group's tentative beginnings, Malina said that "[Bollay] knew evidently that I had an interest in rocket propulsion and turned Parsons and Forman over to me to have a chat." [197] Parsons and Forman brought their own special fire to the group. Of course, Malina could not have then known how mercurial a figure Parsons was. It would not have been Parsons' earnestness that won him over to Malina; more properly, it was Malina's sober assessment that Parsons was already a skilled rocket builder and a chemist in his own right.

Malina needed Parsons and saw Forman's mechanical skills as a plus, too. Parsons and Forman sought the respectability that affiliation with GALCIT could provide. Even though, as Malina would later admit, there was little he and Parsons shared in common outside of their work, both possessed a creative spark that was to unite them. Since Malina, in all his written references to Parsons, displayed no hint that he was anything but the

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[197] Hall Interview with Malina (1968) NASA History Archives, Washington, DC. LEK 1/12/3, pp. 3-4.

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dispassionate scientist, scrupulously avoiding obvious references to his emotional attachment to the project, the often strained, sometimes exasperating, even amusing relationship between the two men did not easily shine through. However, the fact that he and Parsons tried concocting a screenplay, hoping to sell it to Hollywood and finance the group in rather desperate times, indicates that his creative bent, or at least his fantasies, pulled him closer to Parsons.

Malina, Parsons, Forman, Summerfield, Smith, all these men were to come into ready contact with explosives and unstable liquid propellants, with rocket motors that could shatter or ignite their surroundings quite without warning. The element of adventure certainly inspired their exploration into the frontiers of engineering. Perhaps the quality of living and working on the edge is one that appeals to young people, whether intellectual or not. It, too, was a part of the group's chemistry.

There was an added incentive: the ambition laced throughout academic work: the ambition to succeed at a chosen task, to achieve the recognition that comes of discovery. For Parsons and Forman, this ambition took the form of satisfaction at their acceptance, publication of their research, the construction of their inventions—even patents. The burning desire to build and set off rockets drove them on. For Malina, ambition took the form of the need for recognition within the academic community which, outside a small circle of devotees, had largely scorned rocketry as not fit for serious pursuit. Within two years, he had achieved a small measure of this recognition. Malina's collaborative work with 'Amo' Smith on the performance of a sounding rocket eventually wound up in an issue of the *Journal of the Franklin*

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Institute (1940) and led, more immediately, to an invitation from von Kármán to conduct a GALCIT seminar in April 1937 on the results of the rocket group's first year of work. [198]

Malina also considered sending a variant of this paper to France as an entry for the 1938 REP-Hirsch Prize in Astronautics. However, before he was able to do so, he learned of another paper on a topic similar to his, recently published by Willy Ley and Herbert Schaeffer. [199] Feeling somewhat chagrined, he decided not to apply for the prize that year. The next year he re-entered the REP-Hirsch competition with another paper. Malina claimed to have been motivated more by financial concerns than anything else in 1939; the prize consisted of 1000 francs and a medal. He did not discover that he had actually won the 1939 prize until 1946, after five years of war.

According to von Kármán, General Hap Arnold's May 1938 visit to Caltech inspired Malina to continue his research. [200] Malina spoke that December at the Caltech chapter of *Sigma Xi* and soon afterwards, von Kármán assigned him the task of traveling to Washington "to give advice [on] the possible use of rockets for the assisted take-off of heavily loaded aircraft." [201] Malina then prepared a report to be delivered to the National Academy of Sciences entitled a 'Report on Jet Propulsion for the National Academy of Sciences Committee on Air Corps Research' (December 28, 1938).

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198 Malina, Frank. 'Characteristics of the Rocket Motor Unit Based on the Theory of Perfect Gases.' *Journal of the Franklin Institute* , Vol. 230, No. 4, 1940, pp. 433-54.

199 This paper was 'Les Fusées Volantés Météorologiques,' published in October 1936 in *L'Aerophile.* (Volume 44, 1936, pp. 228-32.)

200 Von Kármán (1967), p. 243.

201 Malina (1967), p. 125.

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On the basis of this report, and undoubtedly with the support of both von Kármán and of General Arnold, The National Academy of Sciences granted the GALCIT group $1000 for continued rocket investigation. This was heady stuff for a graduate student. The economic boost gave added impetus to the group's work exactly when hopes seemed to be fading. It did not diminish Malina's standing among the others, either; government support delighted Parsons and Forman.

Yet, as well-placed as Malina was within the small and budding nucleus of the Rocket Team, there were other formidable minds and personalities at work within the group. Hsueh-shen Tsien and Martin Summerfield both altered the group's dynamics and contributed profoundly to its theoretical and practical success. Tsien had a keen eye for inserting himself into a high-powered physics or engineering program at just the right instant, leaving his indelible mark upon it. He was intense, yet able to communicate his ideas to his peers (those he felt of his intellectual stature) and a dynamo whose aristocratic exterior never quite hid his brilliance.

Tsien was as unique to GALCIT as was Parsons, 'Amo' Smith, or Malina. There was no question that his demeanor could create friction, although he got along well with Malina and von Kármán. The key to Tsien's personality lay in his incisive intellect and his self-confidence. Tsien was well aware of his prowess. He felt himself much more at home under the wing of von Kármán than with a technical rocket builder such as Parsons. Never comfortable with manual labor, Tsien's mind penetrated deeply into the mathematical structure of rocket flight dynamics, into the design and

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thermodynamics of rocket motor combustion: this is where he best fit into the GALCIT world. He would never be as gregarious as was Parsons, but that did not matter to him. At Caltech, Tsien would contribute to the theory of supersonic flight and turbulence—particularly near the surface of supersonic airframes. He would become a pioneer in the study of high speed flight and a major force in rocketry in the mid-twentieth century.

Each man on the Rocket Research Team, in fact, had his own niche and task to perform. Yet, there was no clear division of labor, at least not in 1936. Tsien fitted in best as one of two interpreters of von Kármán's methods of mathematical discovery. (The other was Malina.) Martin Summerfield touched on one aspect of Tsien's character: his ability to give detailed structure to ideas that had been only sketched out by even more brilliant minds than his. "He [Tsien] became a close assistant—Kármán's arms and legs—working out formulas that Kármán had masterminded. He had the brilliance and the speed. It was unusual to find someone like him." [202]

Yet, unlike von Kármán, who could generate ideas in the middle of a classroom lecture or when surrounded by a few of his devoted students in his study, Tsien had to work in solitude. He often worked late into the night, developing the full structure of von Kármán's insights or even his own original work. Tsien liked to present ideas fully-formed, with all their potential diversions, flaws, and details already resolved. He could think fast on his feet and was often several steps ahead of the concept he was writing down; but he was somewhat more rigid than von Kármán, whose natural

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[202] Chang (1995), p. 65.

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pedagogical style encouraged a free exchange of ideas and the flexibility to see new ideas as improving and deepening a line of thought.

Perhaps Tsien had less secure a personality than von Kármán; he had greater technical mastery of mathematics than did Malina, but he was far more reclusive a person. At the age of 25 (in 1936), he was already well on his way to his Ph.D. in aeronautical engineering (which he gained three years later, in 1939), and was a confidant of the director of his own school. [203] One of his roommates at Caltech, Shao-wen Yuan, saw one side of Tsien's character: "He was [a] very stubborn, very individualistic fellow who criticized others quite often. He always thought he was right, and usually he was. But he made a lot of enemies." [204]

Tsien's intellect and arrogance could alienate people, although apparently not Frank Malina, who considered him a close friend. Tsien had overheard 'Amo' Smith and Malina discussing rocket dynamics in Smith's office at GALCIT early in 1937. He then read Malina and Smith's article on the 'Flight Analysis of the Sounding Rocket,' and questioned Malina about its details at the end of class one day. By May of that same year, Malina had invited Tsien to join the Rocket Research Team. [205] Tsien and Malina began to collaborate on theoretical studies of rocket propulsion ('Flight Analysis of a Sounding

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[203] The exact title of Tsien's Ph.D. dissertation is: 'Problems in Motion of Compressible Fluids and Reaction Propulsion' (1939.) The dissertation is on file at the Caltech Library in Pasadena.

[204] Chang (1995), p. 79.

[205] Chang (1995), p. 69. In a 1996 oral interview, A. M. O. Smith commented to this researcher that Malina had been the principal author the article on flight characteristics of the sounding rocket. He made this remark in response to a question as to why Malina had been chosen to deliver the paper at the New York convention of the Institute of Aeronautical Sciences in January 1938, rather than both Malina *and* Smith.

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Rocket with Special Reference to Propulsion by Successive Impulses,' published in the *Journal of the Aeronautical Sciences*, 1939.) [206] It is significant that the twin themes of rocket propulsion and sounding rocket construction should crystallize so early in the Rocket Research Team's history. It was Malina's cherished hope to build a sounding rocket, but the group realized early on that it would not be possible to construct a motor of sufficient thrust to carry such a rocket into the upper ionosphere—not in 1937, at any rate. [207]

In February of 1936, Malina, Bollay, Parsons, and Forman had all discussed a program of action for the small group, and had come to the conclusion, on the basis of sketchy information from Eugen Sänger's research, as well as the limited success of the American Rocket Society's efforts in the New York region, that building a sounding rocket was out of the question if existing vehicles could not even reach several hundred feet in altitude. As for Parsons and Forman, Malina only commented that the two men "were not too pleased with an austere program that did not include the launching, at least, of model rockets." [208]

The next year Malina found in Tsien a qualified candidate to pursue the group's immediate goal, the theoretical understanding and design of a working liquid-combustion rocket motor. Malina noted in his memoirs of the 1936-1938 GALCIT years:

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[206] *The Journal of the Aeronautical Sciences*, No. 6, 1939, pp. 50-58. See also Chang (1995), p. 277.

[207] Malina (1967), p. 115.

[208] Malina (1967), p. 114.

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> In the winter of 1938, Tsien and I...extended the study, by Smith and me, of the performance of a sounding rocket to the case of propulsion by successive impulses from a constant-volume solid propellant rocket engine. We had reviewed Goddard's 1919 paper on "A Method of Reaching Extreme Altitudes"and decided to find a mathematical solution for the flight calculation problem, which Goddard had not carried out. We did this in spite of the difficult practical problem of devising a reloading mechanism for such a rocket engine, for at that time no propulsion method could be discounted. [209]

It is clear from a comparison of the two papers, the first published by Malina and Smith in January 1938, the second a joint effort by Malina and Tsien (July 1938), that the collaboration of Tsien and Malina resulted in a formidable mathematical exposition of a more challenging problem than had the work of Smith and Malina. [210] Smith and Malina generally confined their calculations to differential calculus of one variable; Tsien and Malina concentrated on the theoretical problem of propulsion almost exclusively, rather than on aerodynamic considerations such as drag. They worked with non-linear differential calculus, although they attempted to simplify some of the parameters so as to convert the more complex non-linear mathematics into linear differential equations. They came to the significant conclusion that powder rockets, which burn in successive, but minutely-spaced intervals, produce less thrust than constant pressure liquid propellant motors. This

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[209] Malina, Frank. (1967), p. 124. Malina is probably inaccurate about the date of the work with Tsien, because the paper, as published in the *Journal of the Aeronautical Sciences*, is dated July 6, 1938 (the date the completed paper was received by the journal.)

[210] 'Flight Analysis of the Sounding Rocket,' (1938); 'Flight Analysis of a Sounding Rocket with Special Reference to Propulsion by Successive Impulses,' Malina & Tsien, July 1938. Both papers may be found in the Library of Congress (Madison Building), Malina Collection, Box 18.1.

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may have had an effect on the direction of research for the group, as Malina and Parsons eventually abandoned nitrocellulose powders in favor of another more stable and complex constant burning solid propellant, and simultaneously, along with Summerfield, worked on an effective hypergolic liquid propellant. [211]

Smith handled complex mathematics quite competently, as he was an aeronautical engineer responsible for the design of propeller and jet aircraft at Douglas Aircraft for several years; yet few could claim to rival the penetrating ability of Tsien to construct detailed solutions to novel and difficult problems. Tsien was a mathematical architect. He could build an entire mathematical edifice from a single sketch. The remarkable fact is that both Tsien and Smith, coming from very different circumstances, should land squarely in the center of the fledgling rocket research group.

In January 1938, von Kármán presented a paper he and Tsien had written on the 'Boundary Layer in Compressible Fluids.' [212] This paper evaluated the thermodynamic and wave flow characteristics of fluids (such as air) near material surfaces at high Mach numbers. At supersonic speeds exceeding Mach 2.6, Von Kármán and Tsien asserted that the viscosity of a fluid in the boundary layer between the fluid and the skin surface of a supersonic projectile (such as a rocket) diminishes; consequently, the transfer of heat from the quickly moving fluid to the surface of the projectile actually

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211 The term *hypergolic*, first used by the Germans in their experimentation with propellants, refers to liquid propellants and oxidizers which ignite spontaneously on contact with each other.

212 Von Kármán presented this paper at the Aerodynamics Session, Sixth Annual Meeting of the International Aeronautical Society, January 26, 1938. See: *The Collected Works of Theodore von Kármán, Vol. III, 1933-1939*; London, Butterworth's Scientific, 1956, pp. 313-323.

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increases many-fold. This contradicted the notion that at high altitudes the decreased density of the air would essentially reduce skin friction.

> The ratio of skin friction and wave resistance (in the case of a rocket) is now 0.123/0.100, = 1 : 23. If the boundary layer is partly turbulent, the ratio will be even greater. This shows clearly the importance of skin friction in the case of a slender body moving with high speed in extremely rarefied air. It also disproves the belief that wave resistance would always be the predominating part in the total drag of a body moving with a velocity higher than that of sound. The reason underlying this fact can be easily understood when one recalls that the wave resistance of a body is approximately directly proportional to the velocity, while the skin friction is proportional to the velocity raised to a power between 1.5 and 2. Therefore, the ratio of skin friction to wave resistance increases with speed. [213]

If, as is implied by this paper's calculations, the boundary layer between a highly fluid medium and a solid surface breaks down at Mach speeds, then flying at supersonic speeds is indeed a complicated test of endurance. Turbulence in this boundary layer would actually increase skin friction and create a more hazardous and unstable condition for a vehicle traveling at high speed in the upper atmosphere. [214] Tsien was a penetrating thinker and, highly unusual for a graduate student, almost von Kármán's partner in exploring the mathematical boundaries of new aerodynamic phenomena. He would prove the equal of any one of the GALCIT Rocket Team members, and a groundbreaking theoretician.

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213 Von Kármán, *Collected Works, III* (1956), pp. 319-320.

214 Of course, a rocket traveling at supersonic speeds would be traversing the upper atmosphere for a matter of seconds or at most, minutes, and not continually subject to atmospheric friction. Once it reached an altitude above 50 miles, it would be essentially in space.

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Summerfield, enigmatic in his own right, was ostensibly a man less at ease with himself than Tsien. Liljan Malina Wunderman spoke of him as 'one-dimensional,' as if he didn't quite "feel comfortable in his own skin." [215] However, one-dimensionality, or at the very least, an intense, determined focus on his work was characteristic, not only of Summerfield, but of most members of the Rocket Team. Summerfield's work was recognized for its groundbreaking insights, for its dogged exploration into areas of chemical and physical knowledge that had escaped detection. His discoveries shed light on crucial problems with liquid propellant rocket motors. He was a key member of the GALCIT team; yet he was never quite an insider.

Summerfield had been a roommate of Malina's at a small rooming house near the Caltech campus. He was a native of Brooklyn, New York, and earned his Bachelor's degree in physics at Brooklyn College in 1936. Summerfield also studied chemistry and mathematics at Brooklyn, taking courses which stood him in good stead while pursuing his graduate physics degree at Caltech. (In fact, his knowledge of chemistry was to make Summerfield indispensable to the rocket team.) Summerfield himself admitted in a 1994 interview with NASA historian J. D. Hunley that he was surprised to have been accepted by Caltech. He applied at the urging of Brooklyn College physics professor Bernhard Kurrelmeyer, but apparently had little expectation of entering the West Coast school. [216] "To my surprise, they accepted me," Summerfield related in the same interview. "It was there I met...Frank

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215 Wunderman Interview, April 1996, New York City.

216 Summerfield took a course in optics and an advanced physics lab with Kurrelmeyer. He also enrolled in other senior level physics courses, including Course 21 (in 1935) and Course 25 (1936). Kurrelmeyer later became Chair of the Brooklyn College physics department and retired in 1971.

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Malina. And he and I became good friends before I became associated with rocketry. It was then in the year 1939, that he suggested to me that I might join him in rocketry." [217]

Again, Malina was the conduit through which talented young physicists, engineers, and experimenters found themselves collaborating in the development of rocketry at Caltech. Whether or not Malina was actively recruiting new experts cannot be known, as he reveals few details of his decision-making process in his memoirs. But there is at least a connection. Not only did Summerfield and Malina room together, but Smith and Tsien shared an office. Tsien and Malina attended class together. Malina saw himself as shepherding the project through, under the fatherly supervision of von Kármán. Although Malina did not fully direct the project from start to finish, the talent appeared at his door, and he had the perspicacity to gather it into his small, but widening circle.

Summerfield's comment on this process is revealing. Speaking of the year Malina began his work in rocketry at Caltech, he said: "He [Malina] started in 1936, but he was amateurish at that point." [218] It was to be two full years, well into 1938, before the Rocket Team could consider itself baptized by repeated experimentation—and failure—and by substantive theoretical work. Early in 1940, Summerfield joined Malina full-time in rocketry. In June, he graduated

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[217] J. D. Hunley. Interview with Dr. Martin Summerfield, September 27, 1994, Monmouth Junction, New Jersey. Available from Capitol Hill Reporting, Washington, DC. Copy courtesy of Dr. Hunley, pp. 2-3.

[218] Hunley Interview with Summerfield (1994), p. 3.

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from Caltech. More precisely, he completed his thesis in June, but "June was too late to award the degree. And the degree was then awarded in 1941." [219]

Summerfield immediately began to work where Parsons, Forman, and Malina had left off: by studying the design and combustion characteristics of specific liquid propellants, their burn rates and efficiency. In 1938 and 1939, the three men had tried to construct test motors using an ethylene/gaseous oxygen combination. Forman, Parsons, and Malina systematically measured pressure and flow data through the motors with oxygen alone, checking the durability of the flow system, and then compared the data they had compiled with experimental data recorded by Italian rocket experimenter Aldo Bartocci in 1938. That same year, Bartocci had constructed a gaseous oxygen motor similar in size to the one built by the GALCIT crew. [220] According to Malina, "[Bartocci's] results were in close agreement with the theoretical analysis which Tsien and I had made." [221]

However, Malina and his colleagues were less fortunate in their design of the actual test apparatus, for in March 1939, while an experimental motor was firing—inside the GALCIT building—the oxygen line exploded, sending metal fragments all around a workroom. Metal shards from a pressure gauge imbedded themselves in a wall just at the point where Malina had been

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219 Hunley Interview with Summerfield (1994), p. 3.

220 Bartocci, Aldo. 'La Forza di Reazioni nell'Efflusso di Gas.' *L'Aerotechnica* , March 1938. See also Bartocci's work , 'Some Jet Propulsion Formulas of Over Thirty Years Ago,' in *First Steps Toward Space.* San Diego, Univelt, 1985, pp. 1-3.

221 Malina (1967), p.124. The studies done by Malina and Tsien began in 1936 and were eventually included in Malina's doctoral thesis (where he gave appropriate credit to Tsien), and in the article published by the Franklin Institute in 1940: 'Characteristics of the Rocket Motor Unit Based on the Theory of Perfect Gases.'

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sitting earlier in the day. [222] At the same time, the group, following tests by 'Amo' Smith, experienced modest success in finding materials (graphite and solid copper) for the motor nozzle capable of withstanding the intense heat of combustion. Clearly, a new threshold in the technology of the liquid propellant rocket motor was just around the corner. Not only did the men need to design and build a sturdier test apparatus, but they also had to devise some way of cooling the motor during ignition.

This is where Summerfield picked up and began to explore the chemical thermodynamics of rocket motor combustion. He also focused on the crucial connection between nozzle throat diameter and the potential of the motor to explode during ignition. By studying and thinking over the problem, Summerfield realized that there must be some optimum width for the nozzle throat. Too wide an orifice would allow hot gas to vent too quickly; too narrow a throat would bottle up exhaust gases and force pressure in the combustion chamber to rise precipitously.

Summerfield first re-designed the test apparatus to make it less likely to explode. Then he proceeded to determine a key ratio between the actual thrust achieved by a rocket motor and the *ideal* thrust. The latter is expressed by the equation $F_0 = p_c A_v$, where F_0 is the ideal thrust in a vacuum, p_c, the chamber pressure of the combustion chamber and A_v, the volume of the chamber itself. Summerfield realized that the angle of divergence of the nozzle just past the throat would affect combustion rates and thus, thrust. He then wrote an equation that related actual thrust to ideal thrust:

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222 By 1939, the group was conducting most of its tests in the *Arroyo Seco*. The fact that this March test occurred in the GALCIT building on the Caltech campus indicates that there were as yet no permanent facilities in the *Arroyo* where the men could work .

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$$C_f = \frac{F}{p_c A_v}$$

where C_f is the *thrust coefficient*, and p_c and A_v maintain the same definition as in the previous equation. The thrust coefficient is an important quantity, as it can be used to compute both a rocket's thrust and the nozzle's burn efficiency. [223]

Realizing that thrust was directly related to chamber pressure and volume, Summerfield then determined that the thrust coefficient was at its maximum when the ambient pressure and the chamber pressure were in fact equal, that is, under ideal conditions. [224] But such conditions are rarely sustainable in a rocket motor, and the thrust coefficient actually decreases from the ideal with different propellants. The specific heats of various propellants then become crucial to determining the actual thrust coefficient of a particular motor. The size of the combustion chamber, the angle of divergence of the nozzle (beyond the throat), and the type of fuel used in combustion were all important factors in calculating thrust. But which parameter was clearly the most significant?

If one increased the size of the combustion chamber and used a fuel with a higher specific heat, perhaps a higher thrust would result. In that case, should the nozzle angle also be increased or decreased? Some good sleuthing on Summerfield's part helped him solve the dilemma. As Clayton Koppes described the solution to this conundrum, Summerfield went

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223 See: Cornelisse, J.W., H. F. R. Schöyer, and K. F. Wakker. *Rocket Propulsion and Space Dynamics*. London, Pitman Publishing Ltd., 1979, pp. 116-117.

224 This pressure equilibrium, under ideal conditions, approximated pressure at sea level, where initial pressures surrounding and inside the chamber were equal.

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> rummaging through the Caltech library, turned up an old chemistry text that claimed a hydrocarbon fuel could be made to burn completely in a thousandth of a second in a small volume. He worked out elaborate calculations that suggested a small compact chamber would allow enough burning time to provide the desired thrust. [225]

And so, a smaller combustion chamber, rather than a large one, if designed in conjunction with just the correct nozzle angle of divergence, would result in maximum thrust, with less chance of excessive heat loss or explosion, as the propellant would be consumed much more efficiently. [226] This was a significant discovery, allowing the GALCIT team to build small rocket motors and to concentrate on the type of propellant used.

Summerfield's mind worked to integrate knowledge from physics, chemistry, and hardware design. He did not exclude exploration into possible new realms of knowledge simply because they were out of his field of expertise. What is more, he was not averse to working independently on a problem until he found the solution, whether or not others in the group knew of his efforts. His own comments on the discovery of the more efficient small combustion chamber are revealing. In his 1994 interview with Hunley, Summerfield recounted how he embarked on research into problems with the liquid propellant combustion chamber without Malina's knowledge, much less approval. Malina had instructed him, shortly after coming on board the project, to look into the problem of improving the performance of the liquid propellant motor test stand the crew had set up inside the GALCIT Laboratory.

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[225] Koppes (1982), p. 13.

[226] The nozzle's 'angle of divergence' measures from a horizontal center line bisecting the motor from the throat to the outer rim of the nozzle itself.

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> I examined what they were using, and I found it much too big. It was accounting for much of the heat transfer that was degrading the performance....Without his knowledge, without approval by Malina, I made a small combustion chamber. I adapted this small combustion chamber by filling up the entire space with a cylinder, except for a cone....And, lo and behold, the performance went up by 20 percent. [227]

It appears from his comments that Summerfield regarded Malina as responsible for delegating tasks within the group. Malina had asked him to join in 1940 and Summerfield saw him as unofficial leader of the project, under von Kármán. [228] Malina had administrative talents that he himself would have balked at admitting. Summerfield was new to the team in 1940 and had so far not proven his mettle with concrete accomplishments.

Nevertheless, as with Malina's decision to include Parsons, Forman, and later Tsien, the choice of Summerfield as liquid propellant researcher was a sound one. Summerfield had an uncanny knack for manipulating hardware and tools; he was evidently enjoying this puzzle-solving realm he had entered, and combined his craft skill with his new Ph.D. in theoretical physics and with a competent knowledge of chemistry. He may not have been the most socially adept member of the group, but he certainly was a talented scientist.

Summerfield's comment that he had not informed Malina of the details of his work suggests that by 1940, the dynamics of group interaction for the

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227 Hunley Interview with Summerfield (1994), pp. 3-4.

228 As Malina said himself, in his first memoir of the GALCIT Team's struggles in the years 1936-38, "I worked with von Kármán on many projects....In a way he became my second father. We worked so closely together during the formative years of the Jet Propulsion Laboratory, until he went to Washington in 1944, that it is not always possible to separate the contribution either of us made to technical and organizational developments during the period 1939-44." Malina (1967), p. 115.

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Rocket Team had altered somewhat. There was a greater sense of stability and accomplishment among the men; no longer were they struggling to survive or planning to write fanciful screenplays.

Undoubtedly, neither Parsons, Forman, nor Tsien regarded Malina with any sense of awe or saw him as *the* leader of the group. In 1940, Malina was not the only young engineering student to bask in the light of von Kármán's favors. The certainty of being among the anointed also accrued to Tsien. Von Kármán, with his special effervescence and magnanimity, looked after the welfare of several of his graduate students. Yet, Parsons and Forman were still on the periphery, for they had only tentatively deferred to Malina's insistence in attacking theoretical problems of rocket motor function before actually building a sounding rocket. [229]

Each individual in the group underwent an adjustment to working as part of a loose, but still very real team. Summerfield's experience, his awareness of Malina's informal authority within the group, represented no great departure from the norm of the Rocket Team's dynamics. He may have seen Malina as the group's leader, but no sooner was he given a challenging task to perform than he began working largely on his own. Summerfield took the initial results of his library investigations into combustion dynamics to Malina. Malina brought Summerfield's calculations to von Kármán, who "was impressed at [Summerfield's] research on combustion chamber speed." [230] In

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[229] In reality, however, the two men went on building and firing small black powder rockets throughout 1937 and 1938. They chafed under the academic restrictions Malina had seemingly imposed on them.

[230] Hunley Interview with Summerfield (1994), p. 4. Summerfield's own recounting of the story of his discovery is indicative of his determination to carry out research doggedly and systematically. "There were only a few books in the library having to do with combustion

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fact, it was not at all unusual to bring results to von Kármán, who would then modify them, suggest new avenues of analysis, or even reject them. Until Summerfield's data reached his desk in 1940, von Kármán actually believed the task of constructing a chamber without some sort of cooling mechanism would prove "impossible." [231] Initially, the GALCIT director believed no one would be able to build a self-sustaining lightweight liquid propellant rocket motor. After reviewing Summerfield's conclusions, von Kármán changed his mind and began to believe in the possibility of constructing such a motor. [232]

Summerfield's success in attacking the problem of heat dissipation from the motor and the creation of a more efficient combustion chamber gained him added respect within the group. Edward G. Crofut (d. 1968) and Walter B. Powell (1917-1994), who were hired in 1941, worked with Summerfield to construct new liquid propellant motors. Crofut was a technician, Powell, a young engineer. The three young men then became known as the "liquid propellant JATO unit group." [233] Powell played an important role in defining combustion dynamics. He authored JPL-GALCIT reports on the

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chambers," he recalled in 1994. "And one of them had the speed of combustion of a combustion chamber. And I thought that this was relevant. So I analyzed it and found that the combustion chamber was much too big, far too big. " Summerfield could not remember the exact title of the book he used, or its author's name, although he recalled that the author was 'English.'

231 Hunley Interview with Summerfield (1994), p. 5.

232 Hunley Interview with Summerfield (1994), p. 6.

233 Malina (1969), p. 179. According to JPL Archive Biographical Summaries on JPL employees, Crofut worked in the machine shop at GALCIT/JPL with Ed Forman, became Summerfield's assistant in 1941, and later left the laboratory for work with Allis - Chalmers. Malina hired Powell on September 15, 1941. Powell had just completed graduate work in aeronautics at Caltech and had been at Douglas Aircraft (where 'Amo' Smith worked). Powell assisted in the construction of liquid propellant JATOs and was present for Summerfield's 1942 tests of a hypergolic propellant.

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work of the 'liquid propellant JATO unit group,' including 1943 test results of copper-nozzle motors delivering 1000 lbs. of thrust and 1944 tests on underwater rocket firings. [234] The 1000 lb.-thrust motor was the largest constructed at GALCIT at that time.

Powell and Mark M. Mills (1917-1953), a young physicist who developed the theory of liquid propellant rocketry along with Summerfield in the 1940s, introduced the concept of *characteristic velocity*, c*, to the lore of rocketry. [235] The characteristic velocity is the exhaust velocity of the rocket motor measured independently of the effects of nozzle design. It is strictly a measure of propellant efficiency, dependent upon chamber temperature and the intrinsic properties of particular combustion products. [236] Since different propellants each have specific *characteristic velocities*, it is possible to analyze several propellants and choose the one most suited to a particular propulsion task, and one with the greatest thrust potential. Defining this quantity, then, marks a significant step forward in the development of liquid propellant rocket motors.

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[234] Powell, Walter B. 'Design and Test Results on an Uncooled 1000 lb.-Thrust Liquid Propellant Rocket Motor.' JPL-GALCIT Report No. 1-17, July 20, 1943. Powell also wrote JPL GALCIT Report No. 2-2, 'Installation and Operation of the Rocket Unit for Propulsion of the Hydrodynamic Tank Towing Carriage at the ACJP Project,' August 15, 1944. (Unpublished JPL Report). He co-authored, along with Summerfield and Crofut, JPL-GALCIT Report No. 1-13, 'Development of a Liquid Propellant Jet Unit and its Operation on an A-20 Airplane,' September 14, 1942. Pasadena, JPL Archives. There are unfortunately no known extant copies of the first report, No. 1-17. However, the Archives does hold a copy of JPL 1-13 and JPL/GALCIT No. 2-2. Their Collection No.'s. are 3-1011 and Microfilm # 614-245, respectively.

[235] Mills, who contributed significant work on the physics of rocketry, was killed tragically in a helicopter crash on Eniwetok atoll in 1953.

[236] Cornelisse, J.W. *et al.*(1979), pp. 118-120.

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Summerfield, on his own, further investigated the chemical kinetics of liquid propellant combustion, and determined, following preliminary data provided by Sänger, that the ratio between combustion chamber volume and the nozzle-throat area, symbolized by the quantity L^*, is of great importance to measuring combustion efficiency. [237] L^*, or characteristic length, is defined by the equation:

$$L^* = V_c / A_t,$$

where V_c is the chamber volume, and A_t the cross-sectional area of the combustion chamber. L^* is a factor which plays a role, as well, in combustion instability analysis; unstable combustion in liquid propellant motors was a problem that plagued GALCIT's early rocket motor designs. Summerfield's intuitive discovery that any lag time between fuel ignition in the combustion chamber and its combustion would eventually lead to an explosion of the chamber only partially resolved the issue. [238]

Summerfield, basing his discovery upon his own 1941 work in the area of combustion analysis, hypothesized that by "reducing the delay between ignition and the start of combustion to the absolute minimum," motor throbbing could be virtually eliminated. [239] Actually, there was a difference between virtually eliminated and completely eliminated. The problem of

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[237] Malina (1969), p. 162. Sänger first began to investigate problems of combustion chamber size in 1934, at the Vienna Technical University, and determined that "the propellant had to spend a certain minimum amount of time in the combustion chamber to run properly through the order of processes such as mixing and burning....The total period is primarily dependent on the construction of the combustion chamber, its 'characteristic' length, which is the ratio of the chamber volume and the narrowest cross-section area of the exhaust nozzle, i.e. the throat." L^* is thus *characteristic length*. See: Eugen Sänger. *Spaceflight: Countdown for the Future*. New York, McGraw-Hill, 1965, p. 85.

[238] Cornelisse, J.W. *et al.* (1979), p. 121.

[239] Koppes (1982), p. 14.

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unstable combustion with specific fuel combinations was to plague rocket motors for some years to come. Von Kármán, even in his final years in the 1960s, was still conducting research into the nature of combustion instability. True to his experience with turbulent fluid behavior, the old scientist suspected that turbulent gas behavior within the combustion chamber might be responsible for this instability. In 1969, Malina was to write that the "phenomenon of 'throbbing' has not been completely cured to the present day." [240]

How then could Summerfield have claimed to have virtually eliminated the problem, as early as 1942? Summerfield knew that for all intents and purposes, choosing the right propellants would suppress the tendency toward throbbing. But which propellants would accomplish the trick? This question was partially solved with the help of Navy Lieutenant Ray Stiff, chemical engineer and assistant to Robert Truax (1917-). Truax was then an ensign at Annapolis and was building rockets on his own. In February 1942, Malina took a train east to confer with Truax. While at the Naval Academy Malina listened to Ray Stiff describe two chemicals: red fuming nitric acid (an oxidizer and extremely toxic substance) and aniline (a combustible hydrocarbon), which would ignite spontaneously upon contact. Stiff suggested adding aniline to the fuel Malina and Summerfield were using in their motors.

Summerfield had been conducting experiments with red fuming nitric acid (RFNA) and gasoline since 1940. However, he had never been able to rid the

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240 Malina (1969), p. 167.

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motors of the throbbing phenomenon using this mixture. As Malina wrote in 1969, "the chief cause of the liquid propellant motor failures was improper ignition. Ignition was not instantaneous, propellant accumulated in the chamber and, if it then ignited, an explosive reaction took place." [241] Ray Stiff's suggestion that aniline should be added to the gasoline inspired Malina to *substitute* aniline for gasoline as the propellant.

On the return trip home, Malina's train stopped in Ohio long just enough for him to cable Summerfield with this exciting idea. Summerfield tried an actual demonstration. One day in February 1942, with Walter B. Powell standing at his side, Summerfield poured aniline into a small bowl filled with nitric acid and, standing back, watched as flames instantaneously shot into the air. Malina's visit with Truax and Stiff had definitely been worthwhile. Summerfield and Malina were elated. They had, or so they thought, solved the problem of unstable combustion. [242]

Malina's 1942 trip east to Annapolis reiterates an important truth about rocketry in the 1940s. Collaborative efforts worked to produce results. Not every rocket engineer had to follow in Goddard's footsteps. From its inception the GALCIT team had taken the tack of consultation, open

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241 Malina (1969), p. 178.

242 Malina went on to say in his memoirs that "The 'throbbing' months drew von Kármán, Summerfield, and the Sage Chemistry Group into a concerted theoretical and experimental attack on the problem. Von Kármán became so fascinated with the problems of liquid propellant combustion that he pursued them until his death in 1963; Summerfield worked on them throughout his life, as well. Von Kármán broadened the field to include thermodynamics, and Summerfield subsequently branched off to include the combustion process of solid propellants." Malina (1969), p. 179. The Sage Chemistry Group refers to a small team under Dr. Bruce Sage, Caltech chemist, who occasionally helped the Rocket Team carry out detailed chemical analyses.

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dialogue, and a search for informed advice. They sought out a network of experts to share their ideas and add to their fund of knowledge. The group started out as a tiny nucleus of graduate students and amateur experimenters. They worked in a university environment where expert technical advice was readily available. It was natural that collaboration should result.

Yet, because of Caltech's ambivalence toward rocket research in the late 1930s, the safety net below the GALCIT rocket group was a thin one. Von Kármán held it at the edges and the young researchers knew this. The constant awareness that their team's survival was never assured bound them together, despite differences in working style and fundamental disagreements over methodology and direction. At the dawn of their efforts, the composition of the group was fluid. Malina maintained a steady commitment, as his Ph.D. depended upon successful completion of the project. Parsons and Forman were as energetic and tireless in their rocket building in 1939 as they had been when they approached William Bollay early in 1936.

For all its energy, the GALCIT group was in a unique and sensitive predicament for almost four years after its inception: it existed unofficially and yet had a tangible set of goals, a dynamic, and tacit backing from an academic administrator high in the echelons of Caltech's faculty. It possessed no bureaucratic structure and had no appointed leaders as yet. Malina's crew relied partially on the 'trial and error' method, on insight, good fortune, and the early definition of a research plan.

There were two 'wild cards' at work. First, even in 1939, rocket theory was still a jigsaw puzzle only incompletely filled in. Clearly Tsien, Malina and

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Smith were working in a partial vacuum. The second was the behavior of Parsons and Forman. Whether or not these two young men had fully assessed the difficulties involved in rocket experimentation is open to question. From several years' experience working together, they had developed reasonable confidence in their abilities and had constructed many black powder rockets. Forman was an expert in welding and other practical aspects of constructing rocket casings.

Even so, there were technical limits on what could be achieved by rocket builders. Parsons and Forman were loathe to accept this reality. Parsons undoubtedly had some novel ideas in his mind as early as 1936 and 1937 and this may have been the source of his frustration with the team's chosen program. By 1938, he and Forman had built "a smokeless powder constant volume rocket motor similar to the one that had been used by Goddard." [243] The group tested several models of this motor, with fruitful results. They discovered that black-powder and smokeless powder impulse rockets did not work well. The goal was to construct a motor that would burn for longer than one second, utilizing a series of what were, in effect, controlled explosions.

Solid propellant rocket motors were then as unstable as liquid propellant motors. The propellant burned through unevenly and far too quickly. Parsons had solutions to these problems. First, he recommended that efforts be concentrated on restricted-burning propellants. Such powders would burn

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[243] Malina (1969), p. 169.

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only at the open surface of the propellant charge, rather than unevenly throughout the casing. They would thus provide constant thrust.

But how to devise such a propellant? Powders burned very fast and there was no way to control their burn rates once combustion had started. Parsons was an expert at building solid propellant motors. Somehow he knew that by putting a small hole in the center of the charge the powder would burn more evenly.

Actually, the idea of placing a central orifice in a black-powder rocket charge was quite old. Arrow-rockets of the Mongol era (1200s) had probably not contained any center orifice, as they burned rapidly over short distances; but a certain Johann Schmidlap of Nuremberg, Germany had published a work on non-military fireworks in 1591 which described the construction of a wooden tube filled with a pasty gunpowder-charcoal mixture. This powder was hammered into the tube around a 'thorn,' so that the entire mixture surrounded a central hole, usually conical in shape. According to Willy Ley "the purpose of this center hole was to provide a large burning surface for the powder so that a large quantity of gases would be generated." [244]

Whether or not Parsons actually knew of Schmidlap and his thin volume on rocketry cannot be known. He may well have heard of Ley's work with the *VfR* in the late 1920s and early 1930s, although he could not speak or read German. But by then, forcing a center-hole in a powder rocket had become common practice. In effect, one created a small internal nozzle by punching a hole in the powder.

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244 Ley, Willy. *Rockets, Missiles, and Space Travel.* New York, Viking Press, 1958, pp. 58-60.

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Both Parsons and Malina knew that this conical hole in the center of the charge kept the charge from burning along the sides of the container and exploding. However, the problem was that the charge usually did not burn for longer than one second. In 1939, Parsons began experimenting with various other mixtures of black and smokeless powder. He worked with metal chambers of 1 and 3 inch diameter. He bought 6 inch long pellets of powder that were pressed into the chamber at about 6,500 p.s.i. and "coated with various substances to form a solid or liquid seal between the charge and the walls of the chamber." [245] Almost every one of these motors exploded shortly after ignition. Was there something desperately wrong with Parsons' design? Was a restricted-burning motor inherently unstable?

Malina and von Kármán went to work and completed a theoretical study of the 'ideal solid propellant rocket motor.' [246] Satisfied, following the conclusion of this paper, that the process was stable, the team hunted for mechanical causes of the failures. Parsons' hunches had been right. A slow-burning solid propellant could work. In this case, theory followed practice, and Parsons managed to push forward the limits of knowledge by insisting on continuing his experiments.

Parsons' obstinacy and the hint that a tug-of-war had early ensued over the focus of research within the GALCIT Rocket Team indicate that the plan of action devised by the team was only loosely sketched out in 1936 and 1937.

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245 Malina (1969), p. 169.

246 Theodore von Kármán and Frank Malina. 'Characteristics of the Ideal Solid Propellant Rocket Motor.' JPL/California Institute of Technology Report No. 1-4. In von Kármán, *Collected Works, Vol. IV* (1956), pp. 94-106.

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There was no clear organizational structure for the group, and the direction work took depended very much upon the success or failure of experimentation. Parsons and Forman had, of course, no objection to this course: they had been pressing for more rocket motor testing all along.

If Malina and von Kármán had insisted upon shaping the rocket program at GALCIT strictly as a graduate engineering endeavor, then theoretical papers would clearly have become the chief product. But including Parsons and Forman at the inception meant fundamentally altering the group's methods. Malina did not realize at first how incorporating these two men into the group would change the playing field. After all, rocketry at Caltech was too new a technology to risk excluding an expert, no matter what his background. The chances for failure were just too high. So Malina shrugged his shoulders and accepted Forman and Parsons' independent work, concluding there was nothing he could do to halt it anyway. But he was not happy about it. In 1969 he commented that the model tests of Parsons and Forman were "doomed to be disappointing and made support [for the GALCIT Project] even more difficult to obtain." [247]

What is striking is that, once government funding began to roll in after 1938, Parsons was specifically delegated to work on solid propellant motors and the theoretical support began to materialize. Not merely success, but struggle make for strange bedfellows. Actually, Malina's assessment of the effects of Parsons' actions reveals his frustration at his own inability to control Parsons and Forman. Funding for the project was virtually non-existent before the

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[247] Malina, Frank. 'The Rocket Pioneers: Memoirs of the Infant Days of Rocketry at Caltech.' *Engineering and Science* #31, February 1969, p. 10.

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end of 1938, when the National Academy of Sciences finally agreed to provide the first $1000 for continued research. [248]

Why, then, did the group hold together, when Parsons insisted on going his own way and success was limited or even non-existent, in late 1936 and early 1937? It was not desperate necessity which brought two amateur rocketeers to Caltech, where they remained for nearly ten years, never matriculating. Nor was Malina's or von Kármán's patience with their difficult behavior inexhaustible. Not all the credit can go to the GALCIT director's foresight and faith in the abilities of his students. Both Forman and Parsons were remarkably competent craftsmen and innovators; rocketry at GALCIT needed just such men as these. The barometer of success at GALCIT was measured, not in production of scholarly papers, but in a crucial spirit, a willingness to accept challenge and risk.

A key to the mystery at the heart of the GALCIT team lies in exploring the way in which Edward Forman, the man least likely to join a rocket engineering team at Caltech, became an important contributor. Ed Forman's story illustrates the unusual nature of the group's working dynamics. At first glance, it would appear that Forman was quite unlike others on the team. He did not possess Parsons' knowledge of chemistry—he had not even progressed beyond the junior college level of mathematics.

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[248] The NAS granted this support to the GALCIT Rocket Group as a result of Malina's presentation of his 'Report on Jet Propulsion for the National Academy of Sciences Committee on Air Corps Research,' December 28, 1938. It is likely that the presence of von Kármán and General 'Hap' Arnold on the committee influenced the decision to grant funding.

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Forman attended Pasadena Junior College in the early 1930s, but dropped out and worked as a carpenter, chauffeur, chemical plant worker, and mail deliverer for the Post Office. Venturing his mechanical talent in the aviation world, Forman also riveted sheet metal for Lockheed Aircraft and helped build airplanes. He even managed an explosives plant. At Caltech Forman served as a draughtsman, drawing up working plans for the GALCIT team's early liquid propellant motors, and he was generally relied upon to do the fine machine work in constructing cases for the rocket motors the group was building.

It was Forman who played a key role in determining a site for future rocket tests the team intended to carry out in the *Arroyo Seco* in 1940, when it seemed that the area they had used since 1936 would no longer suit the size of their operations. He scouted the *Arroyo,* convinced the Pasadena City Council to lease three acres to the Jet Propulsion Laboratory, and borrowed a city-owned bulldozer to clear the site.

Forman also wrote several papers—at least one of them published—along with Parsons, Malina, and Summerfield, on the design and development of solid and liquid-propellant motors. [249] In the *GALCIT Project Final Report for 1939-1940,* Forman likely wrote in the construction details of solid propellant motors (although since the report is a collaboration, this cannot be confirmed.) [250] It is also possible that Forman completed some of the design

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[249] The published work was written in collaboration with Parsons: 'Experiments with Powder Motors for Rocket Propulsion by Successive Impulses.' *Astronautics,* No. 43, August 1939.

[250] Malina, Parsons, and Forman. 'GALCIT Final Report for 1939-40.' Report No. 3, June 15, 1940. See: pp. 64-65 and p. 78 for construction details likely written by Forman. Library of Congress, Malina Collection, Box 9.7.

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draughts and detailed plans of the rocket motors in the report. However, as Malina had also illustrated an engineering textbook, *Mathematical Methods in Engineering*, for von Kármán and his colleague Maurice Biot in 1937, Forman's actual contributions to the report may never be known.

Forman is one of the least revealing members of the original GALCIT team. This is not an insignificant issue, considering how oblique Malina could be in describing problems among the group members and the total silence of Tsien on his own relations with the rest of the team. Forman was apparently bitter at the turn of fate he experienced after the GALCIT crew formed the Aerojet Company in 1942. Aerojet was eventually bought out by the General Tire Corporation and several of the founding members of Aerojet succumbed to pressure and sold their stock to General Tire. Forman gave up his shares and when the new company, Aerojet-General, became very successful, Forman felt he had been cheated, as the stock's value soared. But he was not the only loser; von Kármán also sold his stake in Aerojet.

Off-duty, Forman associated very little with the Malinas, even though he lived close to them. There didn't seem to be a felicitous conjunction of personalities. Forman may have felt 'the odd man out' on the team, since he had not completed college; then again, neither had Parsons. Parsons' facility for creative solutions to knotty problems—devising answers which neatly solved theoretical dilemmas Malina happened to be puzzling over—distinguished him from his partner. But no rocket enterprise ever succeeded without a highly skilled mechanic.

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In a 1995 interview, Jeanne Forman (Forman's second ex-wife) alleged that Malina treated Forman condescendingly, as he was not university educated. Yet, there is no evidence of arrogance in Malina's tone toward Forman to be found in the scientist's memoirs. Nor does such a claim fit Frank Malina's character. Whatever his level of comfort with Forman and Parsons, Malina accepted both men as full members of the Rocket Team and undoubtedly valued their expertise. [251]

What makes Edward Forman, then, a symbol of the type of worker gravitating toward the GALCIT Rocket Research Project? Like many other members of the team, Forman drifted in and out of active participation in the group. He eased himself gradually into the team, which did not take undue notice of his talents at the start. At first, Forman was just John Parsons' friend and partner. His skills were not immediately evident to Malina (as were Tsien's); more to the point, everyone on the team had to prove his own worth.

In the early days (the fall and winter of 1936-37) when Malina, Parsons, 'Amo Smith, and Rudolph Schott (1914-) were busy setting up and test-firing liquid propellant motors in the *Arroyo Seco*, Forman was nowhere to be found. [252] He did not attend these tests at all. He and Parsons continued to build and fire off solid propellant skyrockets, hoping to be recognized publicly for their

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[251] Interview with Mrs. Jeanne Forman, conducted by Brad Branson and Susan Pile, July 15, 1995. JPL Oral History Archives (alphabetically listed under Forman.) Mrs. Forman commented: "Ed had nothing against Malina but Malina had something, I feel, against Ed. Here were these dumb people who were getting honors. How dare they, when they hadn't gone to university ? Jack and Ed couldn't stand to be held down by going to college."

[252] Rudolph Schott was present at some of the early (1936) rocket motor tests in the *Arroyo Seco*, but had relatively little involvement with the team afterwards. He left rocket work completely after 1937.

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efforts. (They eventually were, in the *Pasadena Star News.)* The early GALCIT rocket motors were designed without Forman's input. Smith and Malina both had enough design sense and engineering expertise to design and build (or have built at Caltech) the first test-firing stand. The story of how they scrounged around junkyards in Pasadena for test apparatus, gauges, etc. is part of the lore of the GALCIT team's early days.

By 1938, Forman was much on the scene and in several photographs of the more complicated liquid propellant test stands, his handiwork in the construction of piping, tubing, and welding of crucial parts of the rocket motors is in evidence. In a 1939 article published in *Astronautics*, the organ of the American Rocket Society, Parsons and Forman describe the experimental test-stands they constructed for their 1938 solid propellant motor tests.

> Two motors...were used in the experiments; one small, having a one-half inch diameter combustion chamber and two interchangeable nozzles having a nozzle angle of six degrees, of 3/16" and 1/8" throat respectively, and constructed of chrome-molybdenum steel. The larger motor had a combustion chamber of 1" diameter and a fixed nozzle of 1/4" orifice and a 9 and 1/2 inch degree nozzle angle, constructed of Ludlum Seminole steel treated to give a tensile strength of 130,000 pounds per square inch. [253]

The building and welding of these motors must have taken considerable effort and skill, involving lathe work, arc or gas welding, and a knowledge of the characteristics of different quality steel. Where Parsons and Forman obtained this steel is unknown. Nor is it clear how they paid for the raw

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[253] Parsons, John W. and Edward S. Forman. 'Experiments with Powder Motors for Rocket Propulsion by Successive Impulses.' *Astronautics*, No. 43, August 1939. pp. 5-6.

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materials. It is possible that the funds for these projects came out of their own pockets, with the tacit approval of Malina and von Kármán. After all, Weld Arnold had supplied an infusion of $1000 in 1937, but no other funds were to become available until the National Academy of Sciences grant late the next year. Nearly everything the GALCIT Team bought was an 'out-of-pocket expense' in those days. Since Forman and Parsons had worked with explosives companies in the mid-1930s, they knew how to obtain black and smokeless powder. The explosive chemicals came from the Hercules Powder Company of Pinole, California (and perhaps even from sporting goods stores.) [254] Parsons and Forman also designed and built the motor test-stands with two interchangeable springs, measured their spring-constants and even improved their design following initial experimentation. [255]

In designing their research machinery, Parsons and Forman had clear objectives. By 1938, they had already amassed considerable experience building (and exploding) powder rockets. They defined their experimental goals as: determining the thermal efficiency (efficiency of burn) of specific rocket powders, measuring the effects of internal chamber pressure on thermal efficiency, discovering new methods of varying motor chamber pressure, designing more efficient chambers and nozzles, and testing a variety of methods of ignition and the physical state of the powder in the rocket chamber.

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[254] The types of powders were: Hercules/Laflin and Rand FFG black sorting powder, with a theoretical exhaust velocity of 7,000 feet/second, and Hercules Bullseye Dust smokeless powder, with an ideal velocity of 10,600 feet/second. Parsons' contact at the Hercules Powder Company was Henry N. Marsh. See: Parsons and Forman (1939), p. 6.

[255] Parsons and Forman (1939), p. 6.

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Their results were clear and significant: the velocity and efficiency of a particular powder were increased by shrinking the nozzle throat diameter, by increasing the weight (and thus mass) of the powder, and by the use of wadding around the interior of the chamber and in the center of the charge. In their own words, the rocket motor's efficiency was increased "by the factors which tend to increase the chamber pressure." [256] Neither Parsons nor Forman made any claims to have definitively measured the experimental elements under study. They assumed that actual effective exhaust velocities were probably somewhat lower than data would indicate. But their conclusions are scientifically sound.

However, they failed to mention in their article a significant problem encountered in solid propellant impulse motors. Primarily, this type of rocket motor tends to explode after its initial burn. In the summer and fall of 1939, von Kármán, listening through the window of his GALCIT office, heard many such explosions outside the campus building as Parsons and Forman continued their experiments. Eventually, the two men switched to building restricted-burning powder motors—those that burned only at the aft surface, much as a cigarette burns. [257]

This change in strategy did not end their problems because such motors *also* exploded. Malina explained in his 1969 memoirs the general expert belief in the 1930s: "The combustion chamber pressure of a restricted burning solid

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[256] Parsons and Forman (1939), p. 8.

[257] In uneven burning , propellant burns into its own core and around the sides of the casing, generally resulting in an explosion or burn-through of the rocket.

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rocket unit would continue to rise from the moment of ignition until any combustion chamber of reasonable weight would burst. In other words, it was thought that any such combustion was inherently unstable." [258]
In time GALCIT's director became so exasperated by the continual explosions that he sat down one day in the Spring of 1940, wrote out four non-linear equations, and asked Malina to prove them that very same night. [259] Malina did so. Forman and Parsons' persistence, risking even the ire of GALCIT's venerable director, bore significant fruit.

The equations proved that *theoretically*, "a restricted burning unit would maintain a constant chamber pressure as long as the ratio of the area of the throat of the exhaust nozzle to the burning area of the propellant charge remained constant." [260] This bit of knowledge, of course, presented Forman and Parsons' insight into the importance of nozzle throat diameter in a new and more complex light. It was not merely the size of the throat that was crucial to a successful burn, it was more specifically the *ratio* of the nozzle throat's diameter to the actual amount of propellant burning that made the difference. Pressure inside the combustion chamber which built up without limit would ultimately end in an explosion. The limitations of Parsons' and Forman's work is apparent. They could plumb the depths of rocket dynamics

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[258] Malina (1969), p. 163.

[259] The truth is that by the spring of 1940, when von Kármán wrote out his four complicated equations, he could no longer hear the explosions; following dangerous failures inside and just outside the Guggenheim Laboratory building the previous year, he banished the group to the *Arroyo Seco*. Still, the continued failures rankled him and he decided to intervene personally in the discovery process.

[260] Malina (1969), p. 163. The equations are written out in Malina's hand on p. 166 of his memoirs. Eventually the Team discovered that by widening the dimensions of the nozzle throat (still maintaining the proper throat to chamber capacity ratio), the throbbing which plagued many of these early motors could be lessened, if not eliminated.

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only so far without more complex mathematics. Still, their insights, built upon a wealth of practical experience, were not to be underestimated.

Moreover, Forman's mechanical and drafting skills were essential to key discoveries made by the Rocket Team. The extent to which he fully understood the physics of the experiments he was helping to conduct is debatable, but his skills were indispensable. Forman welded rocket test stands and may have also built gaseous propellant motors at GALCIT in 1938 and 1939. It is probable that 'Amo' Smith and Professor E. E. Sechler of Caltech actually designed the rocket motors for these tests. Did Forman work alone in the *Arroyo Seco*, constructing rocket test set-ups for GALCIT without the assistance of Malina or even Parsons? Undoubtedly not, for the test mechanism appears to have been relatively complex.

> The oxygen cylinders were suspended in a cradle hung at one end of a beam and balanced by a counterweight and tension coil spring at the other end. The beam was free to see-saw on knife edges located as close to the tank cradle as possible to reduce the amount of counterweight. The free motion of the beam was restrained by a coil-spring under tension and was damped by a viscous damper. The unbalance of the beam due to the loss of gas in the tanks was measured by a dial gage *[sic]* operated by an extension arm attached to the spring-suspended end of the beam. The dial gage reading was calibrated to give the weight of the gas used during a test. A similar scale was used to determine the amount of ethylene used. [261]

This test stand was fairly large, judging from photographs, and ran successfully for several burns. The pressure of oxygen and ethylene injected into the combustion chamber was regulated manually; while the motor ran, a

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261 Malina, Forman, and, Parsons (1939), pp. 37-38.

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still camera took photographs of the rocket motor every one to three seconds. Tests lasted at least 30 seconds—rather long for an uncooled gaseous (or liquid) propellant burn; from start to finish, an entire run could last 90 seconds. This was a far cry from initial liquid propellant tests the Rocket Team held in 1936 and 1937, where an uncooled motor burn of 4 seconds was considered a resounding success.

Forman's role, then, appears not nearly as enigmatic or peripheral as a first glance might suggest. Rather, it confirms the hypothesis that collective effort is what sustained the GALCIT team, despite the individual discoveries, experiments, and the failures of the early years. Forman was a member of the GALCIT Rocket Team, not only because he was John Parson's inseparable friend, but because he had a valuable skill to offer the group. Certainly, Hsueh-shen Tsien would not have gone out of his way to befriend him, but Malina and Summerfield treated him as an important member of the team. This does not imply that Malina fraternized with Forman, only that the two men developed a working relationship.

Both Malina and Summerfield, fresh with their Ph.D.s in 1940, bridged the gap between the theoretician and the hands-on technician with relative ease. Despite individual perspectives, an egalitarian tenor suffused the work among most of the team members. Each man had a specific task—in fact, several tasks—to perform, and all relied on the strenuous efforts of others to unravel the secrets of the rocket motor.

Of course, subterranean tensions persisted within the group. Some of the men involved in the late 1930s and 1940s, Clark Millikan and Tsien,

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particularly, were thoroughly Caltech, university trained and bred. Although the composition of the Rocket Team took little account of the traditional boundaries of social class, in the main, the men respected the hierarchy of authority at the university. Von Kármán, as GALCIT's director, world-renowned engineer, held sway over the group's activities. But there was little micro-management from above, except when experimental or theoretical insights could be had, or when the lab building actually suffered from the explosions of rocket motors gone awry.

At Caltech, despite the growing prestige of the institution and the fame of its scientists, a warm sunny climate and palm-studded campus contributed to the chiseling down of social barriers. Yet, it was the hard work facing the team—disappointments, challenges, doldrums, and scant initial success—which propelled these men to work without regard to differences in their educational and social background. They recognized each other's limitations. They prized the intellectual feats of Tsien and Malina as these two developed new insights into the rocket motor's functioning. But practicality ruled. There was little money and little time to dwell on social distinctions. A degree mattered personally, but did not elevate one man above another in pursuing the common goal.

The early years of the GALCIT Rocket Team were years of *sturm und drang,* but they were also energetic and fruitful years. A collective consciousness began to grow: the team's work began to take on a life of its own. Even when the group seemed on the verge of disintegration in mid-1938, when Malina went off to work with von Kármán, studying (in the GALCIT wind tunnel) the effects of wind erosion on sand dunes, when Smith left the group for

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Douglas Aircraft and Weld Arnold returned to New York, when Tsien decided to concentrate on finishing his Ph.D., there still was a sense that they all had begun something dramatic that was to profoundly affect their lives.

Military interest in rocketry was about to revive the group. The years ahead, particularly the early 1940s, were to be a period of intense activity and accomplishment for the GALCIT Rocket Team. This era of discovery would not have blossomed if the ethos of work toward a common goal on the very edge of established science had not burned within them. The work they had embarked upon was new uncharted territory. Despite the centrifugal forces tugging at them all, the quest never truly ended for these young men.

In 1938, when Parsons and Forman took jobs with the Halifax Powder Company, they actually cemented a link that was to hold them in good stead in the next decade. They continued to fire black powder rockets with low trajectories. [262] Malina sensed the essential importance of the bond he and Parsons had developed. Tsien, trenchant mathematician and theorist, already gravitated toward work in high speed aerodynamics. [263] His interest in rocketry was not likely to flag, so long as he remained at Caltech. Smith had gone, as had Bollay, Weld, and Rudolph Schott.

By 1939, a metamorphosis in the size of the group, its internal dynamics, the human energy devoted to it, and the organizational commitment to rocketry

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[262] Koppes (1982), p. 8.

[263] Perhaps taking his cue from von Kármán, Tsien's interest in the theory of compressible fluids grew, and he was to devote much of his early career to its study (including his Ph.D. thesis.) Compressible fluids such as air develop remarkable properties (as Prandtl had discovered early in the Twentieth Century) when under high pressure or when moving at high velocity.

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at Caltech was set to begin. The creative core of the group had not diminished, only shifted its object. This spark was to ignite again once a new infusion of funds flowed in. The GALCIT Rocket Team was about to grow and solidify its accomplishments. It had established a base of theoretical competency, made some promising discoveries in the realm of rocket propulsion dynamics, and achieved small success, not to be underestimated in 1939 America, in firing test motors. A remarkable period of deeper discovery was waiting around the corner. Even so, the tension between the experimental and the theoretical refused to fade away.

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Chapter 7
The Logic of Investigation in Rocket Research: On the Threshold of a New Science

The rocket motor is a unique and peculiar machine. With a steel or aluminum casing, a heat-resistant combustion chamber, a nozzle honed of high-temperature absorptive material such as graphite, or pure copper, which can resist melting to the limit of 6000° F, it is built to withstand extreme operating conditions. Gas pressures within its firing chamber are enormous. It operates on the equal-and-opposite reaction principle, derived from that most familiar of Newton's *dicta*, the Third Law of Motion. Its essential principle is simple, yet the process of moving forward at high speed by shooting a jet of extremely hot gases and flame from the aft end of a nozzle still conjures up high drama. The turbofan jet engine, unlike the rocket, ingests air and forces it out under higher pressure and at high velocity.

The rocket motor, furthermore, can boost a payload to speeds unattainable by any contrivance operating within the limits of the atmosphere. Theoretically, in the vacuum of deep space, it could approach the speed of light. The rocket is not bound to the realm of the earth; such is its ability to stretch the limits of our imagination that its systematic development in the twentieth century marks a qualitatively new stage in human history. And yet, it may be constructed of the most mundane materials: in the case of the liquid propellant motor, pipes and tubing made of steel, aluminum, or copper; pumps, wiring, perhaps a radio servo-mechanism, and gears. Not the stuff of which fantasies of travel to the *Alpha Centauri* system are made. [264]

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[264] *Alpha Centauri* is the nearest star system to our solar system and consists of three stars,

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Rocket motor design and construction involves the study of the process which propels a working rocket. Although its internal works may look like a maze of densely packed and intertwined tubing, it is the research into the functioning of the rocket motor and the choice of design materials that is most complicated. This research, in the jargon of the test pilot, 'pushes the envelope,' or tests the design to its operational limits.

Rocket development follows a standard model of experimentation and depends upon simultaneous efforts and expertise in several fields. It would seem that a fairly long period of theoretical investigation would necessarily precede the actual building of a rocket, but this is seldom the case. The truth is that rocket engineers are out at the tests site as soon as they possibly can be, and are invariably refining theoretical expectations in the light of experimental results. Since the modern rocket motor is a product of systems engineering, several different design specifications must fit seamlessly together and various components must work flawlessly before the motor can be successfully tested.

The field of rocketry delves so much into the unknown that experimenters are left to evolve a strategy of work which depends upon the merging of several different lines of investigation. Of course, rocket engineers and experimenters prioritize, as do other scientists. The GALCIT Rocket Research Group decided to concentrate on problems of rocket propulsion, rather than on the design of a complete rocket, at least in the early days of the late 1930s

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two of which are binary (that is, they revolve around each other.) The third member of the system is *Proxima Centauri*, which is actually closer to the earth than its binary neighbors. The *Alpha Centauri* system is about 4.3 light years from earth.

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and into the 1940s. Their choice was not made in a cavalier manner, nor without resistance from Parsons and Forman. But both Bollay and Malina (and ultimately, von Kármán) insisted that it made little sense to build a rocket if one were unable to get it off the ground. The dynamics of rocket propulsion took first priority. However, the actual course of research at GALCIT depended upon the proclivities of individual researchers, their training, the difficulties encountered with each type of propellant—and on the demands of a government in need of military rockets.

With so many diverse influences tugging at the men on the team, it was imperative to have a plan of action and try to adhere to it. Of course, Forman and Parsons insisted, as part of the plan, on continuing the same pattern of experimentation that had originally brought them to Caltech. But without specific input from researchers who could model projected performance, both of propellants and the rocket casing, Parsons' experimentation would have led inevitably to failure after failure. Defining theoretical parameters before starting to work seems a reasonable course of action.

At issue is: how should a rocket engineer shape his machine? Design surely must surely facilitate the rocket motor's work or it is useless as a tool of research. Of course, scientific models that do not depict contemporary reality in some meaningful way may still serve as inspiration for future discovery. There is ample evidence in the history of science of inventors and visionaries who created and sketched new projects before their time. But the model of an experiment one is likely to perform, particularly one involving the direct conversion of heat energy into energy of motion, should illuminate the steps in the conversion process. In the case of a rocket motor, where the theoretics

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are based upon known principles of chemistry, thermodynamics and stress characteristics of materials, the experimental design should also be straightforward.

And so it was for early GALCIT motors. The liquid propellant motor was encased in a simple cold-rolled steel chamber, with a straight cylindrical nozzle attached. In 1938, after Apollo Smith had performed tests on a variety of materials, the chamber was lined with graphite to provide better insulation, and solid copper replaced steel in the exhaust nozzle. Graphite and copper are more heat-resistant than steel. There was nothing complicated about these motors, although the support apparatus, consisting of fuel and oxidizer tanks, valves and pressure gauges, proved the weak link in the chain: valves often blew, lines leading to the combustion chamber ruptured, usually long before the chamber or nozzle melted away. The GALCIT crew's solid fuel motors were equally unsophisticated: the combustion chamber "was a Shelby steel tube, three inches inside diameter, four inches outside diameter, and twenty four inches long. One end was threaded on the inside to contain a pipe plug, and the outside of the other end of the tube was threaded to fit an eight inch pipe flange for attaching the exhaust nozzle." [265]

Yet, even this simple design had the ingredient of unreliability imbedded within it. Mechanical failure and poor propellant mixture could abruptly halt the motor's operation. Rocket motors are not simple machines, after all. Their components may all be analyzed separately, but this does not imply that

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265 Malina, Parsons, and Forman. *Air Corps Jet Propulsion Research* 'GALCIT Project No. 1, Report No. 3, Final Report for 1939,-40.' p. 77. JPL Archives, File # HC3-1206.

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when functioning as a system, the myriad components will work smoothly. There is always a weak link in the system.

The first GALCIT liquid propellant motors were set up behind protective sandbag barriers. [266] The chamber and nozzle were positioned so that the exhaust flame shot straight up into the air, while the reaction force was measured on a small glass cylinder to which a diamond point was attached. As the thrust quickly increased, a spring mechanism would depress in direct proportion to the degree of thrust generated. The fuel flowed in under high pressure, as did gaseous oxygen, from opposite sides of the test stand. The firing chamber sat in a water-cooled container vessel. Gas pressure inside the combustion chamber could be gauged visually by one of the crew, crouched behind the sand bags. (Early firings had no spark plug at the nozzle head. Parsons had literally to stand near the nozzle and light the mixture with a match.) The test apparatus usually broke down precisely where it should have been expected to: at the connecting joints between separate supply tubes, in valve seals, and, clearly, at the point of ignition.

Solid propellant motors usually had a tapered nozzle just aft of the propellant casing. The propellant was initially black powder, and later, a sulfate-charcoal-amide mixture. Even the innovative new propellant, GALCIT- 53, a combination of asphalt and potassium perchlorate, had a simple housing. The rocket crew designed the tail of this unit with shear bolts. In the event of over-pressure in the nozzle or combustion chamber, the entire nozzle compartment would blow off, immediately reducing exhaust pressure. The

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[266] A design sketch of the 1936 motors appears in Appendix I, at the end of this dissertation.

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design was a bit awkward at first, but evolved according to the demands of simplicity and practicality. The whole purpose was meant to reduce mechanical distractions to a minimum. The test apparatus was designed to provide data on thrust, burning time of the propellant, and the rate of propellant consumption. [267] The team did not want to spend an inordinate amount of time tinkering with the motor, but wished to concentrate on measuring its performance.

And yet, tinker is exactly what they had to do. Early JATO units tended to explode. In 1941, the team tried to test black powder JATOs stored overnight. The idea was to impress the US Navy with the JATO's reliability as a booster rocket for carrier-based aircraft. But consistent performance was not one of the early JATO's strong points. One even tore a hole in the fuselage of the single engine *Ercoupe* trainer on which it had been installed. Luckily, this happened on the ground. On inspection, one of the men dryly remarked, "Well, at least it isn't a big hole." [268] In a second test, an explosion occurred in flight. On the ground, all Malina and Parsons could hear was a muffled bang as the plane flew overhead. The pilot, First Lieutenant Homer Boushey, managed to complete his flight and return to the airstrip safely. Eventually, the team got it right. But not without travail. Malina assumed that, once he had shed light on the theoretics of the motor's operation, the construction of working motors would follow without too many glitches. This was not to be.

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[267] Frank Malina, H. S. Tsien, John Parsons, *et al. Report of the GALCIT Rocket Research Project* . 'Characteristics of a Rocket Motor Utilizing a Slow Burning Solid Propellant.' (Parsons and Malina.) Library of Congress, Malina Collection, Box # 9.4., p. 86.

[268] Malina (1969), p. 170. See also, Koppes, p. 11.

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New problems, like new headlands just around a bend in the coastline, kept appearing.

A little more than a year earlier, in the spring of 1940, Malina solved the four complex equations on an ideal restricted burning solid propellant motor von Kármán had given him. [269] "Much to our relief, " he wrote, "we found that, theoretically, a restricted burning unit would maintain a constant chamber pressure as long as the ratio of the area of the throat of the exhaust nozzle to the burning area of the propellant charge remained constant....Experimental verification of the theory was soon obtained. [It has been shown that the theory is correct provided the chamber frequency is low]" [270] Constant chamber pressure, Malina had theoretically predicted, was crucial to a smooth combustion process. For, if pressure inside the combustion chamber fluctuated, that surely indicated the solid propellant was not burning evenly. However, for larger combustion chambers, gas pressure could fluctuate. The motor then began to pulse and, if not quickly shut down, explode.

Despite such vexing problems as unstable chamber pressures, Malina believed he was on the right track. As the GALCIT team coalesced, he had insisted on focusing its efforts. Malina wanted to hone down the system and determine its physical and chemical characteristics carefully prior to conducting

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[269] For further discussion of these equations, see Chapter 10 of this thesis. There is some question of the completeness of Malina's solution of the four crucial equations. Since they are non-linear equations, they are not at all straightforward in their solution.

[270] Malina (1969), pp. 163-164. What Malina meant by the *chamber frequency* is the ratio between the chamber volume and the diameter of the nozzle throat. The ratio, known symbolically as L^*, gave a hint that for small motors at least, stable burn could be achieved if the right balance between the two quantities could be achieved. For larger motors, the factor is overshadowed by other problems that set in.

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experiments. Intensely focused on building a working motor, Malina and the crew looked at the frequent explosions and technical mishaps as hurdles to be overcome. Mathematical approximations of motor operation were necessary factors in the design equation.

However, it was precisely these technical stalls and mathematical idealizations that were to hint at deeper theoretical problems, incompletely understood. The GALCIT engineers had to study the rocket motor as if it were a model operating in a restricted field without any extraneous factors impinging upon it. To study one or more specific characteristics of a system, such as propellant burn rates, Malina calculated the heat efficiency of a particular type of fuel as if it were in a vacuum, not subject to influence of altitude, ambient air pressure, composition impurities, etc.

Traditionally, isolating part of a system and measuring a specific aspect of function would reveal some essential property or a previously undiscovered truth about the system. The model's design in part determined the measurement of expected results, but the raw experimental data provided tangible proof of the shape of underlying phenomena. If the data were consistent and sufficient, then their interpretation would flow logically from an accurate reading.

If an experimenter imposes a design on an experiment, expecting the physical rules of behavior to fit known models, and discovers unexpected results, then perhaps the theoretical framework under-girding his model needs changing. Investigating phenomena on the very edge of the known often forces an explorer into an entirely new theoretical framework. And, if the

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mathematical tools at hand are inadequate to the task, or the phenomena not well understood, then certain key problems may be overlooked or ignored. Experimental design pre-fashions the limits, not only of the known, but of the unknown; what is not yet illuminated takes on tentative substance and shape. In a sense, the experimenter has created a shadow image and projected it into the darkness ahead.

Preconception of a system's structure is at the heart of research. Engineers, having an idea of what they wish to construct, design their experimental apparatus with the technology available to them, and with an image in mind of the system's function. Every model is a product of its time and of the expectations of its creator. The accepted language of experimental discourse must influence the experiment. Even in an experimental design deemed 'elegant' and innovative, research proceeds in the light of preconceived expectations.

Still, the system may defy predictability. The more complex the system under study, the more complex the experimental apparatus, the greater the number of parameters involved. A complex system, operating as a unit, may not function as if all its components parts were working independently of each other. There is an added element of coordinated functioning to be considered. It is this coordination of diverse elements of the experimental apparatus, sub-units of the larger structure, that bring the entire experiment to the brink of unstable, even unpredictable behavior. Real systems confound the hopes of the inventor. The dynamic core of a mechanical or chemical system manifests a shifting balance between order and instability. There is no predicting exactly when disorder will dominate the system.

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Rocket propellant combustion often crosses a threshold into chaotic behavior. To visualize the onset of non-periodic behavior in a rocket motor, the dynamics of combustion may be defined as a kind of energy information system. The temperature and pressure relations among its molecules are forms of information exchange among the chemical components of the system. Chemistry involves alteration of matter, sometimes spontaneously generated, sometimes engineered.

Chemical and physical systems are constantly in process of exchanging internal information about the shape, combination, and potential for transformation their atoms possess. When combustion becomes unstable, interaction among the propellant molecules or between the propellant and chamber wall does not proceed according to theoretical expectations. This is only an indication that some deeper process is taking place that standard dynamics only partially explains, if at all.

Energy within a complex system does not always flow down predictable paths. Each instant of exchange of motion or information between combining atoms may alter the energy state, as reactions proceed. As the composition, weight, and kinetic energy of the atoms of rocket propellant change, the entire system's behavior changes. [271]

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271 The concept that energy is the equivalent of information raises the specter of a reversal of entropy, that is. a challenge to the Second Law of Thermodynamics. But information exchange does not violate the Second Law. In fact, when information in a system is erased, by an external action, entropy is increased, rather than decreased. Information exchange is not unidirectional, but no information in a dynamically evolving system is ever permanently stored. Thus, there should be no violation or permanent reversal of the Second Law, provided that the system operates independently of any conscious outside influence.

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A complex chemical or physical system's structure cannot be expected to perform as if it were simply a set of building blocks neatly stacked. Its behavior as a system radically alters the energy relations among its component parts. A liquid propellant rocket's complex and integrated mechanical systems, its pumps, its pressure tanks, its nozzle and directional vanes, its servo-mechanisms, its navigational systems, all can be engineered to higher tolerances, or can be more ingeniously designed. These mechanical systems are the machine's backbone.

Yet, there is another vantage point from which to approach rocket motor performance. When a rocket functions properly, it is not only a highly integrated tool, but a self-adjusting energy package. Both the chemistry and the mechanics of the rocket contribute to its effective operation. Its energy dynamics evolve as it is consuming itself. Burning rocket fuel, confined by the interior surface of the combustion chamber, nozzle casing, etc. unleashes kinetic energy. Eventually, all the potential energy of the unignited fuel will become energy of motion. Instant by instant, molecular changes involve changes of state on a sub-atomic level, and this actually produces exchanges of information among atoms relative to position, momentum, and energy level. This is in no way an intelligent process, but does describe phase changes succinctly.

Can a phenomenon be known instantaneously, while it is in flux? Yes, within the limits of the Uncertainty Principle. There are two pathways. The first assumes that the macroscopic—gross—manifestations of the system are all that matter, and that the final state of the system will arrive, regardless of any perturbations. The second assumes that turbulent behavior within the

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system constantly changes the surface and the molecular arrangement of the system, and the expected result comes about only as one of several possible outcomes. The outcomes may be infinite in number, in which case it is absolutely essential to know the initial state of the system in order to begin to predict the degree of turbulence which may result.

In other words, chemical or physical dynamics can change from periodic—predictable and deterministic—behavior to a transitional state known as quasi-periodic, or even directly into chaotic behavior. Quasi-periodic behavior of a system actually looks non-periodic and unpredictable, but still has a set number of frequencies for each independently moving variable. If we have a function, x(t), x as a function of time, and this x describes quasi-periodic behavior of a turbulent fluid, then

$$x(t)= f(w_1t, w_2t...w_kt)$$

where f is a periodic function of period 2π (or 360°) and $w_1, w_2,...w_k$ are independent frequencies of some rationally finite number. The system's behavior is quasi-periodic because the number of independent terms has some ultimately defined number, k.

However, a tiny change in the system's initial conditions can replace

$$(w_1t, w_2t...w_kt) \text{ with } w_1t+\alpha_1,...w_kt+\alpha_k,$$

α_1 and α_k, being small stochastic variables. [272]

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272 A stochastic variable is a random variable involving some degree of chance. For further information on the nature of quasi-periodic and chaotic behavior of variables (signals, atoms, etc.), see: David Ruelle. *Chaotic Evolution and Strange Attractors: The Statistical Analysis of Time Series for Deterministic Nonlinear Systems.* Cambridge, the University Press, 1989. pp. 4-7.

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A chaotic system can possess a finite number of independent variables (such as atoms colliding, transforming, and moving unidirectionally), but with an additional tiny element whose initial state cannot be determined. Therefore, although the ultimate fate of the system is clear—its entropy will increase—the route to this end cannot be foretold with good assurance.

Energy, then, is a form of information. [273] Energy absorption and release are changes of state within a molecule. These changes of state reorder the entire energy package, not only of the molecule, but of its neighbors, and eventually, the entire collection of molecules. The macroscopic properties of matter give evidence of the matter's hidden structure and dynamics. Yet, the thermodynamics of an energy system can only be predicted statistically. The position and momentum of individual molecules cannot. Turbulent behavior originating within the tiniest component can throw the system into disarray.

Chaotic molecular behavior within a rocket's combustion chamber spells potential disaster. Whether or not the motor flies apart depends on reaction rates and upon the cascading flow of information pulsing through its fuel as it burns. Propellant molecules burn at even rates when their molecular structure is similar enough to permit even release of energy. The release of energy must be relatively orderly. Yet, the reaction is contained by its vessel,

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[273] The concept of energy as information originated with Leo Szilard, who first postulated the idea in 1929, in his 'Überdie Entropieverminderung in einem thermodynamischen System bei Eingriffen intelligenter Wesen.' ('On the Decrease of Entropy in aThermodynamic System by the Intervention of Intelligent Beings.' *Zeitschrift fur Physik*, 1929, No. 53, pp. 840-856. The essay can be found in Harvey Leff and Andrew Rex's, *Maxwell's Demon: Entropy, Information, Computing*. Princeton, The University Press, 1990, pp. 124-133. Szilard was, incidently, a graduate of the *Minta*.

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and this changes the energetics of the system, as pressure and heat within the reaction chamber have only one exit. When propellant and oxidizer are mated, with temperature and pressure of the components varying from instant to instant, the anomalous behavior of any one (or more) component can alter the entire reaction. The assumption has usually been that a sufficient number of molecules will behave in an orderly fashion and obey the statistical laws of quantum mechanics, so that predicted results will unfold. But there is no assurance of this.

The very essence of the Uncertainty Principle reveals that unpredictability lies at the core of molecular behavior. Any attempt to simultaneously define or quantify the momentum and position of any single atom would instantly change its state. The interaction of all the individual atoms and molecules changes the structure and the amount of energy they contain. This energy state transfers information to surrounding molecules to alter their state as well. Energy transfer is a system of relays, or messages within a complex system of constantly interacting components. Energy changes within one molecule will induce specific effects in its neighbors, and the entire community of molecules will alter its state. Thus, turbulence is essentially a pattern of instantaneous change within a molecular system, a pattern not without its own order and rhythm.

The sequence of steps in this approach to rocket combustion dynamics is straightforward: predicting rocket performance of the rocket is not merely a matter of isolating one or two crucial components of the system and then refining them to within ideal tolerances. The engineer might want to look at the entire machine as a complicated energy exchange package—as a

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coordinated system. This system tends toward non-ideal, even turbulent behavior in its component parts. Chaotic behavior becomes, not the anomalous, but the expected state of the system's internal dynamics.

This assertion immediately raises two important problems. Firstly, if chaotic systems behavior is at the heart of a physical system under stress, then no rocket should fly successfully. Nearly all of these complex machines should experience burn-through, veer off course, explode during their ascent, etc. Yet, they do not. Secondly, if rocket *qua* machine demands coordinated design, then Malina's strategy, that of developing a working motor first, was misguided. This was not the case either. Within its limits, power-plant construction had to precede systems development.

The point is that there are three levels of complexity to confront in designing a rocket. The first is the technical or mechanical challenge of propulsion and guidance. When difficulties become evident, the engineers and builders confront them by seeking their cause in mechanical malfunction and re-design or replace affected parts. This should solve a multiplicity of problems. But on another and deeper level rocket propellant combustion intensifies the system's instability. Finally, since the entire rocket must work as an integrated system in order to fly successfully, the factor of unpredictability, even in mechanical terms, becomes magnified several times.

A rocket is not just a mechanical device, as its functioning involves fluid and aero-dynamics; the discrepancy between ideal depiction of fluid behavior and real behavior can be large. Malina had to assume that his ideal treatment of propellant combustion dynamics would sufficiently describe a rocket motor

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in action. He knew this was not entirely the case, yet had no choice but to use the mathematical tools at his command. Neither Malina, Tsien, nor von Kármán, it may be added, had high-powered computational machines at their disposal.

To illustrate the hurdles GALCIT rocket engineers faced, and the theoretical assumptions they made while trying to overcome these hurdles, one need only look at a von Kármán monograph written 15 years after the 1936-37 rocket motor tests performed at Caltech. Even a decade-and-a-half later (1951), von Kármán still had not delineated the fundamental processes of combustion without resorting to approximation and mathematical idealization. In 'The Thermal Theory of Constant Pressure Deflagration,' written with Gregorio Millán, von Kármán spoke of

> the fundamental problem of the theory of combustion [which] consists in the computation of the so called normal velocity of combustion, i.e. the velocity of propagation of a combustion wave in a gas mixture, which is capable of a well-defined chemical reaction. Instead of investigating the propagation of the combustion process in the gas mixture at rest one can consider stationary flow relative to a combustion or flame front, which is fixed in space. We assume that the gas is infinitely extended in space and the flame front is perpendicular to the x -axis, so that temperature, density, chemical composition and velocity are functions of the x -coordinate only. For a great class of combustion processes laminar flow can be assumed and it can be shown that the assumption of constant pressure gives a fair approximation. Also viscous processes can be neglected. According to a full analysis of the combustion process two physical phenomena are responsible for carrying the combustion ahead through the gas mass: heat transfer and transfer of matter....Thermal theory considers heat transfer only and neglects diffusion. Although this means a considerable restriction, the thermal theory gives a fair description of the phenomena and many of its predictions are confirmed by experiment. [274]

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274 Von Kármán and Gregorio Millán. 'The Thermal Theory of Constant Pressure

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The assumptions that gas under expansion during the burning process is "infinitely extended in space," that "laminar flow" and "constant pressure" accurately describe gaseous conditions in the combustion chamber are open to question. First of all, in a rocket combustion chamber, the process is not infinitely extended, but limited on all sides by the chamber walls; nor is the flame front infinite outside of the chamber (in free air). There is no way to tell if gas flow is *ideally* laminar (arranged in smooth discrete layers.) Nor can one neglect fluid viscosity, which will influence the rate of fluid flow. Von Kármán was well aware of the limitations of his thought-experiment, but he had no choice but to treat the burning gas ideally, as there was no theory available to him which might have described the actual chemical and physical state of the gas.

Von Kármán so much as admitted the deficiencies of his treatment of the combustion process at the close of his short paper:

> These conclusions may reconcile the conscience of the chemist, who believes that the experimentally found ignition temperatures are not significant for the burning process...and the conscience of the mathematician, who believes that without defining a definite ignition temperature the problem is undetermined. However, we are afraid that this reconciliation of the two different viewpoints is a rather superficial one. It remains a serious problem, what physical or chemical process determines the beginning of this reaction. [275]

In effect, it was impossible to know the exact initial state of the gas as combustion began, and so, impossible, theoretically, to foretell how the gas

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Deflagration.' In Von Kármán, *Collected Works, Vol. IV* (1956), p. 473. Von Kármán originally wrote this paper for a conference on Applied Mechanics in Haarlem, Holland, 1951.

275 Von Kármán and Millán (1951), in Von Kármán, *Collected Works, Vol. IV*, p. 480.

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would actually behave. One might safely assume the ultimate result: the Second Law of Thermodynamics (entropy) would apply. But just what would happen inside the combustion chamber *en route* to the final state? This could—and did—vitally affect the performance of the rocket motor. Several years after the first GALCIT rocket motor tests in 1936, the essential problems remained theoretically unresolved.

The difficulties of embarking upon the experimental study of the rocket motor without fully understanding its dynamics—that is, its unpredictability—was reflected in test results between October 1936 and January 1937. The GALCIT Rocket Team was only partially and incrementally successful in its first experiments. Witness a motor firing of forty-four seconds in late 1936 tests in the *Arroyo Seco.* (This was no mean accomplishment in an age of extremely modest successes.) This relatively long burn came after three months of failure. Looking at the records of the *Arroyo Seco* tests of October 1936- January 1937 (taken from notes written by Malina and Parsons), one finds the following:

> **For liquid-fuel motor tests, oxygen and methyl alcohol mixture:**
>
> **Test date: October 31, 1936.**
>
> 3 tests completed-- in all three tests, the ignition fuse blew out of the chamber without igniting the fuel. During the last test, the falling fuse ignited the alcohol-oxygen mixture above the tank and created a flame almost two feet high. Malina noted that he doubted the combustion chamber actually ignited at all. Other problems complicated the tests. In a 'Summary of Test I,' Malina wrote that:

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1) the pressure connector leaked.

2) the fuel and nozzle diameters were too large.

3) the lack of a protector for the thrust mechanism allowed cooling water to spill on it.

4) a spark plug would have to be added to the ignition chamber to ignite the fuel. According to Parsons' comments for the earliest series of tests, he was igniting the fuse by hand, to begin each of the test runs.

Based of the last test run of the day, Malina calculated that the fuel had burned at 10.72% of its potential efficiency, despite the fact that ignition occurred outside the chamber and the oxygen line blew off.

Test date: November 15, 1936.

Three tests performed.

1) in test 1, fuel flooded the chamber; no ignition.

2) in test 2, excess fuel-burn; 5-second combustion with bright yellow flame. Oxygen line caught fire. All parts clogged with burnt rubber.

3) in test 3, the spark plug was wet; no ignition.

Following these tests, Parsons suggested various improvements to the apparatus, including keeping the check (safety) valve vertical, keeping a good eye on the rubber plugs and, in general, using a smaller set-up.

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Test date: November 28, 1936.

Four tests performed.

1) test 1, exhaust flame for two seconds

2) test 2, intermittent 3 inch to 1 foot flame

3) test 3, continuous 6 inch to 1 foot flame (no duration recorded.)

4) test 4, intermittent exhaust flame (no duration recorded.)

In these last tests, a mixture of methyl alcohol and nitrous oxide (NO_2) was used.

These tests clearly resulted in some success. Parsons made no known comments about improvements to the test apparatus.

Test date: December 31, 1936.

Four tests performed.

1) test 1, oxygen at 80% of maximum pressure, fuel flooded out. No ignition.

2) test 2, oxygen at 80% of pressure, flame at start, fuel flooded out and extinguished fuse.

3) test 3, same results as test 2

4) test 4, oxygen at 100% pressure; 2-3 second intermittent combustion. Oxygen line broke free and whipped around.

Following this series of tests, Parsons recommended using "properly working regulators and a much smaller fuel nozzel [*sic.*]"

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Test date: January 16, 1937.

Two tests recorded:

1) test 1, a 28 second flame achieved.

2) test 2, a 44 second flame achieved.

In this second test, the motor turned bright red. (The motor was pure copper.) [276]

Obviously, by mid-January, some of the mechanical deficiencies had been corrected.

In all these tests, success meant a rocket motor burn of more than a few seconds. Most of the problems encountered were mechanical. The test apparatus was relatively small (the entire nozzle and combustion chamber were eight and one-half inches long.) In sum, the duration of burn was not long enough to record any abnormalities of fluid flow and burn within the combustion chamber for these tests. The longest test, 44 seconds, provided no data on possible problems.

Following this first series of liquid propellant tests, Malina mentioned that the group, with the exception of Parsons and Forman, went back to the drawing boards. There were no further liquid fuel tests until later in the spring of 1937, when the group tried suspending a motor on a pendulum

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276 Data obtained from GALCIT Rocket Research Notes, Malina and Parsons, October 1936-January 1937. Courtesy, JPL Archives, Document # HC3-94. Note that in the November 28 tests, Parsons began experimenting with nitrogen compounds as oxidizers. He eventually settled on red fuming nitric oxide as an oxidizer of choice, one of the first storable liquid oxidizers in American rocketry. Red fuming nitric oxide (RFNA) was far more practical an oxidizer than was liquid oxygen, which necessitated cryogenic storage and was too expensive for the group to purchase. The only major drawback to RFNA was its toxicity. It is an extremely corrosive liquid and gives off acid fumes during oxidation.

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inside the GALCIT laboratory. The results were disastrous, and the group found itself forced to move its test apparatus outside. A cloud of red fuming nitric acid had filled the entire laboratory, coating all of its equipment with a fine yellow layer of dust. [277]

Meanwhile Parsons and Forman continued their solid propellant tests, initially to the annoyance of Malina and von Kármán. The tests were noisy, often ending in explosions. Parsons conducted some of them near the GALCIT building, where, of course, von Kármán couldn't help but hear them. Malina felt he just had not completed enough solid theoretical work to justify the tests. But as usual, he could only control Parsons with difficulty. Most of these tests were failures, as Parsons concentrated on building modified black powder or nitrocellulose powder rockets.

There is no mention in Malina's memoirs of any liquid propellant tests scheduled for 1938. This year seems to have been a year of theoretical exploration, presentation of data to scientific societies, disappointment and finally, recognition by the scientific community—at least, to some limited degree. In July 1939, after the Rocket Research Team had begun to receive funds from the US Army, Parsons contracted with the Hercules Powder Co. of Pinole, California, to construct "sticks of powder pellets to be manufactured,

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277 Malina (1967), p. 121. In a 1996 Interview with 'Amo' Smith, this researcher learned that Smith and Malina had designed the rocket motor suspended by a pendulum from the ceiling of the GALCIT building. The motor never fired, but a cloud of RFNA spread across every piece of steel in the laboratory. Smith also recounted his belief that Parsons and Forman had designed and constructed the first liquid propellant rocket motor used in the 1936-1937 *Arroyo Seco* tests. Smith could not be specific about the exact date of the indoor tests, but Koppes (1982) placed them in May 1937, as Malina had written his parents about the testing that month.

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composed of (1) sodium nitrate black powder, (2) potassium nitrate black powder, (3) various mixtures of these powders with smokeless powder. "[278]

Parsons designated these powders, GALCIT 1-A and GALCIT 2-B. Their composition was, respectively:

GALCIT 1-A:

Potassium nitrate-	72. 45%
Sulfur-	11. 53%
Charcoal-	14. 07%
Starch (a binder)	1. 00%
Moisture-	0. 95%

GALCIT 2-B:

sodium nitrate-	72. 05%
Sulfur-	12. 25%
Charcoal-	15. 25%
Moisture	0. 45%

GALCIT 2-B actually came off the assembly line at the Hercules Powder plant first, and Parsons used it in the first two black powder experiments he conducted in 1939. GALCIT 1-A became the propellant of choice in experiments 3 - 11. GALCIT 1-A, it will be seen, had a slightly higher percentage of sodium nitrate than did GALCIT 2-B, as well as a starch binder. GALCIT 1-A thus had a higher loading pressure than its predecessor (10,000 lbs/square inch, rather than 6850 lbs/sq. in. for GALCIT 2-B.) This meant that more propellant could be compressed into the rocket shell, at least theoretically increasing thrust.

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[278] Malina, Frank, John W. Parsons, and Edward S. Forman. *Air Corps Jet Propulsion Research, GALCIT Project No. 1, Report No. 3, Final Report for 1939-1940.; Caltech, June 15, 1940.* JPL Archives, Document # HC 3-1206 (unpublished), p. 76.

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Despite their careful manufacture, Parsons and Forman had little success with these powders. A review of data on tests conducted in 1939 indicates that all the motors exploded or burned in layers. The motor casings were not completely destroyed, however. The two men had been clever enough to install shear bolts on the motors which blew the nozzle section off when a critical pressure was reached. Parsons concluded dryly that "a more satisfactory powder would have to be developed." [279]

Beyond recognizing mechanical problems and the poor choice of powders for the solid combustion tests, Malina made some intriguing comments. He noted that the probable error rate (that is, the failure rate) for the liquid fuel tests was between 2% and 5%, not particularly high (basing his calculations solely upon imprecision in machining the apparatus, malfunctioning valves, pressure gauges, etc.) He then went on to say:

> critical pressure prevailed at the nozzle throat for the whole range of pressures....the propellant flow coefficient should be independent of the pressure ratio after the critical pressure is reached in the nozzle throat. The deviation of the experimental results from the theoretical prediction can be charged to the fact that the gas does not enter the nozzle entrance from rest, the presence of turbulence in the chamber, and the non-consistency of the chamber temperature. [280]

By 1940 Malina had done some initial studies on the problem of turbulence. (He had even noticed the problem while researching his Ph.D. thesis); this is why he mentioned this potential 'fly in the ointment' present during testing.

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[279] Malina, Parsons, Forman (1939-1940), pp. 80, 82.

[280] Malina, Parsons, Forman (1939-1940), p. 17.

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He could not, however, pinpoint turbulence as the sole cause of "deviation... from theoretical prediction." Results were mixed, and in fact, failure was the rule most of the time. But these early mishaps surely do not unequivocally point to one cause, and one cause alone, of malfunctions. Nor is it possible to trace failure solely to an overlooked or poorly understood factor, when success eventually appeared on the horizon.

After all, to be fair, *no* rocket group experienced unmitigated success in the 1930s. The Germans may have been ten or more years ahead in solving the design and dynamic problems of rocket construction when the Caltech group was only just starting, but in no way did the Peenemünde group avoid the pitfalls of poor design, the demon of unpredictability, and outright failure. [281] The Germans, too, experienced their share of motor explosions, steering vane meltdowns, pump failures, gyrostabilizer malfunction, etc. At issue for the Caltech group was: could the team construct a sound theoretical framework and translate this into reliable working rockets? With the design models and working techniques (including the mathematical tools) available to them, the answer to the both questions was eventually, yes.

Malina discussed the turbulence factor as if it were 'part of the total equation' of testing. He did not let it get in the way of continued testing, or his belief in the essential accuracy of his calculations. The rocket had to work; at least, that is what the mathematics predicted. Deviation from ideal results was a matter of degree, not one of fundamental flaws in the approach to the problem.

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[281] For an illuminating description of the Nazi rocket group's struggles to design and construct a ballistic missile in the 1930s, see Neufeld (1995), pp. 64- 71. (in Chapter 2, 'The Founding of Peenemünde.')

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And, to the credit of the group, the turbulence factor was part of their agenda. The team intended to tackle the problem within the framework of engineering solutions to complex problems. After all, instability in the combustion process was the culprit behind numerous explosions. Malina and his crew had to confront the issue.

During this very stage of testing—from late 1936 until 1942—Theodore von Kármán was continuing his studies into the nature of turbulent fluid dynamics. It was a problem on which he was to work for the rest of his professional life. In fact, he had begun to publish monographs on turbulence in real incompressible fluids (such as air or liquids) as early as 1934, and his 1911 studies into the alternating flow of turbulent fluid around a smooth cylinder (since named in his honor the *Kármán Vortex Street*) indicate that he had discovered this puzzling phenomenon of turbulence while still a young graduate student at Göttingen University. [282]

Frank Malina was no stranger to the phenomenon of turbulence, either. During the summer of 1938, when the Rocket Project seemed about to wither away for lack of funds, Malina took a job with von Kármán investigating the rippling properties of sand dunes. The Department of Agriculture had contracted with Caltech to investigate the nature of dust storms and the viability of a planting a 'green shield' of trees across the nation to prevent or at least slow the advance of these monster storms. Malina, Summerfield, and

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282 Von Kármán (1967), pp. 62-63. The *Kármán Vortex Street* describes a phenomenon in which "vortices are shed alternately from the top and bottom of [a] cylinder. The configuration becomes stable when there is a definite geometric arrangement of the vortices. This arrangement occurs only at a certain relationship of two distances—the distance between two consecutive vortices and the distance between the two rows of vortices. Or to put it another way, instead of marching two by two, the vortices are staggered like lampposts along both sides of a street. "

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von Kármán built a portable wind tunnel to simulate wind and sand conditions in the desert. They hoped the tunnel would be transported to various areas of the country afflicted with dust storms, to study actual conditions in different geographical areas. But with the approach of the Second World War, the Soil Conservation Service lost interest in the project. [283]

Almost ten years later, in 1947, von Kármán published the results of this study, entitling it 'Sand Ripples in the Desert: An Aerodynamic Approach to a Baffling Natural Phenomenon.' In his short paper, von Kármán modeled sandstorm behavior after that of a non-homogeneous fluid.

> If we consider the flow of a homogeneous fluid over a wavy surface, we find that the velocity is increased over the crest and decreased in the trough. In aerodynamics we sometimes call this effect the "Bernoulli effect," because it is predicted by Bernouilli's equation. Thus, a wavy ground surface consisting of sand could not maintain its shape in a homogeneous air stream. However, inasmuch as a considerable amount of suspended sand travels with the wind, we must consider the wind as the motion of a fluid with a non-homogeneous density. The lower strata of the fluid are heavier; the specific weight of the fluid decreases with height. [284]

Von Kármán emphasized the "turbulent flow pattern" of the sand suspended in air and wrote that the number of sand particles in suspension was a non-integral function, as extremely small particles can "travel almost indefinitely at arbitrarily high altitudes." [285] The experience of working with von

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283 Malina (1967), p. 124.

284 Von Kármán. 'Sand Ripples in the Desert,' *Collected Works, Vol. III* (1956), pp. 352-356.

285 Von Kármán. *Collected Works, Vol. III* (1956), p. 354.

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Kármán on this seemingly arcane problem (in order to earn extra money during the Depression) proved to be Malina and Summerfield's first practical exposure to the phenomena of turbulence.

It only seemed an interlude in their lives, but the problem of turbulence was to confront them again in early 1940, when their liquid propellant JATO motors began to explode mysteriously just after ignition. [286] Summerfield found a mechanical solution to the problem by shortening the gap between injection and ignition of the rocket fuel in the combustion chamber. His novel idea, discussed above, allowed the team to move ahead with production of the liquid propellant JATO, but did not explain why the lag between introduction of fuel into the chamber and its ignition should cause throbbing and premature explosion in the first place. The problem lay not merely in a timing gap. More likely, this minuscule gap in time had created a certain instability in the burn rate which then cascaded through the chamber, destroying it. Von Kármán was never able to explain the molecular dynamics of the phenomenon. However, the theme of turbulence and nonlinear behavior in fluids cropped up again and again in his writings. It presented, in fact, a consistently woven thread through the fabric of his work.

Not only had Von Kármán, astute analyst of mechanical and dynamical phenomena that he was, seen through the standard explanations of baffling nonlinear behavior of compressible fluids, he had begun to develop the mathematics of nonlinear behavior in some detail. He used the calculus of variations and Fourier transforms to define this seemingly strange behavior

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286 Malina discussed the problem at some length in his Ph.D. Thesis, 'Characteristics of the Rocket Motor and Flight Analysis of the Sounding Rocket' (1940).

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and sought to expand engineering into the realm of events that defy the neatness of the ideal equation of state. [287] He did not accept statistical averaging in physics, and tried to explain more precisely such diverse events as the buckling of the Tacoma Narrows bridge (1940), friction and turbulence around airfoils and fuselages, and the dynamics of turbulence at the supersonic threshold.

In one important paper written in collaboration with his student Chia-chiao Lin (1916 -), 'On the Statistical Theory of Isotropic Turbulence' (1949), von Kármán used Fourier transforms to analyze the transfer of energy among various frequencies of fluid waves flowing homogeneously in all directions. (Although the waves were uniform in structure and direction, they were non-periodic.) Lin and von Kármán determined that, if turbulence were studied over a small local area, its intensity would depend only on two factors: the fluid's viscosity and the total rate of energy dissipation. Even in high frequency turbulence, each frequency could be measured independently, and the stages from onset of turbulence to energy decay would be discrete. In effect, von Kármán had created a model for understanding turbulent

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[287] Jean Baptiste Joseph Fourier (1763-1830) did remarkable work on trigonometric series in the early nineteenth century which is still of great value in the study of wave phenomena. Fourier series permit the physicist and mathematician "to represent a complicated wave as a sum of its fundamental components (called the pure tones in the case of sound waves.)" Essentially, in a Fourier series, one may treat a discontinuous function as if it were continuous—over a limited interval, say $-\pi$ to π. Von Kármán used Fourier transforms in the study of isotropic (invariant in all directions) turbulence, treating non periodic functions as if they converged into periodic wave functions. One important equation derived by Fourier,

$$f(x)= \sum_{v=1}^{\infty} b_v \sin vx \quad (\text{for } 0 < x < \pi)$$

indicates that every function, whether discontinuous or not, could be represented as a periodic series. In this case, b_v= the area under a curve $y= (2/\pi) f(x) \sin vx$ between 0 and π. See: Varberg and Purcell. *Calculus, with Analytic Geometry (6th Ed.)* Englewood Cliffs, NJ, Prentice Hall (1992), p. 519. For the derivation of the above Fourier series equation, see: Kline, *Vol.II* (1972), pp. 671-677.

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behavior in a fluid (such as air.) [288] The problem of unstable burning in liquid propellant rocket motors intrigued him—he eventually thought it might fit into the model he had developed for isotropic homogeneous turbulence. This was not to prove the case, but the work of the quirky GALCIT Rocket crew had put him like a bloodhound on the track of discovering the roots of turbulence.

In truth, the work of his unorthodox graduate students at Caltech could not have led von Kármán to a new and completely unfamiliar path of study. Von Kármán was a polymath. His own roots led him directly to a branch of engineering that few other scientists dared to climb. An admirer of Henri Poincaré, he had read at least part of the great French physicist's work on celestial mechanics (*Les Méthodes Nouvelles de la Mécanique Céleste*), published between 1892 and 1899. [289] Poincaré had a most fecund and penetrating mind, but he did not necessarily tie himself down to set solutions of trenchant mathematical problems. In his introduction to *Les Méthodes Nouvelles..*, the editor, Daniel Goroff, finds the pulse of Poincaré's method: "The point is that Poincaré let the problem determine the method rather than letting the method determine the problem. In this way, Poincaré 'shattered the rigid frames in which the theory seemed to be encased and contrived for it vistas and new windows on the external world.'" [290]

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288 Von Kármán and C. C. Lin. 'On the Statistical Theory of Isotropic Turbulence.' In *Collected Works, Vol. IV* ., pp. 435-451. Von Kármán had studied the work of A. N. Kolgomoroff, Russian mathematician and early explorer of the theory of turbulence.

289 Poincaré, Henri. *New Methods of Celestial Mechanics* (1892-1899), *The History of Modern Physics and Astronomy, Vol. 13, Nos. 1-3.* New York, The American Institute of Physics, reprinted 1993.

290 Poincaré, *New Methods of Celestial Mechanics* (1993), p. 136.

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This had been von Kármán's mental calculus since the days when he was a student at his father's academy, the *Minta.* It was natural that he would tackle difficult engineering problems such as turbulence. In an article written for the *Bulletin of the American Mathematical Society* in 1940, entitled, 'The Engineer Grapples with Nonlinear Problems,' von Kármán gently chipped away at the foundations of standard linear engineering problems:

> Many physicists and engineers have the feeling that so-called mathematical proofs of existence and uniqueness of solutions very often merely verify facts which are more or less evident from physical considerations. Unfortunately, in most nonlinear problems physical reasoning is not sufficient or not fully convincing, so that in these cases the questions of existence and uniqueness represent a real challenge to the mathematician. Certain investigations of H. Poincaré and T. Levi-Civita can be mentioned as examples in which mathematical reasoning has virtually extended the range of physical vision. [291]

Von Kármán speculated that "the modern methods of function spaces are likely to make essential contributions to the problem of existence and uniqueness of solutions of nonlinear equations and their boundary problems." Then he added, surprisingly, "However, this writer is afraid that this aspect of the subject is beyond his own competency and the scope of this paper." [292] This startling admission did not influence Von Kármán's own

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[291] Von Kármán, *Collected Works, Vol IV* (1956), p. 36.

[292] Von Kármán, *Collected Works, Vol. IV* (1956), p. 36. Von Kármán did not define *function spaces*. However, in mathematical analysis, function spaces are a set (say, X) of geometric mappings of a specific space (say, W) into another space and its elements. Function spaces can define the differentiability or continuity of elements in corresponding sets—such elements may be mapped out geometrically in the second set. For example, mappings of W into Y. They resemble the concept of *phase space*, which Stephen Kellert defines in his short book, *In the Wake of Chaos*, as " a mathematically constructed conceptual space where each dimension corresponds to one variable of the system. Every point in [phase] space represents a full description of the system in one of its possible states, and the evolution of the system

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teaching style. Given the plethora of articles he wrote on turbulence and nonlinear problems in engineering, it is clear that he felt confident enough to let Malina and Tsien work on such challenges to the limits of their competence.

For Malina, approaching the phenomenon of turbulence meant working largely within the realm of the calculus of variations. This in no way implies that he was unaware of the complexity of the combustion process within a rocket motor. In his first significant study of rocket motor function, 'Characteristics of the Ideal Solid Propellant Rocket Motor' (1940), Malina wrote that the equilibrium chamber pressure, p_{ce} depends upon the physical properties of the solid propellant, and on the area ratio of the combustion chamber to the nozzle throat, f_c/f_t.[293] Describing the pressure increase graphically, Malina noted how the pressure rises slowly at first and then very rapidly tapers off asymptotically to a limiting value as the area ratio increases.

> The upper limit of *pce* corresponds to the pressure that would be developed in a closed chamber. It has to be noted that the process is not unstable in the exact sense of the term; as a matter of fact, to every ratio *fc/ft.* corresponds an equilibrium pressure and the pressure in the chamber approaches asymptotically this value. However, the slope of the p_{ce} versus *fc/ft* curve reaches very large values in a certain range, so that enormous changes in the pressure are produced by small variations in the burning area, f_c, or the burning characteristics of the powder. 294

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manifests itself as the tracing out of a path, or trajectory on [phase] space." Kellert. *In the Wake of Chaos, Unpredictable Order in Dynamical Systems.* Chicago, The University Press, 1993, pp. 7-8. See also: *The Encyclopedic Dictionary of Mathematics, 2nd Edition.* Cambridge, Mass., MIT Press, 1987, pp. 645-652.

293 Malina, 'Characteristics of the Ideal Solid Propellant Motor,' in Von Kármán, *Collected Works, Vol IV*, p. 98.

294 Malina. 'Characteristics of the Ideal Solid....' in Von Kármán, pp. 98-99.

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Extrapolating from Malina's comments that equilibrium chamber pressure is plotted as a function of the ratio of combustion chamber volume to nozzle-throat diameter, one can deduce that the curve reaches local maxima at points where the total burning area diverges just a bit, and that the graph approaches ∞ as the total burn area decreases and the pressure rises. [295] Such an unbounded increase, of course, would quickly produce an explosion. What is most remarkable, however, is Malina's last remark, that "*enormous changes in the pressure are produced by small variations in the burning area...or (by) the burning characteristics of the powder.* " (Italics added.)

Here Malina was probably referring to tiny changes in the nozzle throat area which produced dramatic changes in chamber pressure. But there is a hint of other, less easily measurable forces at work. As the combustion components burn and exit the nozzle, the total dimensions of the burn area increase. This increase does not occur smoothly, unless the configuration of the burn area allows uniform burn rate. [296] The burn would proceed unevenly, until a burn-through or explosion occurred. It was difficult, as John Parsons so frequently discovered, to regulate the burn rate. The very molecular arrangement of the packed propellant would result in a combustion rate

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[295] This assumes we are plotting pressure on the horizontal (x) axis and the ratio of chamber volume: nozzle-throat diameter on the vertical (y) axis. Then, the narrower the nozzle throat, the more the ratio approaches infinity, as the ratio between chamber volume and nozzle throat diameter approaches 1/0, or is unbounded.

[296] Three JPL engineers, Charles Bartley, John Shafer, and Lawrence Thackwell were responsible for improving upon a British *star-shaped design*, imprinted into the cast by a mold as the hot propellant was poured in (1947). This star-shape permitted more or less uniform burning of the propellant and is still the shape of propellant casts in the Minuteman and Polaris missiles. See: J. D. Hunley. 'The Evolution of Large Solid-Propellant Rocketry in the United States.' *Quest: The History of Spaceflight Quarterly*, Vol. 6, No. 1, June 1998, p. 3.

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gradient. The entropy of the system would increase, but not uniformly. If the chamber sides or the nozzle throat were not perfectly smooth, if minute imperfections appeared before or during combustion, then the combustion rate would be uneven.

Thus, three important variables, the chemical composition of the fuel, its consistency, and the physical properties of the chamber wall also determined whether or not the motor would fire smoothly. No matter how carefully the engineer defined the area of combustion, there would still be some degree of uncertainty in the success of the burn. Malina recognized that his calculations, designed to prove the feasibility of an ideal rocket motor's even burn, were only approximations and that the slightest variations in actual conditions could produce dramatically different results.

The crucible, of course, was experiment. Eventually both the solid and liquid propellant GALCIT JATO units worked. Surely, it would have been impractical to mass produce a rocket motor that could not be relied upon. If the demon of turbulence could not be entirely controlled, then it could be limited. The job was to eliminate as many of the mechanical faults of the rocket motor as possible. First, determine the correct dimensions of the motor, define the terms that regulated combustion, and then find a fuel that burned relatively evenly without cracking or deteriorating over time. The focus had to be on the dynamics of combustion, once the proper material of the container-chamber had been discovered. Limit the potential for failure, and eventually success will come in the majority of cases. GALCIT rocket engineers accepted the possibility that every one of the rocket motors that the team tested might fail.

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Malina and von Kármán had faith in the theory. If theoretical calculation showed that the motors would work, then practical proof was only a matter of time. The team worked to measure and control the construction process. The timing problem in liquid propellant rocket motors was crucial, as was the discovery of a solid propellant that would not crack during storage or burn unevenly. The combustion process was the key to success. This is why Malina focused on the problems of rocket motor construction in 1936. He knew, perhaps instinctively, that rocket fuel combustion would present enough challenges to keep the group busy for some time, and preferred to make an intuitive leap, accepting the accuracy of their calculations.

Understanding how a machine as inherently complex as a rocket motor could function involved the acceptance of intuitive solutions to complicated mathematical and physical problems. Von Kármán expressed it best himself. In a monograph co-authored with Hsueh-shen Tsien in 1941, 'The Buckling of Thin Cylindrical Shells Under Axial Compression,' von Kármán wrote that the integration used to describe the different directions in which metal shells compress under stress were only approximations. "Due to the complexity of the problem, the results given in this paper can only be considered as a rough approximation and most of the statements are qualitative rather than quantitative. To put the new theory on a solid footing, a more accurate solution of the differential equations of equilibrium is necessary." Many of the equations derived by Tsien and von Kármán in this monograph were "established by intuitive arguments, not by systematic reasoning." [297]

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297 Von Kármán and Hsueh-shen Tsien. 'The Buckling of Thin Cylindrical Shells Under

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Von Kármán's comments implied that there were two possible routes to the solution of these equations: the first was to solve by integration and perform the resulting linear (additive) functions, knowing experimentally the value of *each* component in the equation; the second would be to approximate the value of certain key functions, "with undetermined parameters," drop terms that could not be experimentally discovered, and solve the simplified linear equations that resulted. [298] Deductions by intuitive reasoning would help bridge the gap between idealization and reality.

But what did von Kármán mean by intuition? Certainly not the abandonment of rational logical mathematics. The equations he and Tsien derived in their monograph were rigorous; terms dropped out because they were experimentally negligible or undetermined, but the mathematical solutions were still logical and tight. [299] It was mathematical insight—the jumping of an experimental gap, using creative awareness and logic—that frequently substituted for hard data at key points. Intuition and trial-and-error experimentation were keys to success, for the problems of each new type of rocket motor were unlike the problems of their predecessors. Liquid propellant motors presented one constellation of dilemmas; solid propellant motors, another set of puzzles. [300]

Axial Compression,'(1941), in Von Kármán, *Collected Works, Vol. IV*, p. 125.

[298] Von Kármán and Hsueh-shen Tsien (1941), in Von Kármán, *Collected Works, Vol. IV*, pp. 108-109.

[299] For example, a certain term describing the number of waves propagating through a thin cylindrical shell in the "circumferential direction" was on the order of $1/n^2$; if n= 10, the number of waves radiating through the shell would have been 1/100 the number radiating longitudinally. Von Kármán believed this to be an insignificant number. Von Kármán and Hsueh-shen Tsien (1941), p. 110.

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Aside from mechanical problems directly attributable to the more complex machinery of a liquid propellant motor (pumps, piping, nozzle coolant, etc.), there was one fundamental hurdle to be overcome in developing a liquid propellant that solid propellant motors did not manifest: timing. Ignition had to be instantaneous, with the fuel and oxidizer mixing and burning simultaneously, or the burn would become unstable. For the solid propellant motor, however, engineers could leap-frog this issue by combining the propellant and oxidizer in one solid mass. The question for solid propellants became: was the mixture pure, and tightly compressed? Could the mixture be designed as to burn evenly? In both types of propellants, the underlying dilemma was, of course, fluid turbulence.

Although it is clear that turbulence is an important dynamic fact of rocket motor function, in the early 1940s von Kármán and his graduate students had only a partial grasp of the possible remedies. They could recognize and define turbulence, but not sketch solutions for it, except in terms of the mathematics they knew best, linear differential equations. The point was to get the rocket motors to work.

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300 In a 1940 paper von Kármán delivered in Columbus, Ohio at the 15th Annual Josiah Willard Gibbs Lecture, he made the following remarkable comment: "The reader may wonder why the turbulence problem has not been included in this review....The study of turbulent motion has not, as yet, led to a clear mathematical formulation of the fundamental laws for the mean motion. The problem of transition from laminar to turbulent motion has been discussed mathematically...by means of linearized equations without reaching a satisfactory agreement with experiment. The adequate treatment of nonlinear equations is bound to contribute essentially to the solution of this important problem." Von Kármán. 'The Engineer Grapples with Nonlinear Problems.' *Bulletin of the American Mathematical Society*, No. 46 (1940), pp. 615-683. Also in *Collected Works, Vol. IV*, p. 74.

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The solution to the problems of quirky motors lay not in refining their equations to fit the possibility of unstable behavior, but in constantly re-designing rocket machinery until it functioned properly. The problem appeared as an engineering dilemma, not exclusively as a mathematical puzzle. Turbulence in the rocket motor deeply intrigued the team's young engineers (especially Tsien, Summerfield, and Malina), but it did not dominate their continued efforts. Whatever the underlying cause of the mishaps they encountered, failure along the way did not deter experimentation, especially after Army Air Force funding came through. [301]

What is remarkable is the effectiveness of the intellectually adventurous approach the GALCIT team took to a field still in its infancy. There are factors built in to the design of structures that perplex and stymie engineers until they have either developed a novel theory or truly thorough experimental techniques to account for them. Already working at the frontier of design and

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[301] In a monograph prepared for a symposium on combustion in 1970, Summerfield confronted the complexity of mathematical models describing the way a gas ignites in a partially open container: "The mathematical models describing both the gas phase and surface reaction models are non-linear and extremely complex; several approximations were used in the treatment of these models. The characteristic time for the transient heat conduction within the solid phase is typically much longer than those of the processes in the gas phase; thus the momentum, energy, and species equations for the gas phase are taken to be quasi-steady. They are also simplified in accordance with the laminar boundary layer approximation. Numerical solutions of both models have been carried out, utilizing the additional approximation of local similarity. All of these approximations are reasonably valid up to the time of first ignition. The results indicate that a gas phase ignition model predicts sensitive dependence of the first ignition point on the flow velocity; for the surface reaction model, on the other hand, the point of first ignition is always the leading edge of the sample." In: 'Ignition of a Solid Polymeric Fuel in a Hot Oxidizing Gas Stream,' abstract submitted for the 13th International Symposium on Combustion, Salt Lake City, Utah, August 23-29, 1970. Library of Congress, Malina Collection, Box 6.25. This paper, of course, appeared nearly 30 years after Summerfield's initial discovery that discontinuities in ignition time could lead to unstable burning conditions in the combustion chamber. Summerfield devoted the rest of his life to problems of rocket motor dynamics and combustion.

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inventing the process as they went along, these men could not be said to be ignoring a problem they only incompletely understood. Because von Kármán could extend his vision beyond the realm of the conventional, he could discern the outlines of the new mathematics required to resolve the unusual phenomena involved in rocket experimentation, but he neither could nor did devote time to creating a new mathematics to explain turbulence.

Von Kármán sought understanding within the realm of knowledge available to him in the 1930s and 1940s. His team sought solutions that were practical, searched for refinements in the gross characteristics of rocket functioning, in the chemistry of the propellants, in alterations of the mechanical state of the engines. For Malina and Summerfield, the source of unstable burning was in the timing of ignition. Once they had solved this problem adequately, they could move on to the next phase of development. Von Kármán and Tsien uncovered in their theoretical study of nonlinear problems a new range of phenomena about which they had only sketchy knowledge. Why the eminent Hungarian scientist did not follow in Poincaré's footsteps and devote his considerable mathematical skill to investigating turbulence fully is a matter of speculation. He did not. This is part of the essence—and the frustration—of the GALCIT team's investigations. [302]

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[302] The full extent to which Tsien may have investigated turbulent phenomena cannot now be known. Following his return to China in 1955, his work developing ballistic missiles for the Chinese government became classified and is currently inaccessible to Western researchers. However, von Kármán recognized a key aspect of low intensity turbulence: in a 1951 paper, 'Introductory Remarks on Turbulence,' he wrote, "the tendency of turbulence toward some order is well known by experimental evidence, but its theoretical foundation remains a challenge to those who work on the fundamentals of statistical mechanics." *Collected Works, Vol.IV*, p. 460.

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Along with their intellectual audacity and their courage in the face of actual danger, the GALCIT team had the discipline to define problems within certain clear limits. When Summerfield discovered in 1941 that the volume of the combustion chamber determined the extent to which a hydrocarbon fuel burned efficiently, he inadvertently recognized that an important ratio, that of chamber volume to burning area, would prove the key to complete combustion. (This was his important realization that a small combustion chamber burned fuel more effectively than a large chamber.) But he also opened the door to a challenge: for each increase in the size of the combustion chamber, there had to be some ideal relation between chamber volume and total burning area that would yield complete combustion. Yet, the larger the motor the GALCIT team built, the more new problems arose.

> One of those unexpected quirks of experimentation
> cropped up as the scale of the unit increased.
> Combustion in the 1,000-pound (thrust) model was unstable;
> sometimes it was delayed; sometimes it failed completely.
> Moreover, a disturbing new phenomenon appeared.
> Once combustions occurred, the motor began to pulse,
> gathering in intensity, until, if not shut off after
> the fourth or fifth throb, the motor exploded. 'Now we had
> a real unknown and there were no experts to call in,'
> Kármán recalled. 'We weren't sure we'd ever come up
> with a practical solution.' [303]

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[303] Koppes (1982), pp. 13-14. Summerfield's insights into the efficiency of small rocket motors contrast neatly with the discovery in the 1760s by James Watt that smaller steam engines were less efficient—wasted more energy— than larger, full-scale engines. In a small steam engine the "ratio of the surface area of the cylinder to its volume was larger than in full scale engines." In other words, the cylinder was so large in relation to the size of its containing wall that less volume was available to allow for the expansion of steam. Less energy was then available to perform work. Watt partially solved the problem by keeping the main cylinder at the same temperature (isothermal) and by re-condensing the steam in a separate chamber. See: Stephen F. Mason. *A History of the Sciences*. New York, Collier Books, 1962, p. 277. Note that thermodynamic conversion of liquid water into steam does not alter the quantity of heat in the system, even though the water has changed state. However, the combustion of a liquid fuel

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Of course, Summerfield's insight on reducing the delay between ignition and actual combustion to the barest minimum was a *partial* solution. Summerfield recognized that the initial conditions of combustion would determine the outcome. The problem had only a partial solution because, each time the combustion chamber size changed, the initial conditions changed. Injecting the fuel and oxidizer at even slightly different times into the chamber would cause differential rates of combustion. Malina had seen, as early as 1940, that even minuscule changes in the size of the burning area could induce great changes in chamber pressure.[304] Because fuel and oxidizer were sprayed into the fuel chamber at different instants, the motor would burn some of the fuel in intense bursts, and the remainder of the fuel at a steadier rate. The rate of combustion then was unsteady, or aperiodic.

But why would this aperiodic flow of hot gases and partially burned fuel become unstable only in larger motors? A smaller volume motor means a smaller chamber wall and reduced surface area over which the burning fuel molecules could react, which apparently promotes more efficient fuel consumption. In other words, an equilibrium temperature for the gas is quickly reached and the gas burns at an even temperature, its final temperature, until it is nearly all consumed. But at a certain critical chamber size, a threshold is crossed, and an unstable turbulent gas flow sets in. This

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in a chamber brings about not only a phase change, but a quick and dramatic increase in the total amount of heat in the system. Summerfield also realized that pressure and volume in the combustion chamber were inversely proportional (under ideal conditions), so that by decreasing the volume, one could increase the internal pressure of the system. Of course, energy efficiency in a thermodynamic system is not merely a function of the containment vessel's size, but of burn rate, as well.

304 See Malina, 'Characteristics of the Ideal Solid....' (1940.)

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implies that chamber design may be critical to a successful rocket launch. As the unit's capacity increases, there may be an incremental increase in instability. There is no question that factors such as thrust coefficient and specific impulse of fuels contribute to stability in fuel combustion. The speed with which the fuel burns and the resultant energy released depend upon the type of fuel employed. This is why rocket builders invariably choose fuel/oxidizer combinations with a high specific impulse: they want more exhaust power out of the fuels they use, enabling the rocket to operate efficiently and accelerate quickly enough to break gravity's bonds.

Two essential factors contribute to turbulence within a rocket's combustion chamber: gas dynamics and chamber design. Ideally, burning gas reaches an equilibrium temperature and stays there until consumption is complete. This is a key assumption Malina and von Kármán made in their analysis of the solid propellant rocket motor. But the ideal model of a gas does not take into account friction and molecular inhomogeneity. The total system interacts: the chamber wall, the flowing gas, the fluid as it is injected into the combustion chamber under pressure. Turbulence—that is, disruptions not only on the molecular level, but as manifested in uneven stream flow—begins when temperature fluctuations disrupt the even flow of the gas through the chamber and out the nozzle. The process of combustion becomes intermittent, rather than steady, and it is this intermittence which creates local areas of turbulent behavior.

One can imagine a gas as essentially dimensionless, except on the molecular level, and yet, as filling a clearly defined and shaped container. [305] The

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[305] The entire concept of dimension is given a new twist by Benoit Mandelbrot in his *The*

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container has volume but the gas itself does not. The model of ideal gas behavior at extreme temperatures and pressure does not hold, as molecules of a burning gas at high pressure are not solid atoms bouncing off each other, but an immense series of rough surfaces interacting with each other in a constantly changing state, or phase space. [306] Since the gas, a flowing fluid for all intents and purposes, is moving extremely fast (macroscopically), its flow is not laminar, but irregular and complex. The critical value at which the onset of turbulent flow begins must depend upon the speed of the gas flow in relation to its container walls. The boundary at which the collection of gas molecules begins to display non periodic behavior depends, then, upon the particular set of parameters defining the gas molecules in a rocket motor. The shape and volume of the containing vessel are not, in and of themselves, the only critical aspects influencing when turbulence begins. They determine the shape of the puzzle, but not the length and measure of every individual piece of the puzzle.

The analogy to a jigsaw puzzle is apt (though not exact.) The size of the puzzle is predetermined and thus limits the gross image of the entire mass of puzzle pieces. The puzzle's designer can create the exact shape and dimension of each particular piece randomly (even if there is a relatively limited number of different shapes that accommodate the interlocking

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Fractal Geometry of Nature, New York, W. H. Freeman and Co. 1977. See: Chapter 3, 'Dimension, Symmetry, Divergence,' pp. 14-19. The normal concept of dimension involves coordinates that coincide, as in the Cartesian system. However, Mandelbrot invokes the idea of a multiplicity of dimensions; such sets of dimensions are *discordant*- they fail to coincide.

306 Phase space is a coordinate system having velocity as one of its component axes. Thus, both position and speed of a particle can be measured. Phase space seems to circumvent the Heisenberg Uncertainty Principle, because it "represents the instantaneous position and velocity of the particle under study." See: Hugh D. Young, *University Physics, 8th Edition*. Reading , Massachusetts, Addison-Wesley Publishing Co. 1992. p. 368.

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pieces.) When does the puzzle lose its discernible shape and image and become a jumble of disconnected pieces? Does this occur when one piece of the puzzle is removed? Ten? One fourth of the puzzle? This depends upon the shape of the pieces, the image their interaction creates, and the puzzle's borders. Similarly, the critical boundary at which turbulence begins is not a set point, but depends upon the initial boundary conditions of the system and upon *which pieces are removed when.*

This geometric portrait of the onset of turbulence within the rocket motor is not the image of turbulence in gas dynamics envisioned by von Kármán, Tsien, or Malina. Turbulence, defined by von Kármán as a macroscopic flow property, was a phenomenon to be approached analytically, not geometrically. Like many other scientists of his era, Lev Landau among them, von Kármán could only glimpse part of the mathematical structure needed to define turbulence. [307] Not until the 1960s and 1970s, when a new generation of mathematicians and scientists approached non-linear phenomena from a different perspective, would more light be directed upon the problem.

The struggle to understand and build a working rocket motor represented the core of the GALCIT Rocket Research Team's efforts. Several key decisions, taken early on, led to the investigation of combustion dynamics, motor design, and to experiment with materials capable of withstanding extreme

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[307] Lev Landau (1908-1968), a superlative Russian physicist, investigated many puzzling phenomena, including the onset and structure of turbulence (fluid dynamics) and superfluidity. Landau's model of the onset of turbulence describes a stacking effect of new quasi periodic dimensions of motion. "Some small disturbance will lead to three-dimensional quasi-periodic motion, then four-dimensional, and so on to infinity. The onset of turbulence represents the piling up of huge numbers of incommensurable frequencies, representing the excitation of more and more degrees of freedom—more and more eddies within eddies." For an introduction to Landau's work, see: Kellert (1993), p. 9.

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operating temperatures. Because the GALCIT team chose to work with essentially simple motor designs, the choice of propellant was key to success. The group made singularly innovative strides in inventing new propellants or in discovering propellant combinations that eventually produced working motors. Yet, along the way, experimentation uncovered fundamental problems that their analytical approach could not solve. Fortunately, there was a partially satisfactory solution to the engineering dilemmas confronting them; this solution was mechanical and design-based.

In other words, build the machine better, and the nagging problems will disappear. This is not always true, and is precisely why focusing on turbulence in complex systems is germane, even from the vantage point of hindsight. New puzzles in physical systems exist, even if scientists do not see them. The truly insightful sketch the image, the design of these yet-to-be-solved conundra, but frequently from the perspective of mathematical frameworks that do not quite fit. There is no judgment attached to the failure to see a problem when the current conceptual frame of reference does not allow it. Looking back at an unsolved problem through new glasses does not necessarily invalidate older ways of modeling a theory. In fact, a new system of thought only enhances or casts an older design into higher relief. Understanding the model of turbulent fluid behavior in a rocket motor, a complex mechanical and physical system, is a process of building insights, of turning the model over and over until new images of it reveal themselves. The roots of turbulence in rocket combustion are still not fully understood. In touching upon the problem and in attacking it with the only tools known to them, the GALCIT Rocket Research Team approached the door to deeper knowledge. This in itself was an accomplishment.

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Chapter 8

The Balance between Theory and Practice in Rocket Building—Part One: The Inimitable John Parsons, Apostle of Experimentation at GALCIT

A complex stone such as a diamond has so many facets that, whichever way one turns it, new light glistens from it. This analogy holds for the work of the GALCIT Rocket Research team. Their creations, sprung from a small niche in the California countryside, certainly lacking in the status always ascribed to the efforts of Robert Goddard, have proven their durability. The single most important invention of their early collaboration, the JATO unit, became the progenitor of a whole class of solid propellant rocket motors.

With modifications, some of them basic, the composite solid propellant molded by the GALCIT crew became the staple fuel of American ballistic missiles, from the *Polaris* to the *Minuteman*. Castable propellants allowed the United States to maintain a credible deterrent to Russian and Chinese missile arsenals. [308] In fact, the strongest arm of the United States nuclear force, its submarine fleet, has been equipped ever since the late 1950s with castable solid propellants designed by engineers and chemists at corporations like Thiokol, Aerojet General, and Lockheed Martin. [309]

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[308] The term *castable* refers to the process that combines oxidizer and fuel in one liquid mixture. This mix, usually prepared at a temperature near the boiling point, then pours relatively easily into molds, sometimes directly into rocket casings. When cooled, the mass forms a complete solid propellant and can be stored for long periods of time.

[309] The Thiokol Corporation, originally founded in 1929 to produce synthetic rubber, began in the late 1940s and early 1950s to collaborate with JPL chemists like Charles Bartley in order to produce a new generation of solid propellants (those using butyl-polysulfides.) Thiokol also produced propellants used on US Space Shuttle boosters— its solid propellant boosters were on

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The advantage of the castable solid propellant lies in its chemistry and its storage capacity. The earliest known castable propellants, those invented at GALCIT, could cook in a vat and then be ladled into JATO casings. More modern propellants, such as those which use polysulfides or polyurethane with aluminum added to boost specific impulse, are relatively resistant to cracking under extreme conditions. This proves of great value on board a submarine, where space, storage capacity, and stability are at a premium.

The concept and creation of the castable propellant, capable of storage under a variety of conditions, can be traced directly back to that most mercurial of GALCIT team members, John Parsons. Yet it is a remarkable fact that Parsons' capacaity for discovery flourished in the most unpredictable of environments—that of tension and adversity. He seemed to thrive on uncoventionality. Personally an affable man, his behavior in most circles during the 1930s and 1940s would normally have resulted in his being shunned by his staid neighbors. In the more tolerant atmosphere at GALCIT under von Kármán and Malina, Parsons was accepted, even if he did occasionally raise a few hackles.

The dynamo of invention that so possessed the GALCIT team (though it would not have always seemed so to them) drew its energy from the constant underlay of conflict between theoretical modeling, traditional for an engineering school, and the free play of discovery and experimentation that

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board *Challenger* when it exploded in January 1986. The story of Aerojet General's founding, closely linked to the dénouement of the story told in this thesis, follows in Chapters 10 and 11 (see below.) For further information on the history of modern solid propellants, see Hunley, *Quest*, June 1998.

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characterized Parsons' and Forman's incessant rocket testing. Actually, there is nothing unusual about this: two complementary strains of research, theoretical design and intuitive insight coupled with experimentation, merge in scientific invention. John Parsons and Frank Malina not only tolerated each other's approach, but came to respect each other's talents and unique approach to problem solving. The two men relied on each other's energy and perseverance, despite clear differences in values.

The relationship between Parsons and Malina mirrored the growth of a synergistic bond between theory and practice at GALCIT. There were so many diverse minds at work on the team, it is a wonder the research did not spin off in any number of different directions. Yet, despite their widely divergent talents, and to some extent because of Frank Malina's will-power, the GALCIT Rocket team produced. No one man was responsible for the team's eventual success—a collective effort, the normal additions and defections which characterize any group process swelled and shrank its ranks.

Still, there were pairings, working alliances that gave the group its peculiar dynamism. Creation is like the swell and trough of a sea wave. It has its peaks and valleys. It is the continual stream of energy to the wave that counts. Undeniably, in Malina's case, the support net of his close relationship to von Kármán, his regular communication with his parents in Texas detailing his work, his new marriage, which despite its strains, gave him the stability he needed to pursue his research, and his belief in the originality and value of his work, all contributed to his vitality and productivity. [310]

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[310] In Liljan Malina Wunderman's two interviews with this researcher, she occasionally mentioned differences in personality and career objective between herself and Malina. Malina loved her artistic proclivities, but disliked her traveling to Los Angeles to art school.

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Malina was an artist at heart. Although an excellent, even exacting engineer, he nursed an urge to create with pencil, and later on canvas or even with metal and light. On some of his handwritten notes, written at JPL executive board meetings in 1944 and 1945, there are sketches and doodles of participants. In 1953, Malina became a kinetic artist, using light, mirrors, and paint to create works in what he called the *Lumidyne System.*[311] In Paris, 1968, he founded *Leonardo*, a magazine devoted to the arts. His paintings also remain in several private collections and at Texas A & M University.

Tsien, on the other hand, was a pure scientist, thoroughgoing, even obsessive in his attention to detail and in his powers of analysis. He loved music, was known to have hiked in Yosemite Valley with Malina in the 1940s, but devoted much of his intellectual life to science. Parsons, the most enigmatic of these three men, was an inventor nowhere nearly as well-schooled as Malina and Tsien, but no less an expert in his field. What he lacked in mathematical depth he made up in sheer insight, practical acumen and imaginative energy.

To varying degrees, these men all had the creative spark—even the craftsman, Forman. But Tsien, too, intense as he was in his calculations and

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According to Wunderman, she felt Malina expected her to assume the role of dutiful wife to the scientist. Differences over independence and life - orientation were amongst those that, by 1946, drove the couple apart. See: Wunderman interviews in 1996 and 1997 by this researcher. Copies have been placed in the Malina file at Caltech's Archives and at JPL.

311 *Obituary*, Frank J. Malina. In *Leonardo*, Vol. 15, No. 1 (1982), pp.i-iii. Malina developed several versions of his kinetic art. His Lumidyne System used "electric light shining through painted moving and static elements, sometimes with the addition of a diffusing screen." (*Obituary*, p.ii.) There are several boxes in the Malina Collection, Library of Congress, devoted to his art activities. See especially: Boxes 39-42. This copy of Malina's obituary, courtesy of Mrs. Marjorie Malina.

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his devotion to mathematics, could be found sitting quietly in a corner during a party, screening out all conversations, his mind focused on a Beethoven symphony rolling off a 78rpm disc in front of him.

The common thread for the entire group was a tolerance of risk and the excitement, despite the periods of intense frustration, even boredom, that came from working without guidelines from the past. They operated in a realm which could have brought them dismal failure. Often it did. But the challenge of their work, and the ostracism, derision they sometimes received from the professional academic community actually spurred them on. They shifted the scene of their operations, but not the thrust.

The previous chapter has revealed some of the shortcomings in the theory of rocket combustion as evolved by Malina, Summerfield, and Tsien. A major problem lay in giving turbulent fluid motion short shrift. Still, partial results lent enough credence to their theories and encouraged them enough to keep on. Only fundamentally novel approaches to rocket propellant combustion and the recognition of key problems to be tackled led to success. Primarily, in the realm of solid propellant mixtures, no powder, whether black powder or nitrocellulose, would produce satisfactory results.

Long-term rocket motor performance depended upon stable burning characteristics, and these were not possible using grains that varied in purity, shape, and consistency. In addition, powders are hydrophilic, and it was impossible to create a packed powder charge that would withstand temperature and humidity variations. The solution to these flaws in the chemical and physical properties of powder propellants lay in an entirely new

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compound, an entirely new mixture with a uniform consistency and sensitivity to airborne water molecules.

In the area of liquid propellants, the team realized, especially under Summerfield's astute eye, that the key to discovering an appropriate propellant lay not only in considering the factor of specific impulse, *I*, but in reducing the sequence of ignition events to a bare minimum. The discovery of the aniline-red fuming nitric acid (RFNA) combination, a product again of Parson's fertile mind, opened the door to creation of a whole new class of liquid propellants. True, these motors lacked some of the thrust of an alcohol-liquid oxygen combination (chosen by the Germans), but they reduced destructive throbbing to a minimum. Success came as a result of finding satisfactory chemical and engineering solutions to intransigent problems. [312]

What was the nature of the relationship between theory and experimentation for the GALCIT Rocket Research Team? What sinews bound engineering theory to the construction of rocket motors, whether successful or not? Theory may define behavior, within designated limits, but experimentation unlocks the door to actual evolution of behavior. In the case of Robert Goddard, a physicist by profession, early rocket construction did not precede theoretical investigation. In Goddard's 1919 *A Method of Reaching Extreme Altitudes*, the Massachusetts rocket scientist described theoretical work done at Princeton University in 1912-1913, fully two years before he began experimenting at Clark University in 1915-1916. In Goddard's own words, "the original calculations were then repeated, using the data from these

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312 The thrust of the German A-4 motor (more popularly known as the V-2), powered by an alcohol-liquid oxygen combination, was approximately 56,000 lbs. See: Neufeld (1995), p. 281.

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experiments." [313] This comment, in fact, is an admission that Goddard's original theoretical work was far from adequate. Only experimentation can provide the kind of raw data capable of refining theory to the point where it fits real performance.

There is no arguing with the fact that the GALCIT engineers tried to plumb the dynamics of rocket motor combustion while simultaneously designing and testing rocket motors. It would be easy—but incorrect—to attribute this simultaneity of experiment and theorizing to the predominant team effort involved. Goddard was working alone for the most part—at least before the 1920s—but despite the collaborative tenor which marked GALCIT innovations, theoretical investigation and experiment occasionally seemed two polarized threads.

Two members of the group placed little value in pure theory and continued experiments begun even before they had joined the GALCIT team. In truth, however, Goddard's theoretical work, involving not only a re-statement of Newton's Third Law but a measure of the propulsive characteristics of black powder and nitrocellulose, depended upon prior chemical analysis and experimentation. Just as, in the history of science, there are significant accounts of theory as the foundation stone of experiment—for example, thought experiment as the germ of the Theory of Special Relativity—it is equally true that Einstein's *gedankenexperiment* had deep roots in more than the theoretical work of Ernst Mach (1838-1916) and James Clerk Maxwell

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313 Goddard, *A Method of Reaching Extreme Altitudes* (1919), in Logsdon, *et al* (1995), p. 86.

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(1831-1879). [314] After all, Michelson and Morley had torn off the emperor's new clothes in 1887 with a key experiment disproving the ether hypothesis. [315] Experiment frequently provides the impetus for theory destruction and creation.

Clearly, Forman and Parsons resisted efforts to halt their work while the group struggled to develop an adequate theory defining the process of combustion. In retrospect, there was no reason why they should have stopped their independent test firings. Malina was concerned about maintaining a proper respect for theory in an academic world he saw as demanding publishable mathematical definitions first. He believed that solid theoretical understanding of the combustion process had to underlie experiment. But in fact, the insistence of Parsons and Forman that tests continue actually contributed to Malina's accelerating the construction of the team's first liquid fuel motors.

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[314] For an illuminating treatment of the development of Einstein's Theory of Special Relativity, see Gerald Holton's *Thematic Origins of Scientific Thought, Kepler to Einstein.*, Cambridge, Massachusetts, Harvard University Press, 1973, especially Chapters II and III ('On Relativity Theory' and 'On the Growth of Science.')

[315] The Michelson-Morley experiment provides a good example of experimental reversal of a cherished hypothesis. Albert Michelson's interferometer, designed to measure the wavelength of light, should have demonstrated a shift in wavelength relative to the assumed ether drift. As the earth passed through the invisible, insensible ether, light should have slowed just a bit and produced a fringe shift in a detector equal to some appreciable multiple of the actual distance the light had traveled. Yet, there was no true light shift. The ether, if it did exist, had no effect on the motion of light relative to a moving body such as the earth. The results of this experiments eventually forced physicists to abandon the concept of the ether entirely. A clear exposition of the Michelson-Morley experiment appears on pp. 1036-1038 of Hugh Young's *University Physics, 8th edition.* Reading, Massachusetts, Addison-Wesley Publishers, 1992. For a somewhat more historical treatment, see: Abraham Pais. *Subtle is the Lord... The Science and Life of Albert Einstein.* Oxford, the University Press, 1982, pp. 112-113.

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There is little doubt that Malina, Forman, and Parsons would have foregone going to junkyards in the Pasadena area in the fall of 1936, little more than six months after the formation of the team, if Malina had not sensed the uselessness of trying to control Parsons' urge to fire off rockets; it was more politic to acquiesce in the effort to build, rather than simply study. Malina, too, was anxious for results. He had gotten the go-ahead from von Kármán, had committed his career to rocket work, and was clearly of a mind not to turn back. The team had little money, and so the issue was strictly one of scientific curiosity and personal commitment. Besides, if facing too much resistance, Parsons had backed out of the fledgling group, Malina knew he would have lost an insightful chemist and pyrotechnics expert, and his own plans might then be in jeopardy.

In his memoirs, Malina referred more than once to his insistence on theoretical groundwork before testing. But the reality was far from this simple strict adherence to engineering school protocol. In a sense, there were two threads of research winding forward simultaneously: the small knot of men who clustered their training in engineering and calculus, who focused the work of the GALCIT group on designing a rocket motor, rather than an entire rocket; and the party of two—Parsons and Forman—who built, exploded, and occasionally flew rockets, whatever the extant state of theoretical progress.

Rocket theory for the GALCIT Rocket Research Team developed in something of a cave. There were key elements to the theory that lay undefined, or which referred to characteristics of motor performance that

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were only incompletely understood. The dynamics of rocket combustion began to fall into place slowly, piece by piece. A good example of this slow accretion of a full conceptual framework is the formulation of the characteristic velocity, c^*, parameter by Walter Powell (1917-1994) and Mark Mills (1917-1953), both of GALCIT, in 1941. [316]

According to Malina, "Characteristic velocity is determined only by the properties of the propellant and the nozzle throat area." [317] But measuring c^* then depends upon chemical analysis and the mathematical design of the combustion chamber, particularly its narrowest section, where gas accelerates from subsonic to supersonic velocity. At first glance, it would appear that, in order to properly define this quantity, the talents of both chemical assayer and physicist-engineer must combine. A concise mathematical definition of c^* follows:

$$c^* = \frac{p_c A_t}{m}$$

where p_c = chamber pressure
A_t = the inner surface area of the combustion chamber at the throat (its narrowest section), and
m = the mass of propellant (liquid or solid)

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316 Walter B. Powell received a degree in aerodynamics from GALCIT in 1940 or 1941 and was quickly hired by Malina to work on the rocket team. Mark Mills completed a Ph.D. in physics at Caltech in the early 1940s and continued to make seminal contributions to the theory of rocketry until his untimely death in a helicopter crash in 1953.

317 Malina (1969), p.162. The term c, as defined by Howard Seifert, Mark Mills, and Martin Summerfield (all of GALCIT), distinguishes itself from the general term v (velocity) because it represents the effective exhaust velocity, a combination of specific impulse and gravitational force [$c = gI_{sp}$.]. See: Seifert, Mills, and Summerfield, *The Physics of Rockets* Reprinted from the *American Journal of Physics, Vol. 15.* Nos. 1, 2, 3 (January-June 1947.) Pasadena, JPL Archives. Document No. HC 9-187. Howard Seifert (d.1976) received his Ph.D. from Caltech in 1940. He worked extensively with Martin Summerfield and was chief of JPL's Liquid Rocket Section from 1942-1947.

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This equation indicates how important the nozzle throat area, A_t, is. For, at a specific value of A_t, c^* depends crucially upon chamber pressure and the quantity of burning propellant. Evidently then, the larger the propulsion chamber, the greater the mass of propellant burned and the higher the characteristic velocity. On the face of it, this seems to contradict Summerfield's 1942 discovery that small volume chambers burn fuel more efficiently than do large ones. But a more thorough treatment (1979) of c^* describes it as solely dependent on the nature of the propellant and disregards the chamber size. [318] In other words, the degree to which hot gas in the chamber expands is more a function of the expansion coefficient of the gas itself, rather than the chamber volume. [319]

Even though Powell and Mills imprecisely defined characteristic velocity, they had more deeply explored two crucial parameters of rocketry: the pressure of the burning gas inside the chamber and the size of the exhaust nozzle. If one could measure c^* for specific fuels, then choosing an optimal fuel for specific thrust requirements would become much easier. Neither Mills nor Powell immediately published the results of their 1941 work on characteristic velocity (they were both fresh out of graduate school at

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318 Cornelisse, *et al* (1979), pp. 118-119.

319 The expansion coefficient of a gas, for real gases known as the Joule-Thomson coefficient, measures rate of change of temperature/rate of change in pressure, given an adiabatic state. (Adiabatic refers to no loss of heat from the internal system.) Technically, a rocket is constantly expelling hot gas through its nozzle and so the process is not adiabatic at the nozzle; but within the combustion chamber, energy is neither gained nor lost so long as fuel pumps in and is consumed. The important point is that the internal energy and enthalpy of the system are independent of the container vessel's volume— so long as it is constant— and solely dependent on the heat capacities of the fuel and oxidizer, that is, on the internal molecular dynamics of the system. See: Gordon M. Barrow. *Physical Chemistry, 3rd Edition*. New York, McGraw Hill, 1973, pp. 124-135.

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GALCIT and working as part of the rocket team); but Mills' contribution to *The Physics of Rockets* (1947) clearly ruled out a purely mathematical analysis of the exhaust nozzle's design: "The geometry of the orifice through which the compressible gases escape is important in determining both mass flow and thrust. It is not evident *a priori* what the shape of this 'nozzle' should be." [320]

Malina confirmed this observation with his 1969 comment that Powell and Mills had invented the parameter c^* during a 1941 series of experiments with liquid propellants. [321] Whether or not Mills began his research into nozzle design with a mathematically derived image of the proper size, angle of deflection of the nozzle cone, etc., remains unknown, but surely he and Powell tested nozzle designs in actual rocket motors.

Mills and Powell also further refined the definition of the thrust coefficient, C_f, another key determinant of total thrust. The thrust coefficient relates rocket motor thrust in a vacuum to the actual supersonic flow and pressure of hot gases within the combustion chamber; C_f measures the actual force of exhaust gases as they exit the nozzle and is strictly determined by the area of the nozzle, particularly its throat. [322]

$$C_f = F/p_cA$$

where F= Force (velocity of the gas in a vacuum)
p_c = again, chamber pressure, and
A = the surface area of the inside of the combustion chamber.

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320 Seifert, Mills, and Summerfield (1947), p. 5.

321 Malina (1969), p. 162.

322 Cornelisse, *et al* (1979), pp. 114-119.

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The total specific impulse, I_{sp} (a measure of the exhaust velocity of the motor, per pound of propellant), then is an additive quantity, determined by a parameter strictly mechanically defined (C_f), and one whose origin is chemical (c^*).

$$I_{sp} = c^* + C_f.$$

However, this quantity has limits to its accuracy, as the exact qualities of a propellant may vary, even when properly assayed, and the precise thrust coefficient for a particular motor can only be determined experimentally over a series of trials.

The concept of specific impulse was not new to GALCIT engineers, however. The Russian theoretician Konstantin Tsiolkovsky (see above) had worked out a basic formula for I_{sp} in May 1897, while writing his *Exploration of Cosmic Space by Means of Reaction Devices*.[323] Tsiolkovsky's equation for the efficiency of "explosives" in a gravitationless medium was not quite the equivalent of its later version. The Russian scientist's statement of what later became known as specific impulse was:

$$T = V^2/2g$$

where T = work done in a gravitationless medium,
V = the velocity of the rocket in such a medium, and
g = the gravitational constant.[324]

However, the present definition of $I_{sp} = c/g$ (c = effective exhaust velocity) differs, not merely by a factor of two, but also in its more complex

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323 Winter (1990), p.10. Tsiolkovsky's *Works on Rocket Technology* has been translated into English and is available from NASA. NASA Technical Translation NASA TT F-243. Washington, NASA, 1965. Tsiolkovsky's work is also included in Logsdon *et al*, eds. (1995), pp. 59-84.

324 Logsdon *et al*, eds. (1995), p. 75.

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assumptions about exhaust velocity; c = V only in space, for c is the ratio of gas pressure to atmospheric pressure. [325]

Such terms as *specific impulse* and *characteristic velocity* are key markers of the efficiency of rocket fuels and motors. Malina and Parsons—in fact, all rocket engineers—used them regularly (and still do.) Specific impulse is a measure of utmost concern to rocket science, as the choice of fuel determines the maximum thrust of the rocket and thus, how fast and far it will travel. [326]

The change in formulae defining I_{sp} over the span of 44 years (that is, from 1897 to 1941) indicates that understanding of basic processes became increasingly refined with time—and experiment. It was experimentation that determined the shape of the formalized mathematical statement, rather than the mathematical sentence created by the scientist. A long train of experimental knowledge lay like a mountain on the shoulders of GALCIT rocketeers. The very activity of Parsons and Forman defied the rationale of theory before practice.

Theorizing involves a vital struggle to build a logical statement—a sentence—about reality. Theoretical statements may define reality, elucidating fundamental laws and qualities of the physical universe, but they are also reflective of the mental structure which creates them. Certainly, it is difficult to quarrel with the truth of $F = ma$, Newton's Second Law of Motion, although physicists have refined this statement several times since the 17th

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325 Francis E. Riley and J. Douglas Sailor. *Space Systems Engineering*. New York, McGraw Hill, 1962, pp. 102-104.

326 A glossary of rocket engineering terms may be found in Appendix II of this thesis.

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century, in order to more precisely fit different phenomena (such as harmonic motion or electrical force.) Nevertheless, the quality of mass *(m)* resists complete definition, unless it is referred to some inertial system. Each time a qualitative, linguistic substitution in the original formula appears, the equation takes on new meaning. Its essence remains, but the expression has metamorphosed. In some cases, precision of meaning is enhanced; in others, meaning becomes more obscure, because added symbolization has layered on nuances which obfuscate rather than enhance the clarity of basic laws.

Theorizing about physical reality involves abstracting it, creating a linguistic convention—commonly recognizable—and assuming that the language and the real world are virtually the same. But are they? To assume so would be to mistake mental process for actual fact. Words create vivid images of the world, but they are not the world.

One fact is clear: early rocket theory was somewhat stochastic in nature. The degree of imprecision depended upon the measurement of several *dissimilar* but combined parameters. The edifice of theory constructed itself as it developed. Deriving theory based upon the assumptions of propellant chemistry or the mathematics of ideal shape and size of key motor components could be risky. Not enough was known about the relation between the two (or more) parameters to make meaningful predictions that mirrored the actual experimental state.

The fact that theory and experiment possess fundamentally different frameworks does not necessarily infer that experiment always—or even frequently—provides definitive answers to questions about physical law or

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phenomena. Thomas Brody's *The Philosophy Behind Physics* casts doubt upon the uniqueness, the absolute reliability of experimental data in general.

> In order to obtain meaningful information, a measurement (or observation) is not enough. It is necessary to prepare the physical system to be studied in an at least partly known state, then to let it interact with known forces (due to appropriately determined fields, or to other systems in a correspondingly prepared state) and finally to carry out one or more measurements; and the result of the experiment is not the measured value but its correlation...with the state preparation. [327]

Experimental results are just as much a reflection of the definition and structure of the experiment as they are the product of independent facts. This, of course, is not a new assumption, but is certainly relevant in the case of the work of GALCIT rocket engineers. There is a certain latitude, a range of values, within which experimental results can fall and still be considered reasonably valid. However, as indicated earlier, there may be observed phenomena—actually manifested within the framework of the experiment—whose behavior is not explained or recognized within the boundaries of the experimental system. Heisenberg put it succinctly: "Every experiment destroys some of the knowledge of the system which was achieved by previous experiments." [328] Experimental design, whether the product of pure imagination or known models (that is, socially constructed, to some extent) can surely produce permanent insights, and it is wise to remember Sir Francis Bacon's advice: by forcing Nature we help her to reveal her truths. [329]

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[327] Brody, Thomas. *The Philosophy Behind Physics*, eds. Luis de la Peña and Peter Hodgson). New York, Springer-Verlag, 1993, p. 240. By "state preparation" Brody means the design and construction of the actual experimental apparatus. Brody critiques the empiricist approach, which "created a false image of the process of knowledge acquisition as essentially passive, as a mere receiving of sense information, from which the outer world has somehow to be built up." (p. 240.)

[328] Werner Heisenberg. *The Physical Principles of the Quantum Theory*. New York, Dover Publications, 1949, p. 20.

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Yet while this is so, we still define these truths in terms that are not Nature's inventions, but our own.

Mathematics can define the behavior of a real system, within acceptable conventional limits; if the actual experimental results fall outside these limits, then either the experimenter disregards the results as invalid or irrelevant—or accepts the results and revises the mathematical structure. A certain lack of precision must therefore be built into any mathematical construct of reality. As Einstein has written, "As far as the propositions of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality." [330]

The mathematics of rocket motor dynamics, as defined ideally by von Kármán, Tsien, and Malina in the 1930s and 1940s are nevertheless valid, insofar as they define the limits of the engineer's expectations. They are taken only as approximations of real behavior: a kind of wish list. Clearly, the science of rocketry was in its infancy in those decades, despite the work of Tsiolkovsky, Goddard and Sänger. For a new endeavor, a field whose theory is still incompletely understood, it may be enough to sketch the outline of expected behavior, rather than the reality of experimental results. This sketch might just be enough to propel investigations forward. The details could be refined as the theory evolves with experimentation.

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329 Sir Francis Bacon. *Instauratio Magna* (The Great Instauration), 1620. In: *Francis Bacon, A Selection of His Works*, ed., Sidney Warhaft. Indianapolis, Bobbs-Merrill Educational Publ., 1965, p. 320.

330 Javier Echeverria, Andoni Ibarri, and Thomas Mormann (eds.) *The Space of Mathematics: Philosophical, Epistemological, and Historical Explorations*. Berlin, Walter de Gruyter, Publ., 1992, pp. 141-142. This statement serves as a reminder of the Heisenberg Uncertainty Principle.

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This is a crucial point. The theory of rocket dynamics as developed at GALCIT sketched the operation of rocket motors in ideal terms. Many complicating factors were assumed to fade from the picture—or more precisely, Malina and von Kármán ignored them. The theory, however, provided impetus for further experimentation and this experimentation was eventually productive. If the incompleteness or even inaccuracy of the theory is accepted as a given, then it is possible to ascribe to it limited success. GALCIT's JATO units worked, after repeated trials and changes to the propellant chemistry. If they had not, it would have been impossible to found a corporation (Aerojet) to produce JATOs and convince the Army Air Corps and the US Navy to purchase them. The theory did not fit reality well; but experimentation proved the worth of theory as a guide.

What is the bridge between an incomplete, imperfect theory and experimental success? This bridge rests on a foundation of insight, determination and a repeated process of trial-and-error. The structure of scientific discovery builds upon the recognition that there will always be some limit to the accuracy of the experiment (if for no other reason than that it has altered reality in the process of studying it). If experimental results exactly confirm theoretical predictions—and this does happen—then a felicitous conjunction has occurred. But as Brody has suggested, experimental design can occasionally fall into the track of running a course and winding up right back at its starting point: that is, the preparation of the theoretical groundwork for experiment can confirm its own necessary definitions and conditions, rather than reveal a new reality. Beyond even this lies another insight: order in the universe is a multifaceted affair.

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In an insightful essay, 'The Role of Mathematics in Physical Science,' (1992) Erhard Scheibe mentions a comment by the physicist Stephen Weinberg:

> Mathematics is the science of order, so perhaps the reason the mathematician discovers kinds of orders which are of importance in physics is that there are only so many kinds of order. [331]

Scheibe speaks of the "miracle" of mathematical discovery of scientific truths, even before empirical confirmation confirms them. This association of mathematics with truths of the natural or real world is the source of von Kármán's, Malina's and Tsien's firm belief in the soundness of mathematical theory. Suppose that there *is* a finite number of levels to the inherent order of the universe but that physicists have not yet uncovered them all. It is still possible that new layers of physical order, or disorder, for that matter, underlie those presently known.

The manner in which mathematical theory defines physical law and the behavior of natural phenomena involves a layering or recombination of symbols, as occurs in any language. By connecting symbols in specific ways, new levels of meaning occur to those who can decipher the language. However, there is a dizzying number of possible logical arrangements to symbolic language, and each has attached to it a specific meaning.

By referring to the relatively large lexicon of terms rocket scientists use to define the processes they are studying, it is evident that there is a plethora of meanings associated with the same symbols. For example, λ has four different meanings to the engineer: it can refer to 'payload ratio,'

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331 Erhard Scheibe. 'The Role of Mathematics in Physical Science.' In Echeverria, Ibarra, and Mormann (1992), p. 142.

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'wavelength,' the 'nozzle reduction factor,' or to 'celestial longitude,' depending on its context. [332] Each time the symbol finds its way into a specific equation (within a specific logical context) it assumes a particular meaning which is unconsciously and automatically understood by the literate reader. Even if there are a finite number of symbols, there is a much larger number of possible ways to re-shuffle them, as if they were anagrams. This assumes, of course, that mathematical symbols, as with any language, have recognition value on one level and a deeper conscious import on yet another. [333]

Then, in order for a mathematical equation to approximate or even exactly define real phenomena, there must be a conjunction on three fronts: first, the precise combination of symbols must possess a definitive and unique ordering, one which makes a logically recognizable statement. Secondly, of all the possible ways in which the statement may be ordered, a specific example must conform (at least somewhat precisely) to the sequence of steps in which phenomena occur. Finally, the cognitive link between symbols must bear some resemblance to reality. (If $F = ma$, then mass and acceleration must bear some definite relation to each other.)

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[332] Cornelisse, *et al.* (1979), p. xx. The *payload ratio* is the ratio between the mass of the rocket under acceleration and its initial mass on the ground. The *nozzle reduction factor* is the degree to which a rocket motor nozzle curves in toward the throat, its narrowest point, and then diverges out to the exit rim.

[333] For a further discussion of the deep structure of language, see: Noam Chomsky, *Language and Mind*. New York, Harcourt Brace Jovanovich, 1972. Ideas of the superficial and deep structure of language, mentioned above, reflect Chomsky's insights. Chomsky also believed in an innate mental substratum responsible for incipient language development.

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The question is, does the number of possible combinations of symbolic statements parallel the number of observable phenomena in the universe? There may be a finite (though immense) number of symbolic 'sentences,' each with its own particular meaning. On the other hand, there may just be an infinite number of phenomena in the universe. When a mathematical equation accurately defines some physical state, the equality of logical sentence and physical reality exists—and that could be, as Scheibe says, a "miracle." Yet, not such a miracle as to have irrational roots. It is, in effect, a miracle of logic.

One possible solution to the conundrum: does the number and type of phenomena equal the number of symbolic sentences or sequences humans may employ to describe them?—rests in the notion that there are discrete boundaries to every mathematical (and linguistic) system; once these boundaries are crossed, one must create a new system with its own newly defined limits. Simultaneously, there are layers of physical reality which are discrete, with transfer boundaries between them. When crossing from one genre of physical system to another, one must re-define the terminology with which one approaches these new phenomena.

This is precisely why, in defining rocket motor dynamics using idealized equations with a given set of symbolic terms, Frank Malina and Theodore von Kármán found it necessary to confine their explorations to a restricted set of parameters. The language of their mathematics did not fit the new realm of phenomena they were investigating. To delve into the world of turbulent—even chaotic—fluid dynamics would have required a new

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mathematical terminology. This they did not possess and so the language of their theory did not fully conform to the new reality. Rocket motor dynamics crosses a threshold—from stable molecular and macroscopic behavior to instability. The threshold varies with each type of propellant, motor design and performance, and so the point at which turbulent gas behavior sets in cannot be universally predicted.

Simply put, experimentation more definitively mirrors and illumines the actual behavior of a rocket motor. There are still the same caveats that pertain to theory: one should respect the inherent limitations of an experimental apparatus. It is the product of a thinking being, working in a particular historical and technical context.

In the search for experimental proof, there is an important catalyst at work propelling discovery's momentum forward: the creative mind. The creative spark can inhabit both theoretician and experimenter. Brody defines creativity in an unusual way: as a logical mental system that provides alternative solutions to problems when standard solutions don't quite fit. In his own words: "We consider creative an entity which produces more than one idea for each idea fed into it." [334]

Brody's somewhat dry exploration into the realm of logic and imagination likens creativity to information processing. But to reduce the creative process to an input-output neurological mechanism seems to strip away its richness and complexity. The impulse to see and frame reality from a variety of angles, to make associations with past experience and study makes the

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334 Brody (1993), p. 282.

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creative method a more effective tool. There is an historical element, as well—one may refer to knowledge of invention from the long dead past—as was the case with John Parsons' awareness of 'Greek Fire.' [335] A different insight into creativity comes from Marriane Moore (1887-1972), who wrote that poets present "imaginary gardens with real toads in them." [336] The 'new' rests on a foundation partly intangible and partially solid fact. What is striking about Moore's comment is that the real, the factual are perceived as such by the mind, but are imbedded in a web of imagination.

Parsons' most famous creative inspiration was his discovery one day late in 1941 or early 1942, while walking down a street in Pasadena, that common paving asphalt might serve as a castable rocket fuel. There are two variants of the story: first, that Parsons turned and watched as roofers tarred a house—this is the story as it usually comes down to the present, based upon Lieutenant Homer Boushey's memoir; and secondly, there is Martin Summerfield's recollection of Parsons' insight coming after he saw workers laying hot asphalt on a street. [337]

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[335] Greek Fire was an incendiary mixture which included saltpeter, used by the Byzantines to set fire to enemy ships. Greek Fire had several variants. Some sort of incendiary arrows were in the Greek arsenal as early as the 4th century, B. C., according to J. R. Partington. By the late 4th century A. D.,Vegetius described an incendiary mixture composed of sulfur, resin, bitumen, and tow soaked in incendiary oil (probably petroleum.) See: J. R. Partington. *A History of Greek Fire and Gunpowder.* Cambridge, W. Heffer and Sons. Publ., 1960, p. 2. Parsons and Mills describe asphalt as "a semi-fluid mixture of heavy hydrocarbons containing a large percentage of carbon. After the unsaturated carbons have been oxidized, the properties of asphalt change very little with time. It has been used since antiquity as a binding, caulking, and cementing material. It is supposed to have been an ingredient of Greek fire." Parsons and Mills. 'The Development of an Asphalt Base Solid Propellant,' *Air Corps Jet Propulsion Research, GALCIT Project No. 1, Report No. 15,* October 16, 1942. California Institute of Technology. JPL Archives Doc. # Microfilm 10-94 , p. 11.

[336] Rudolph Arnheim. *Art and Visual Perception, A Psychology of the Creative Eye.* Berkeley, University of California Press, 1969. p. 291.

[337] Homer Boushey Jr. ' A Brief History of the First US. JATO Flight Tests of August 1941:

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The building blocks of Parsons' invention were his detailed knowledge of the chemistry of combustible materials, his knowledge of Greek fire, and his creative imagination. All these strengths contributed to his key discovery.

Parsons was not the type of man to be stymied for long by frustration. There is no question but that he had reached an impasse in the fall of 1941, when after several tests of JATOs equipped with nitrocellulose/nitroglycerin (double-base) powdered propellants, he realized that powder propellant rocket units could not be reliably stored for more than a few days, or under variable temperature conditions, and be expected to perform satisfactorily. [338] The charge tended to crack and would then have to be discarded. Its use under an airplane's wing could result in failure or worse, explosion. Even for JATOs packed and assembled the night before installation, if the temperature were below 40° F, the powder's performance (i.e. its specific impulse) markedly decreased. The Army Air Corps had already voiced interest in the JATO's mass production. If the GALCIT Rocket researchers could not deliver reliable JATO units, a lucrative contract would be lost and the crew would miss a chance to be of service to the nation, while war clouds grew over the Pacific.

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a Memoir,' presented at the International Astronautical Association Meeting, Stockholm, 1985. *IAA Series, Vol. 14*, San Diego, Univelt, 1993, pp. 127-137. Martin Summerfield's recounting of the tale is mentioned earlier in this thesis. See above, p. 90. The exact date of Parsons' discovery has never come to light. He did not begin thinking about alternative propellants until after the August 1941 JATO tests in which Boushey participated.

338 The term *double-base* propellant does not refer to the chemical's basicity, but rather to the combination of two explosive components, either or both of which contain an oxidizer in the same molecular chain as the fuel. The fundamental components are nitroglycerin and nitrocellulose.

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Parsons, however, had been struggling with the problems of rocket building since the early 1930s and was no stranger to the design and mechanical problems shadowing rocketry. Parsons also worked in a tolerant environment at Caltech. Both Malina and von Kármán knew well his personal eccentricities, considered them occasionally annoying, but believed they were a stimulus to his creativity. [339] In fact, when directly asked by an interviewer in 1973 to speculate on whether or not Parsons resembled the archetype of the medieval alchemist, Malina replied that he had doubts about the strict relation of magic to Parsons' creative inspiration.

> I am not sure this is applicable, but I would say that I don't think so. I really think that sex played a role, like it does in so many of these activities. He was rather freewheeling in some of these attitudes....But as far as I can see I cannot remember ever talking to him about some calculations on rocket design [and] ever having an input from...alchemy or magic....It seems to me he functioned in these compartments. [340]

Although perhaps a speculation on Malina's part, there is no way to prove a link between Parsons' *libido* and his creative spirit. Malina undoubtedly knew of Parsons' eccentric lifestyle, which included black magic rites and sexual excess, conducted at his home on South Orange Grove Avenue. [341] More to the point is that Parsons saw himself as opposed (even hostile) to any

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339 Some of Malina's own comments on creativity are worth noting. Malina wrote on the relation between science and creativity.: "In order to make a particular creative contribution, a person must be born at the right time, with appropriate genetic characteristics, in a stimulating cultural environment and be blessed by good luck. The low probability of the coincidence of these factors probably accounts for the rarity of genius." From: ' Reflections of an Artist-Engineer on the Art-Science Interface.' *Impact of Science on Society*, Vol. XXIV, No. 1 1974. p. 25.

340 Wilson Interview with Malina (1973), JPL Archives, p. 24.

341 Liljan Malina Wunderman attended one of these sessions at some point in the early 1940s, having been invited by Parsons while Frank Malina was away from Pasadena.

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form of authority or restriction on his freedom. In a collection of his writings edited by his first wife and published posthumously, Parsons wrote that "No man, no group, and no nation has the right to any man's individual freedom. No matter how pure the motive, how great the emergency, how high the principle, such action is nothing but tyranny. It is never justified." [342]

It may well be that Parsons' resistance to the conventional rules of scientific exploration were connected to his belief that individual and personal freedom was not to be limited. Parsons was not trained in the logical rigors of mathematics, and apparently had no commitment to any theoretical justification for his scientific endeavors. Yet, he was capable of storing and categorizing large amounts of chemical data. No undisciplined mind would relish such a task. Malina commented that "Jack in the group served as the source of information, one, on chemistry, and two, he was very knowledgeable about fireworks and sky rockets type of things which turned out to be very valuable." [343]

This comment is revealing. Jack Parsons complemented his creativity with a prior bank of knowledge. Parsons was able to compare old possibilities in his mind, turn them over and reject them, and discard the old with some new

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[342] Parsons, John Whiteside. *Freedom is a Two-Edged Sword* .Eds. Cameron and Hymenaeus Beta. New York, Ordo Templi Orientis. Las Vegas, Falcon Press, Publ. 1989. p.15. In a revealing comment, Parsons wrote in this same collection of essays that "If a liberal were to develop two personalities, and one of these personalities developed a benevolent dictatorship while the other continued his liberal activities, it would be only a matter of time until he killed himself. " (Parsons. p. 14.) Whether or not Parsons was writing of his own conflict here cannot be known. He could energize himself to work in a disciplined manner on rockets—he had the drive of a scientist on the frontier of knowledge. On the other hand, another side of him was far less disciplined. It is intriguing that in 1952, a man so highly skilled in handling explosives should have been careless enough to let a vial filled with fulminate of mercury fall from his grasp.

[343] Wilson Interview with Malina (1973), p. 25.

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information he had collected. In order to imagine a more innovative oxidizer for his asphalt-based propellant, he had to discard the saltpeter (KNO_3) oxygen source commonly found in black powder rockets.

This imaginative choice could only have come from a secure knowledge of the chemical properties of compounds known to be oxidizers. Parsons was evidently comparing various known characteristics of potassium perchlorate, particularly its heat of reaction and the amount of energy per mole needed to release the oxygen from its bond with potassium. [344] He also must have considered its solubility in various substances and its boiling point. Parsons needed to know whether or not $KCLO_3$ would combine well with asphalt. Would enough free oxygen be available to run the combustion reaction at sufficient thrust? To answer these questions, Parsons experimented with a variety of mixtures and consulted with Dr. Bruce Sage of the Caltech Chemistry Department, who knew the chemistry of these compounds in detail and could assay them in his lab. [345]

The essence of Parsons' creative burst in this case was not a 'bolt from the blue,' but his apparent willingness to try several different possibilities until he had discovered a promising one. In fact, the new propellant, GALCIT- 53, as advantageous as it was from the viewpoint of storage and reliability, had a lower specific impulse than did the powder-base propellant, ballistite (a

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[344] The heat of reaction for a substance or group of substances is the amount of energy absorbed or released as the reaction runs. A mole, or Avogadro's number, = 6.023×10^{23} atoms or molecules of any substance.

[345] Bruce Hornbrook Sage (1909-1983) was a professor of Chemical Engineering at Caltech and a technical advisor, both to the GALCIT Rocket Group and to Aerojet-General from the late 1930's until 1969. He performed many of the detailed assays on solid propellants, liquid propellants and oxidizers for the GALCIT team.

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nitrocellulose double-base powder). [346] Ballistite's specific impulse was 200 lb/lb.sec., while GALCIT- 53's specific impulse was 170 lb/lb-sec. This meant a difference in exhaust velocity of 1000 ft/sec., assuming comparable operating temperatures. [347] The problem with ballistite was its temperature sensitivity. As the ambient temperature dropped from 90°F to 40°F, the thrust would shrink from 1000 lbs. to 600 lbs. A comparable temperature drop would only result in a 100 lb. thrust loss for GALCIT- 53.

However, GALCIT- 53 was not exactly the ambrosia that Parsons and the US Navy—both interested in the propellant for use in booster rockets—desired. The Army Ordinance Department had originally objected to potassium perchlorate as an oxidizer because it proved unstable unless it had a minimum purity of 99%. Parsons knew that a pure compound could be produced and tried to demonstrate to the army that GALCIT- 53 was safe. So long as he could purchase $KClO_3$ with nearly 100% purity, Parsons could assure the armed forces experts that his new propellant would not explode or burn unevenly.

Parsons succeeded in producing GALCIT- 53 of sufficient quality and the ordinance department dropped its objection. Yet there was still one major drawback: smoke. The new JATO units, 100 of which had been constructed for the navy by 1942, produced reaction products which gave off "dense clouds

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346 Nitrocellulose powders are polymers derived from cellulose and treated with sulfuric and nitric acids.

347 Parsons and Mills (1942), Table I, p. 4. Ballistite's exhaust velocity was 6300 ft/sec. while that of GALCIT 53 was 5300 ft/sec. However, GALCIT- 53 burned more slowly than did ballistite, assuming a chamber pressure of 2000 lb./square inch.

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of white potassium chloride smoke." [348] Unless this smoke rapidly cleared, navy men stationed on a carrier deck would be unable to come anywhere near the trail of planes launched from the deck.

As a result Parsons changed the oxidizer's composition to ammonium perchlorate and then invited navy experts to Pasadena for a demonstration. Malina picked up the thread of the story:

> When [the Navy men] arrived we posted ourselves some distance from the test pit, the red flag was run up, and Parsons gave the order for his latest creation to be fired. We beheld a big cloud of white smoke and Parsons with a look of surprise on his face. He sheepishly explained that the smoke must have been caused by the humidity, for the air had been very dry on the days they had made tests before. Ammonium perchlorate does reduce the amount of smoke produced if the air is dry, but it produces undesirable chloride in the jet. [349]

The problem of chloride exhaust was never solved, and the navy learned to live with it. But Malina and Parsons were not satisfied and continued to experiment with other compounds. In 1942 and 1943 the GALCIT project investigated napalm gel, gelled wax (paraffin) and butyl rubber. [350] Eventually *thiokol* came along in 1945. *Thiokol* replaced asphalt-based propellants and became the foundation stone of all solid propellant casts used in US booster rockets. [351] The advantage of a polysulfide rubber propellant is

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[348] Malina (1969), p. 174.

[349] Malina (1969), p. 174.

[350] The final product—a butyl rubber derivative—was the product of reaction compounds including di-chlorides and sodium polysulphates. This was *thiokol*, a mixture of ethylene compounds CH_2-CH_2, etc. and sulfates. See: John A. Dean (ed.) *Lange's Handbook of Chemistry, 11th Edition.* New York, McGraw-Hill. 1973, p. 7-452.

[351] Malina (1969), p. 174. For further information on the development of *thiokol*, see J. D.

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a higher specific impulse—on the order of 230 lb.-sec./lb., rather than approximately 180 lb-sec./lb. for GALCIT- 53. The rubber compound also has a wider storage temperature range than does asphalt.

The hidden element in this tale is the experimental process to which Parsons adhered in inventing, not only GALCIT- 53, but a later variant, GALCIT- 61, and in testing other synthetic propellants. Every time Parsons fired off a rocket, or more accurately, misfired one, his mind began to turn on what he could do to improve the next test. He was a keen observer and knew the difference between a tight, safe experiment and a reckless one. [352] After a number of tries, if his experiments failed to perform, he would logically comb his knowledge bank for alternative fuels or oxidizer combinations.

Parsons was resourceful. Malina commented that Parsons developed GALCIT- 53, "no doubt after communing with his poetic spirits." [353] But Malina undoubtedly knew better. Parsons may have been inspired by the vision of roofers or street tar layers, but that image set his mind thinking along lines developed during years of practice and first-hand knowledge of pyrotechnic chemistry. His creative imagination was well-grounded in practice and in the source-book of chemical information he carried around in

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Hunley (1998), pp. 3-10.

[352] In May 1942, Parsons visited New York in order to witness some liquid fuel rocket tests by an amateur rocketeer named Maynor. Maynor had set up his apparatus along the Harlem River, and was testing a fuel combination of acid and benzene . Parsons commented in a letter to Malina (May 5, 1942): "His measuring apparatus is primitive and I believe his records are meaningless. He is absolutely foolhardy, placing himself and all his observers on top of the apparatus, with no protection. " Parsons went on to sketch the rocket test stand, wrote that Maynor claimed to hold six secret patents, and closed with a terse "See you next week. Jack." JPL Archives, Doc. # HC 3-94.

[353] Malina (1969), p. 172. GALCIT -61, developed by Parsons and Mark Mills, consisted of 76% potassium perchlorate and 24% asphalt, a higher percentage of oxidizer than GALCIT-53.

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his head. Malina may have speculated on the irrational roots of Parsons' creativity, but if a key to scientific work is ordered logical examination of a structured set of parameters, then Parsons was a scientist. Originality in science does not require a university diploma, nor does it require acceptance of standard experimental methodology. It does require keen observational skills, determination, a logical mind, and the ability to see problems in an unorthodox way.

A disciplined mathematician like Tsien, a highly competent and confident engineer such as Malina, these, too, possessed the creative spark. In fact, some of the most creative inspirations can be found in the realm of pure mathematics. When personal ambition and scientific curiosity are filtered from the equation, what bound the GALCIT group together was not personal friendship, but mutual respect and talent. As Malina wrote in his memoirs, "it should be pointed out that two of the most important discoveries in the long history of solid rockets were made here (at GALCIT.) First, it was theoretically proven by the Kármán-Malina analysis that a stable constant pressure, long-duration solid propellant rocket motor was possible. Second. Parsons found a new kind of material, the asphalt-potassium perchlorate composite solid propellant, which initiated modern castable solid propellant rocket technology." [354]

In the realm of liquid propellant technology, the GALCIT group also made long strides. Parsons discovered in 1941 that substituting red fuming nitric acid for liquid oxygen as an oxidizer could produce a propellant mixture that worked and presented fewer practical problems than liquid oxygen.

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354 Malina (1969), p. 174.

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(Because LOX—liquid oxygen—has a very low boiling point, 90° K, or about -210° C, soldiers cannot cart the chemical around a battlefield with ease.) Parsons' substitution was undoubtedly timely. The army refused to consider liquid oxygen for its missile arsenal and the GALCIT group had to come up with an alternative. Parsons must have relied on his store of chemical knowledge for his choice, although he could not have known the thermodynamic qualities of nitric acid in detail.

Red fuming nitric acid is relatively stable at room temperature and will dissociate (to some degree) into its components, oxygen and nitric oxide (NO). But its ΔH (enthalpy of formation) is - 41.40 kcal/mole (liquid), meaning 41 kilocalories of energy/mole of HNO_3 must be absorbed for HNO_3 to *dissociate* into its components. This is a moderate amount of energy and in combination with a readily available hydrocarbon fuel such as kerosene, *n*-$C_{14}H_{30}$, or some form of gasoline (probably an aviation grade gasoline such as trimethylpentane, with a very high octane rating and commercially available), the entire reaction—including the combustion of the oxygen released from the dissociation of RFNA—will be intensely exothermic. [355]

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[355] Brown, Theodore L. and H. Eugene le May. Jr. *Chemistry: The Central Science* . Englewood Cliffs, N.J., Prentice - Hall, Inc. 1977. pp. 721-723. The prefix, *n* , before the formula indicates that the primary carbon chain is a straight hydrocarbon chain, in which no carbon atom is bonded linearly to more than two other carbon atoms. An endothermic reaction absorbs heat as it proceeds, while an exothermic reaction releases heat as the reaction drives forward. In Parsons' original study of possible rocket fuels, 'A Consideration of the Practicality of Various Substances as Fuels for Jet Propulsion,' June 10, 1937, he assumed gasoline to be heptane, $CH_3(CH_2)_5CH_3$. But heptane has a very low octane rating, even though it is widely available, and by 1941, when Parsons substituted nitric acid for oxygen as an oxidizer, the GALCIT group was undoubtedly using more refined fuels. It is clear from the 1937 report, part of the GALCIT 'Bible' of original papers, that Parsons had made a careful study of the energetics of several fuels. See: Library of Congress, Malina Collection, Box 9.4.

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If the reaction runs at a higher temperature—under combustion conditions—then a somewhat greater amount of energy is required to release oxygen for use. The reaction is complex. But Parsons had known of red fuming nitric acid at least since 1939. This seems clear from a table included with 'Report No. 3, the Final Report for 1939-40,' in which the characteristics of red fuming nitric acid (HNO3 NO2) are compared (quite favorably) with other oxidizers including liquid oxygen and tetranitromethane. LOX was very unstable under normal conditions and tetranitromethane was not commercially available. Red fuming nitric acid, on the other hand, was relatively stable, commercially available, inexpensive, and would produce, with gasoline as the propellant, an exhaust velocity of 12, 300 ft/sec. [356]

Parsons consulted a chemist, Dr. H. R. Moody, for the details of stability and combustion characteristics of these various oxidizers. However, he had been systematically testing a variety of them, as many as 200, in 1939 and had narrowed down his choices to five, including nitric acid, liquid oxygen, tetranitromethane, perchloric acid (70% solution), and hydrogen peroxide (20% solution) by mid-1940. [357]

Originally, the GALCIT liquid propellant motors of 1936 and 1937 had used a gaseous oxygen supply. This was not recommended to the army, primarily

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356 Malina, Parsons and Forman (1940), p. 105. The data on oxidizers was prepared with the help of Dr. H. R. Moody. Although red fuming nitric oxide does not create an exhaust velocity as high as that of liquid oxygen (15,100 ft/sec.), it is a more than satisfactory substitute for LOX.

357 Moody probably worked for the Hercules Powder Company. This information provided by Dr. John Bluth, Archivist, Jet Propulsion Laboratory, July 1997.

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for storage reasons. It is impossible to store enough O_2, even if design permits installation of individual canisters. Pressurized oxygen tanks are just too heavy and bulky. Some sort of liquid oxidizer was definitely preferable.

Having selected five possible liquid oxidizers for further comparison, Parsons and Forman went on testing. They had constructed a test stand in a "natural semicircular barricade in the *Arroyo Seco*,"the same test site used for solid propellant tests. When they had filled the oxidizer and fuel tanks and readied the motor, the men threw a switch which simultaneously activated a motion picture camera and an ignition sequence. Of the final five oxidizers, Parsons could rule out two without even testing them. Despite its high exhaust velocity in combination with gasoline, LOX—with its low boiling point—would not do. Hydrogen peroxide, which the *VfR* had also looked into in the early 1930s, was also untenable because, the more concentrated the H_2O_2, the greater the possibility of explosion. Hydrogen peroxide is just too unstable at higher concentrations.

Parsons continued to hold on to tetranitromethane as a possibility—after all, some was sitting on his kitchen shelves—but it, too, was extremely unstable and not readily available. Normally, that would not have been a problem for Parsons, who, with his expertise, could mix up a batch in his home. The army might even have been able to procure the chemical through its own channels. But stability counted. And it was expensive, about $5.00/lb. Compared with red fuming nitric acid, at $0.10 a pound, tetranitromethane would prove a costly ingredient for several thousand liquid fuel JATOs. Perchloric acid, both stable and available, was ruled out. It was $0.50/lb more

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expensive than red fuming nitric acid and offered few other advantages over RFNA to offset its higher cost.

So, in another round of tests, Parsons chose red fuming nitric acid as the sole oxidizer, in conjunction with several propellants. It had clear advantages, not the least of which was weight (about 5.25 lbs. acid/lb. of gasoline.) [358] There was no great inspiration involved in Parsons' selection of RFNA. In the 1939-40 Report, Parsons mentioned his priorities very clearly:

> The development of the liquid propellant jet propulsor depends on the solution of the following main problems:
>
> a. the use of a liquid oxidizer that is available and easily and safely handled in large quantities.
>
> b. the supply and injection of the liquid propellants at pressures around 1000 pounds per square inch.
>
> c. the construction of the propulsor to withstand high temperatures. [359]

Parsons knew that red fuming nitric acid fit the bill well on the first count. It was available, cheap, could be produced in large quantities, and would be relatively safe (as safe as highly volatile agents could be.) It had a high thrust potential when used with common gasoline.

Next, Parsons and Forman tried various fuel combinations with RFNA in an open crucible. The tests resulted in some remarkable discoveries. Gasoline, kerosene, ethanol, methanol and similar fuels burned quietly on the surface of RFNA. Benzene (C_6H_6) burned "rapidly and fiercely with a white

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358 In comparison, perchloric acid weighed in at 7.2 lbs/lb. of gasoline, and hydrogen peroxide at 7.45 lbs/lb of gas. LOX was, of course, preferable, at 3.5 lbs/lb. of fuel. See: Malina, Parsons, Forman (1940), p. 105.

359 Malina, Parsons, Forman (1940), p. 103.

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flame." [360] But the most significant results came when a small quantity of metallic sodium was added to each of the fuels. This combination immediately brought about spontaneous combustion. However, Parsons may not have recognized the import of his discovery right away. [361] He tested each propellant and RFNA, with and without the sodium added, in a test motor in the *Arroyo*. No explosions occurred. He concluded that RFNA was a satisfactory oxidizer for a variety of propellants.

In early and mid-1940s, however, Parsons did not use a larger motor unit. The *Arroyo Seco* unit was relatively small. The larger the motor (and its nozzle-throat diameter), the greater the potential for explosion. Inadvertently, through the methodical testing procedure Parsons and Forman normally used, they had discovered the key to successful liquid propellant firings: instantaneous ignition. They could not have known the value of their discovery, for their testing had only begun, and their test motor had not yet begun to throb. Parsons continued to mix the RFNA with aviation gasoline, as it was remarkably inexpensive, stable and gave a high exhaust velocity of 12, 300 ft/sec. [362] Remarkably, this important—if overlooked—discovery came over two years before Malina's 1942 insight into the hypergolic aniline-RFNA combination.

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360 Malina, Parsons, Forman (1940), p. 106.

361 Adding some metals, e.g. sodium or aluminum, to propellants increases their specific impulse; in the above case, the metal additive created a hypergolic (spontaneously igniting) mixture.

362 Malina, Parsons, Forman (1940), p. 107.

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In all of this, had Parsons "communed with his spirits," as Frank Malina had suggested? Undoubtedly not. His discoveries were the result of hard, methodical, even painstaking, and dangerous work. They were the product of experimentation, pure and simple. Through careful investigation, logical, practical choices, and intense, almost incessant testing, Parsons drove forward toward success. He had somehow amassed the immense knowledge of different chemicals which could be used as oxidizers Yet, Parsons only occasionally took courses at the University of Southern California in the late 1930s. He again studied at USC in the late 1940s, this time enrolling in some classes at the School of Pharmacy. [363]

Parsons became interested in chemistry as a high school student, and matured into an expert in pyrotechnics. He was largely self-taught. This fact serves to illuminate the ability of his mind to discipline itself to hard, thorough investigative work. Lacking deep theoretical training in his field, he could not carry out sophisticated assays or analyses of chemical compounds. This in no way implied ignorance, however, for he was intimately acquainted with the reaction chemistry of various compounds, many of them quite volatile. He was familiar with stoichiometry. His calculation of molecular or gram-formula weights, specific gravity of compounds, energy released upon combustion (which he usually expressed in B.T.U's) for various fuels, demonstrated a familiarity with chemical computation that would place him in the ranks of a fine technician.

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[363] Information courtesy of Dr. John Bluth, JPL Archivist, 1997. Parsons apparently studied chemistry in high school, continued his research on his own, at one time working for a skyrocket promoter and expert. Although Parsons' education after high school was sporadic, he nevertheless continued to read and collect data, without feeling the need for a college degree.

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Parsons apparently had an intuitive feel for knowing which compounds would combine well and their correct concentrations. No doubt his familiarity, his level of comfort with volatile chemicals, was problematical, for his somewhat cavalier attitude later cost him his life. But he was not unsettled by the presence of dangerous chemicals around his home and did not shrink from setting off explosions in his own backyard. Parsons was a chemist, despite his lack of university training. His interest in rocketry flowed instinctively from his fascination with pyrotechnics.

No specific mention of any influence of magic or the 'black arts' over his rocketry appears in his semi-autobiographical essay, *Freedom is a Double-Edged Sword.* Although many profound scientific innovators have pursued unconventional pathways to knowledge, Parsons was not entirely untutored; he lacked the requisite degrees conferring legitimacy upon his efforts. Parsons had the inimitable mark of the creator. His inquisitive, imaginative mind never ceased from its quest for collecting, categorizing, and manipulating the world's building blocks.

Parsons slowly developed a world-view which encompassed his chemistry and rocket science. He intended to develop his experience and his accomplishment as a rocketeer within the framework of his unorthodox philosophy. Parsons questioned the authority of established science and of the prevailing social order. In *Freedom is a Two-Edged Sword,* he wrote:

> Developments in modern warfare obviate the necessity for a large army. A world state is the only answer to the atom bomb. In the absence of this, the nations might as well let their citizens live the few remaining years left in peace and freedom. In the absence of this, in another

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ten years it won't make any difference. [364]

Parsons evidently did not feel any great pressure to educate himself formally as society dictated. But this short excerpt from his work hints at a deeper, yet remarkably consistent idea: sensing the effect of his own discoveries upon the world, Parsons struggled to place his work within the wellspring of meaning that had always guided him. He sought to align himself with the ideal of freedom, which to him resolved into resistance to accepted limits of inquiry and behavior. Yet, he was no open revolutionary. He was practically-minded and the essence of his behavior lay in his urge to follow the path of his thought and drives, wherever they might lead him.

And so Parsons felt no compunction about working for the United States Army, developing a new weapon of war. This would pay well at a time when he needed money. He was not amoral; he refused to believe that the rules by which he played committed him to absorbing any established belief- system beyond his own. He sought mystic and sexual expression beyond the norm, yet disciplined himself to learn and master the intricacies of chemical structure and combination. [365] Parsons strove to create a life for himself which fused his inner (sometimes sexual) energy with his creative drive to explore the mysteries of science.

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364 Parsons (1989), p. 24.

365 In another excerpt from *Freedom is a Two-Edged Sword*, Parsons wrote his paean to woman: "It is to you, woman, beautiful lost redeemer of the race, that I address this chapter. That which stirs in you is not madness, is not sin, is not folly, but is life, new life, and joy and fire that will beget a new race, and create a new heaven and a new earth....The body is not beautiful ? Here is a secret. The body is molded by the mind. Embrace fear, repression, hate, then look upon the body—or rather do not look upon it. But go free, love joyously, without restraint, run naked a little, then watch the cheeks flush,...the supple contours, the flowing rhythm....All disease and all deformity are bred in fear and hate. Therefore, oh woman, are you called healer." Parsons (1989), pp. 39, 43.

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Frank Malina, who would have agreed with Parsons' internationalist bent, saw him as "compartmentalized." There may have been some truth to this insight. Malina could not fully reconcile Parsons' libertine behavior with his talent for chemistry. He did not delve into the world Parsons was building, although on some level, the strings of Malina's inner tune reverberated sympathetically with those of John Parsons. Parsons ignored the traditional academic constellation of life in Pasadena during the 1930s and 1940s, and yet sensed that his iconoclastic behavior would find its own niche. His contradictory stances on war, on working for an established institution like Caltech—or the US Army—mark him less as a hypocrite than as an individual who lived by, and loved, an essential inner tension.

Parsons worked within the established order, so long as it did not interfere with his life. If he had been more hostile in his rebelliousness, and less creative, he would have become a misfit of the first rank. But Parsons knew how the constellation of his beliefs fit in with his scientific goals. He could apparently tolerate the contradictory aspects of his belief-system, so long as he was able to follow his own, inimitable path.

> To know ourselves is the highest form of wisdom and to believe in ourselves is the highest form of faith. Science, which seeks to know, and art, which seeks to interpret, are two forms of love...and that these two greatest expressions of the human spirit should be subservient to religion, politics, nationalism, and war is the craziest blasphemy that has been perpetuated on the race. [366]

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366 Parsons (1989), p. 33-34.

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Parsons evidently believed that by extolling the powers of the mind he could walk the tightrope between collaboration with the state and society and his more libertarian beliefs. "There is danger in all organization. But there are times when there is greater danger in the lack of organization," he wrote. Here was John Parsons, the logical, methodical experimenter, speaking to John Parsons, the iconoclast, the man who, on some level, held himself aloof from the academic institute that gave him almost free reign to explore. Under the seemingly diverse threads of his behavior lay a consistency of vision that Frank Malina could not, or chose not to comprehend. Parsons could decry the 'slavery' of the military while building JATOs for the US Army. [367] His conscience—unlike Malina's—remained untroubled by this fact. So long as he could defy authority (while collaborating with it), continue to obey his own demon of disciplined and yet unfettered experimentation, Parsons had few qualms about the ultimate end-use of his work.

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367 Parsons (1989), p. 37.

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Chapter 9

The Balance between Theory and Practice in Rocket-Building—Part Two: Frank Malina and the Enchantment of Theoretical Discovery

The sources of John Parsons' creative impulse were not that far removed from the fountain of Frank Malina's imagination. Both men could be systematic in their attack upon a problem. Both drew from a large fund of knowledge. Both, in fact, recognized the value of the other in the achievement of his own goals. Parsons was not motivated, as was Malina, by concerns over the deteriorating state of world affairs in the 1930s and the threat of fascist aggression. He was neither anti-fascist, nor left-wing. He avoided categorizing himself politically, defining his views only as anti-authoritarian.

With an intensity which mirrored Parsons', Malina could be said to derive some of the motivation for his work from a sense of mission. He felt himself part of the war on fascism, and later, as he matured, in the mid-1940s, became committed to the development of an international framework for peace. But his initial drive to engage in rocket theoretics and experimentation was his frustration with the development of his graduate research. He knew that he had reached a conceptual dead-end in the study of propeller-driven aircraft by 1935 and was searching for other adventures in thought. Malina's mind—like Parsons'—was powerful and systematic in its method of viewing a problem. He tried to approach an experimental problem from every possible vantage point. However, Parsons suffered from the handicap of inadequate

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theoretical training, in that he could not assess the structure and qualities of every chemical he knew from the perspective of an advanced theoretician.

Parsons needed the expertise of Bruce Sage or H. R. Moody to provide detailed thermodynamic data on the propellant-oxidizer combinations he tested; thrust characteristics, maximum gas velocities and pressure ratios he left to Caltech's or Hercules' Powder Company's technical staff. [368] Parsons also had an expert craftsman in Edward Forman to help him construct experimental apparatus. It was as if Parsons had a shelf full of chemicals in his mind and knew which ones to pull down at the right moment. He saw his pyrotechnic ability as an integral part of his model of the world. Parsons worked at the frontier of knowledge in a field considered the realm of fantasy by most established scientists. He did not see himself as an escapist, as a man driven by pessimism to deny the world. Experimentation was another way for Jack Parsons to test the limits of his freedom.

Frank Malina would have understood Parsons, had Parsons communicated his image of the cosmos to his colleague. Malina, too, sought the freedom to explore logical, fundamental relations among physical phenomena, though the avenue of his investigation differed profoundly from that of John Parsons. An analysis of Malina's work from 1939-1940 shows how much it bears the marks of originality, as if the young engineer were, step-by-step, forging a mathematical pathway into a field the horizon of which he could not see clearly. Some of his calculations derive from standard engineering parameters with which he was familiar. Yet, in the course of his theoretical

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368 Parsons' affiliation with Dr. H. R. Moody and other staff at the Hercules Powder Co. may have reached back as far as Parsons' post-high school days, when he worked occasionally for Hercules. Information courtesy of Dr. John H. Bluth, JPL Archives.

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exploration into rocket propulsion, Malina found himself compelled to discover the sequential logic of mathematical terms which related the shape of artifacts (like a rocket motor) to the performance of very unpredictable variables, in particular, hot flowing gases. To some degree, Malina found himself creating a new language for dynamics peculiar to rocket motor performance.

A closer look at Frank Malina's doctoral dissertation, *Characteristics of the Rocket Motor and Flight Analyses of the Sounding Rocket*, 1940, written with the partial collaboration of Hsueh-shen Tsien, and under the guidance of von Kármán, reveals the skein of Malina's thought on the essentials of liquid propellant rocket propulsion. [369] Some of the key parameters of his exposition, particularly the derivation of the angle of divergence of a Laval type nozzle and its role in regulating motor thrust, are Tsien's. [370] This parameter, λ, is a primary determinant of the rocket's thrust potential. Martin Summerfield also learned the importance of the angle of divergence to thrust regulation, for he had by 1941 discovered that small chamber motors required an angle of divergence at the nozzle throat that maximized thrust, and that each angle of divergence was unique to a motor of specific volume.

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[369] Malina's 1940 Ph.D. thesis can be found in two locales: in the Library of Congress, Malina Collection, Box. 15.1, and in the Caltech library, where it is housed in the dissertation collection.

[370] A Laval type nozzle converges symmetrically at its midsection, forcing expanding gas to compress and rush through a narrow passage before exiting the nozzle at a fairly uniform speed (near the speed of sound). The angles of convergence and divergence at the nozzle throat are thus equal. This design forces the gas to increase its compression velocity, thus increasing thrust. The angle of divergence refers to the degree to which the sides of the nozzle diverge from the horizontal.

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But how had Malina come to realize the significance of λ? It is not known whether he and Summerfield discussed the design of a rocket motor while Malina developed his thesis. Malina's 1940 Ph.D. came a year before Summerfield's, and it appears that Summerfield based his conclusions about nozzle design on his own independent work. Malina's investigations into rocketry had preceded Summerfield's by at least three years—since 1937. Clearly, even taking his collaboration with 'Amo' Smith and Hsueh-shen Tsien into account, Frank Malina's theoretical and experimental work mark him as an innovator.

What other novel discoveries evolved as a result of Malina's efforts to sketch the functioning of the *ideal* rocket motor? In 1938, when he and 'Amo' Smith had worked out the basic dynamics of the sounding rocket, the two men had concluded that a self-contained rocket motor could propel a vehicle to altitudes in the exosphere. Malina and Smith calculated in 'Flight Analysis of the Sounding Rocket' that, as the altitude of the rocket increases, the drag coefficient and air resistance decrease, thus allowing the motor to operate more efficiently. [371] In his 1940 thesis Malina wanted to follow the lines of his 1938 inspiration further. He had to push beyond the limits of the known. He insisted in the preface to his thesis that he could not have accomplished his task alone, that several of his insights were the result of work already built up by earlier explorers like Alfred Africano, Hermann Oberth, and Robert Esnault-Peltérie. He acknowledged the effects of close collaboration

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371 Malina and Smith (1938).

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with von Kármán upon the shape of his work, and ascribes "the main ideas in Part IV" to Tsien.

All of this is well and good, but upon close examination, the truth is still that Malina did much of the mathematical exploration on his own. He was even able to define new concepts of rocket motor performance which were to prove essential for further theoretical development of the liquid propellant motor. These discoveries were the fruit of both mathematical logic and invention, for Malina had to create new dynamic relations for the flow of exhaust gas through the chamber and exhaust nozzle: in essence, Malina proved Goddard's assertion that rockets operate at maximum efficiency in a vacuum. He explained the process more thoroughly than Goddard had, and discovered in the process that the nozzle aperture's size was crucial to rocket motor function. [372]

Malina began his analysis of the performance of the liquid propellant motor by defining the reaction propulsion dynamic as an ideal adiabatic process. (That is, no heat enters or leaves the system from an external source.) The process comprises essentially five steps, beginning with pressurization of the chamber and complete vaporization of the fuel prior to combustion. Burning (at constant pressure) begins and the fuel's available heat is liberated. The exhaust products enter the nozzle and the "liberated heat is transformed into kinetic energy of flow by an adiabatic expansion process. The jet energy acquired will be equal to the adiabatic drop in heat content of the products of combustion." [373]

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[372] See Goddard (1919), in Logsdon, *et al* (1995), p. 106.

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In accordance with Newton's Third Law, reaction propulsion will vary directly with energy released in the burning of the fuel. The products of combustion, CO_2 and H_2O, absorb heat, which is then expended as exhaust. The degree of heat expelled at the edge of the exhaust nozzle depends, Malina asserted, on the external atmospheric pressure at the nozzle's open end. Malina demonstrated that, in fact, "the liberated heat energy will be completely transformed into kinetic energy of flow when the exhaust section [pressure] is zero. Thus the ideal transformation of the heat energy is the optimum when the rocket motor is operated *in vacuo*. " [374]

Malina then narrowed down the focus of his investigation to a discussion of heat energy transformation. Working out the energy dynamics and the engineering design of a motor capable of maximizing energy utilization lay at the heart of the problem. By approaching rocket motor function as a thermodynamic problem and as an engineering challenge, Malina calculated energy use in the rocket motor as a measure of specific heat ratios. (Specific heat measures the capacity of reactants to absorb heat.) The specific heats of various fuels are readily available to chemists.

The *ratio* of specific heats is a well-known chemical measure at a particular volume and pressure for an ideal gas. Its symbol is $c_p/c_v = \gamma$. For gasoline and oxygen γ is nearly unity, or 1.2. Why use this ratio at all? Because, as Malina wrote, "specific heats of the products of combustion vary greatly with

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[373] Malina, *Ph.D. Thesis* (1940), p. 7.

[374] Malina, *Ph.D. Thesis* (1940), p. 12. Goddard's conclusions on rocket motor operation *in vacuo* are that "There is a general tendency for the velocities in vacuo to be larger than those in air." Goddard (1919), in Logsdon, *et al* (1995), p. 106.

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their temperature....Their values increase with the temperature." [375] The higher the temperature, the lower the *ratio* of specific heats. The value of γ yields a more easily utilizable quantity than does the direct determination of specific heats for each gas ignited in the rocket chamber. If the ratio of specific heats nears unity—is low—then this ratio implies a constant, stable burn. [376] In other words, at a low ratio (γ), there is little difference in the energy absorbing capacity of the gases in the chamber and a maximum thrust will result. Certainly, specific heats are pressure and volume dependent, and so combustion chamber volume and the exhaust nozzle design also become crucial to determining the energetics of rocket propulsion.

While Malina worked with familiar thermodynamic and gas relations in his essay (such as c, or effective exhaust velocity, and η_{th}, thermal efficiency), he adapted them specifically to the function of the rocket motor. (See below.) [377] Tsien and Malina then created a new parameter, lambda (λ), the degree of divergence of the nozzle from the horizontal, at the nozzle throat. This he determined by integrating the area of the nozzle beyond the throat, using the angle subtended at the nozzle throat as his base angle.

$$\lambda = \frac{1-\cos 2\alpha}{4(1-\cos\alpha)} \quad [378]$$

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375 Malina, *Ph.D. Thesis* (1940), p. 6.

376 This implies that the specific heats (at constant volume and pressure) approach each other in value as the reaction temperature increases.

377 The thermal efficiency of a rocket motor is measured as the *work done* by a given amount (mass) of burning fuel— its efficiency—relative to its combustion energy in B.T.U's. In an ideal rocket motor, then, thermal efficiency serves as a guide to the amount of work done by the motor, as a function of the fuel's energy content.

378 Malina, *Ph.D. Thesis* (1940), p. 19.

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where α = the angle or slope of the nozzle (flaring outward) at the nozzle throat. (A diagram—*Figure 6*—follows immediately after this page.)

Malina based this equation upon a simple assumption—a confined area at the center of the nozzle would funnel the greatest number of expanding gas particles at supersonic speed out the exit. If one knew the dimensions of this area, it would be possible to measure the exhaust velocity of the hot gas at the nozzle's exhaust rim, where the force generated was at its maximum. Malina could then calculate the thrust potential of the ideal rocket motor. Essentially, the angle of divergence of the nozzle, measured from the throat, was the design element which was key to controlling thrust. If, for example, Malina wished to design a motor which was most efficient at low altitudes, all that he had to do was keep λ small and shorten the nozzle. [379]

Malina derived the above equation in a relatively straightforward set of steps. By redefining η_{th}, "in terms of the kinetic energy of the outflowing products of combustion and the available heat energy of the fuel," Malina re-fashioned a standard formula in terms useful to the rocket engineer. [380] This formula, plotted against c—exhaust velocity—for a variety of fuels, demonstrated how the efficiency of a fuel would increase dramatically as its exhaust velocity increased. Conversely, the higher the thermal efficiency of fuel combustion, the greater the exhaust velocity. (See table, reproduced from Malina's thesis-*Fig. 3*, in Appendix I)

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[379] Malina, *Ph.D. Thesis* (1940), p. 19. The small nozzle throat angle and wide nozzle would produce maximum thrust at low altitudes, where it was important to generate speed quickly.

[380] Malina, *Ph.D. Thesis* (1940), p. 9.

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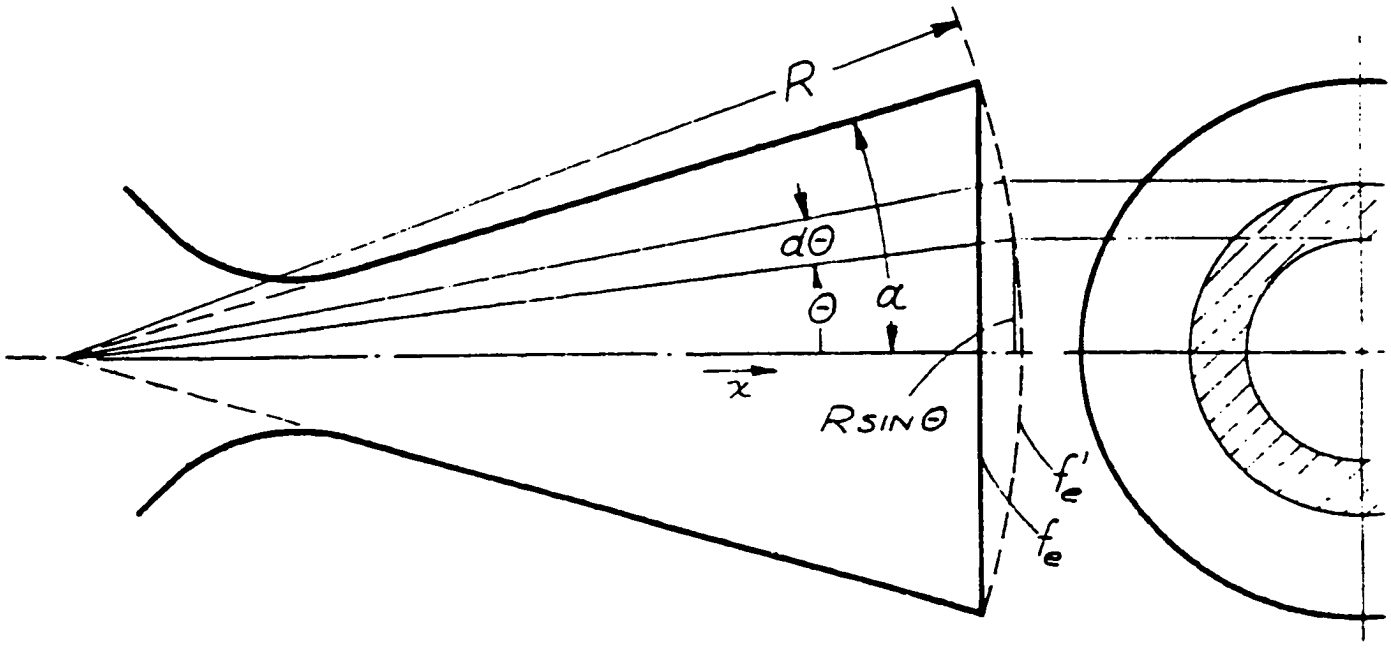


FIG. 6

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The equation is:

$$\eta_{th} = \frac{A\,(w_f + w_{O_2})\,c^2}{2g w_f H_{pf}}$$

where A= the mechanical equivalent of heat, measured in ft.-lbs/B.T.U;

w_f = the weight of the fuel in lbs. burned /second;

w_{O_2} = the weight of oxygen in lbs. used/second;

c = effective exhaust velocity (ft./sec.)

H_{pf} = minimum heat of combustion of fuel (B.T.Us/ lb. at constant pressure.)

g = gravitational acceleration

The determination of thermal efficiency allows a rocket engineer to do more than choose the optimal fuel for its exhaust velocity potential; Malina never forgot that his goal was to maximize the thrust potential of the rocket motor. By determining thermal efficiency, he could evaluate how much heat energy would convert into kinetic energy. One of the building blocks of the rocket propulsion now fell into place.

Next Malina had to relate the thermodynamics of the reaction to the motor's design. How did the nozzle's shape affect the flow of hot gases out of the combustion chamber, and where was the pressure at its maximum? Malina wrote that the nozzle's design was critical to the proper flow of "the products of combustion, under a pressure, p_c, in the combustion chamber. [This flow] is controlled by the existence of a critical pressure, p_t, at the nozzle throat." [381]

The pressure of hot gases at the nozzle throat is actually sensitive to that in the combustion chamber. The ratio of these two pressures is:

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381 Malina, *Ph.D. Thesis* (1940), pp. 10-11.

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$$\frac{pt}{pc} = (2/\ \gamma+1)^{\ \gamma/\gamma-1}$$ [382]

where, again, γ = the ratio of specific heats of the gas at constant pressure and volume. (Malina calculated this value experimentally to be = 1.2.) The calculated ratio p_t /p_c for a gasoline-oxygen fuel combination is close to 0.56. Thus, the pressure of the hot gas at the narrowest point in its passage through the combustion chamber is only about one-half its pressure in the chamber itself. The gas actually slows down as it enters the throat, to just about the speed of sound, and then, as it shoots out toward the edge of the nozzle, gas velocity increases dramatically—to supersonic speed. It is as if the gas molecules bunch up before they reach the nozzle's choke point, then compress and pick up speed as they pass it.

The pressure ratio expressed in this equation is itself dependent upon a key assumption: that critical pressure always prevails in the nozzle throat area. Malina had to assume a constant pressure from the throat to the edge of the nozzle. Once the molecules were traveling extremely fast, they would stay at that speed *uniformly*. In reality, gas dynamics were considerably more complex than Malina assumed in his thesis. He knew this, but for the purpose of clearly constructing a theoretical model, Malina simplified his basic assumptions, and of course, his equations.

Once Malina had eliminated the complicating factors in his description of gas flow, his equation for the ratio of crucial gas pressures possessed a simple beauty. As soon as an engineer knew the ratio γ of heat capacities for specific

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382 Malina, *Ph.D. Thesis* (1940), p. 11

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fuels, he could mathematically describe a picture of how burning propellant would behave as it flowed through the motor, at least under ideal conditions.

Malina then had to determine the velocity of the gas at the nozzle throat area. But the velocity of the gas at this critical juncture is nothing more than the speed of sound. "If the critical pressure prevails at the throat, the velocity of flow of the gas through it will be equal to the sonic velocity of the gas *corresponding to its state in the throat.* "[383] (Italics added.) Expressing the conditions of the gas as it flowed through the nozzle throat in terms of the ideal gas relation in the combustion chamber, Malina derived the gas velocity at the throat:

$$v_t = \sqrt{\frac{2g\,R\,T_c}{\gamma + 1}}$$ [384]

where R = the Universal Gas Constant (for ideal gases), and
g= the acceleration of gravity. But since the velocity of a gas at the speed of sound can be expressed as:

$$a_c = \sqrt{gRT_c}$$

consequently, $$v_t/a_c = \sqrt{2g\,RT_c}$$

$$(\gamma + 1)\,(\sqrt{gRT_c})$$

and by cancellation, $$v_t/a_c = \sqrt{2/\ \gamma + 1}$$

The velocity of the gas at the throat, then, depended fundamentally upon the specific heat ratio of the gas, γ.

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[383] Malina, *Ph.D. Thesis* (1940), p. 11. Emphasis upon the state of the gas in the throat area is added here because Malina assumed the gas to be flowing evenly, rather than chaotically, and believed its radius of expansion to be uniform (that is, it filled the entire throat area without any separation or turbulence.)

[384] Malina, *Ph.D. Thesis* (1940), p. 11.

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Now, since the velocity of the gas (in relation to the speed of sound) at the exhaust end of the nozzle was:

$$v_x/a_c = \sqrt{\ 2/\ \gamma\text{-}1\ [\ 1-(p_x/p_c)^{\ \gamma-1/\gamma}\ ]} \quad ^{385}$$

(where p_x and p_c = the gas pressure at the exhaust edge of the nozzle and in the chamber itself, respectively) then, in the vacuum of space, where the pressure at the exhaust surface of the nozzle (p_x) is zero, the second term under the radical reduced to 1 and the equation for the pressure relation at the exhaust edge was exactly the same as the equation for the pressure relation at the throat. Or,

$$v_x/a_c = v_t/a_c$$

and consequently,

$$v_x = v_t.$$

If the pressure at the nozzle throat were maintained at its critical point (its optimal state), then, in the vacuum of space, the exhaust velocity would also be optimal, and the rocket motor would function at peak efficiency. This critical state depended upon combustion conditions in the firing chamber, the thermal efficiency of the burning gas, and most especially, the angle at which the nozzle diverged from the horizontal at the throat. [386] The crucial insight of Malina's work here is that the motor's functioning depended (at least theoretically) upon three factors: proper choice of fuel, initial burning conditions, and design of the nozzle.

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385 Malina, *Ph.D. Thesis* (1940), p. 12.

386 See *Figure 6* in Appendix I.

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Now a problem arose for Malina. If the angle of expansion were not just right, that is, if the cross section of the nozzle at the throat, f_t, and the cross section at the exhaust edge, f_x, formed a ratio that was too large, the outflowing gas would overexpand and "if the over expansion is continued a shock wave [might] occur." [387] The dynamics of the jet flow became even more complicated. Before the shock wave occurred, the stream of hot gases would separate from the walls of the nozzle, meaning that the rocket's exhaust would not propel the projectile at maximum efficiency. This might happen before gas expansion near the exit rim took place.

Although Malina had not done tests to determine the actual flow characteristics of hot gases through Laval type expanding nozzles, other aerodynamic engineers, not least among them von Kármán and Ludwig Prantl, had conducted such experiments. [388] In his thesis, Malina referred to the work of the eminent American aerodynamicist, William F. Durand (1859-1958), of Stanford University, and Durand's studies of compressibility effects on high-speed fluids as the basis for his own assertion that in "experiments with gases flowing at high velocity in an expanding nozzle,...if over

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387 Malina, *Ph.D. Thesis* (1940), p. 12. Malina later used the symbol f_e to identify the nozzle cross section at the exhaust edge. (See below.)

388 Von Kármán had studied turbulent effects in fluids as a graduate student at Göttingen in the decade before World War I. Prandtl made the study of high speed fluids his life-work. See: von Kármán,' Compressibility Effects in Aerodynamics,' (1941). In von Kármán (1956), *Vol. V*, pp. 127-164. and 'Problems of Flow in Compressible Fluids,' *Vol. IV*, pp. 165-183. Although these studies were completed in the early 1940s, they were clearly based upon theoretical and experimental studies conducted earlier. Prandtl's seminal work, *Fundamentals of Hydro and Aeromechanics* (1935) also discussed compressibility effects in fluids.

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expansion is continued...a sudden change in the characteristics of the gas flow will result." [389]

The potential shock wave presented an ominous threat to the stability of flow. Malina explained as follows:

> Experiments with gases flowing at high velocity in an expanding nozzle show that if over expansion is continued, when the external pressure, p_0, is greater than zero, at some pressure, p_{xs}, a sudden change in the characteristics of the gas flow will result. The flow characteristics again reach a steady state after passing through a narrow transition layer. The thickness of the transition layer is extremely small so that it may be assumed that there is a discontinuity in the flow characteristics. The transition layer is known as a 'shock wave.' [390]

Malina posed no solution for this potentially serious transition to instability within the nozzle. However, he was at least able to analyze its characteristics and determine which conditions would minimize the chances of a shock wave developing during the motor burn. The shock wave's effects would be at a maximum near sea level, when the pressure on the outer side of the wave was equal to atmospheric pressure; its effects would be minimal when the rocket was operating at some higher altitude (which Malina did not specify.) The challenge was to design the nozzle so that any possible shock wave would occur outside the nozzle itself and at a high altitude, so that the flow of gas would not be disrupted.

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[389] Malina, *Ph.D. Thesis* (1940), p. 13. Malina mentioned a specific series of published test results by Durand as his data source for the discontinuity shock wave effect . This reference was Durand's *Aerodynamic Theory, Vol. III.* Berlin, J. Springer, Publisher, 1935, pp. 209-229, 234, 241.

[390] Malina, *Ph.D. Thesis* (1940), p. 13.

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Malina's equations suggest that this could be done if the cross-sectional area of the shock wave were actually larger than the cross section of the outer edge of the nozzle (that is, a relatively narrow nozzle.) Then, the shock wave would intensify outside the rocket motor itself, in space or the upper atmosphere. But the rate of gas flow was a signal factor in preventing a shock wave from manifesting itself inside the nozzle. If the gas flow's velocity were supersonic, a shock wave would likely occur. So, the task was to keep the flow of gas at *subsonic* velocities. This was accomplished, at least in part, by precisely regulating the area of the nozzle throat and the angle of nozzle expansion from the throat outward. Ideally, a wide nozzle would best dampen supersonic gas flow. [391]

Malina hoped, with this design insight, to maintain subsonic gas flow in the nozzle. [392] But his solution also threw into relief a dilemma Malina could not escape.

> If a nozzle is designed to give the maximum thrust at sea level, then the thrust delivered at other altitudes, where the external pressure is much lower, will be less than the amount that can theoretically be obtained. If on the other hand the nozzle is designed to give the maximum possible thrust at a high altitude, then the loss in thrust at lower altitudes may be so great that operation of the rocket may be seriously impaired. " [393]

For a rocket motor whose nozzle was wide-angled, the thrust would be high in the lower atmosphere, but prone to shock-wave formation at high

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391 Malina, *Ph.D. Thesis* (1940), pp. 13-16.

392 Malina, *Ph.D. Thesis* (1940), p. 17.

393 Malina, *Ph.D. Thesis* (1940), p. 21.

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altitudes. By reducing the nozzle's dimensions, narrowing it so that it flared out less, the motor would produce insufficient thrust to boost the rocket above the stratosphere. For Malina, the problem seemed (in 1940), theoretically irresolvable. The only solution he could offer was to continue conducting tests, so the operation of a real rocket motor could be analyzed in detail. [394]

As Malina was struggling with the transition from ideal to real fluid dynamics in his thesis, he inadvertently threw the limitations of mathematical theory into high relief. His equations pointed to a design solution to the 'shock wave' problem, but no matter how carefully he expounded the dynamics of shock wave formation, his mathematics still led him to the inescapable necessity for more hard experimental data. In fact, his very awareness of the problem depended upon experimental discoveries by another engineer (Durand).

The theoretical wall he had reached was to prove only temporarily too high: for with continued experimentation, an optimal size and shape nozzle for each specific rocket motor would later be found. If this were not the case, Malina's own 1945 creation, the WAC Corporal rocket, would not have successfully flown. [395] But even more germane is the conclusion that Malina's theoretical artifact—his elucidation of the ideal dynamics of energy transformation in the rocket motor—led him to see the problem of discontinuous fluid behavior more clearly. Malina's theory of how an ideal

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394 Malina, *Ph.D. Thesis* (1940), p. 21.

395 For the story of the WAC Corporal, see Chapter 13 of this thesis.

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gas flows in a rocket motor was neat and precise. Reality, however, was less so. Or to put it another way, theory could only approximate the deeper, more complex structure of reality.

Malina was a scientist and technician who created a mathematical edifice with its own intrinsic order; he was not given to analyzing superficialities. His knowledge of the calculus and of engineering dynamics allowed him to penetrate further into the complexities of rocket motor performance than had previously been done. Malina did not always state the obvious, but rather left it to the reader to finish his mathematical thought at crucial points. Perhaps he assumed his readers, most of them engineers, would naturally make the final mental jumps instinctively. But in many other ways, Frank Malina was an oblique writer who left much unsaid. He did not perceive the need to explain every detail of his thought in order to make himself understood.

The problems of discontinuous flow of the exhaust gases and shock wave formation in the rocket motor's nozzle would plague rocket engineers for decades beyond the 1940s. Malina did not devote more time to their treatment because his intent was to prove that a liquid propellant rocket motor could work. He did not avoid the issue, but rather delved into it as best he could, describing the behavior honestly. Malina left it to other scientists to discover the roots of complex fluid behavior. His own interest focused on how a rocket motor converted heat energy into kinetic energy.

To come full circle in his essay, Malina treated the core of the reaction principle. This is where his analysis was most elegant, relying in part upon Hsueh-shen Tsien's mathematical insights. First it was necessary to convert

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the flow and pressure of hot gas created by combustion into a quantitative measure of energy propelling the rocket forward. "By application of the *momentum theorem* to the flow through the nozzle," Malina derived a relation between pressure and the force created by the change in momentum of the gas as it passed from the combustion chamber to the nozzle's outer rim. [396] He gave this relation as:

$$\int p dS - \int (p_e - p_o)\, df_e = \int v_e\, dm$$

where p = the pressure acting on the walls of the combustion chamber and exhaust nozzle in lbs./sq. in. (p_o = atmospheric pressure at a specific altitude which the rocket is operating.)

dS = the surface area of the walls of the combustion chamber and exhaust nozzle,

f_e = the nozzle cross section at the exhaust edge

v_{ex} = the axial component (in the x direction) of the exhaust velocity at the edge of the nozzle in ft./sec. and,

dm = the rate at which the mass of propellant flows through the exit section of the nozzle, in slugs/sec.

Since the pressure on the interior surface area of the combustion chamber and nozzle was essentially the force exerted by the burning propellant on the chamber, Malina substituted $F = \int p dS$ for the first integral and produced an equation for the force of propellant flow:

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396 Malina, *Ph.D. Thesis* (1940) p. 17. The *momentum theorem* defines the total momentum P, of any number of particles as equal to the vector sum of momenta p, of the individual particles in a system. In this case, Malina needed to integrate, as he had to estimate the total force/unit area in the combustion chamber and nozzle in order to approximate the combined momenta of all the particles of gas. See: Young, *University Physics* (1992), pp. 198-199.

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$$F = \int_{fe} v_{ex}\, dm + (p_e - p_o)\, f_e$$

Here, again, p_e and p_o are the exhaust pressure and pressure outside the nozzle—in open air—respectively. [397]

Thus, the total force exerted by the rocket motor was a function of gas velocity at the nozzle's outer rim and the change in pressure as hot gases flowed from the combustion chamber out to space. This change in gas pressure depended in part upon internal heat transfer, the diameter of the exhaust nozzle and, as Malina already emphasized, upon the inside dimensions of the nozzle throat. [398] In this one equation, Malina successfully defined rocket propulsive force as a combination of energy dynamics *and* design parameters.

Malina next made a key substitution in the above equation.

$$v_{ex} = v_e \cos \theta$$

θ is just the radius of the core of the nozzle. (See figure 6, Appendix I). Tsien suggested to Malina that the diameter of the nozzle at the throat defined the flow characteristics of the propellant gases at the edge of the nozzle and advised Malina to view the flow at the core as "uniformly radial," meaning that the jet of gas would flow out symmetrically, rather than discontinuously, and radiate out concentrically from a point at the center of the nozzle throat. If angle $\theta = 0$, then $\cos \theta = 1$ and $v_{ex} = v_e$. The gas velocity at the outer rim of the nozzle was then equal to its velocity exactly at the center of the nozzle, where the flow was (theoretically) a vector in the x direction.

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397 A diagram of the nozzle, indicating cross-sections where Malina intended to measure different gas pressures and the angle α, the angle extending from the center of the nozzle (at the narrowest point in the throat) to the rim at the exit, may be found in Appendix I of this thesis.

398 Malina *Ph.D. Thesis* (1940), p. 18.

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Malina's objective was to create a mathematical image of the nozzle as if gas flowing out of it were moving in *concentric rings* (see figure 6) and the edge of the nozzle itself were actually spherical. He knew this was only an approximation, but he was developing a model, not describing actual reality.

A concentric ring of width $Rd\theta$ would then expel a mass of propellant equal to:

$$\rho \ v_e \ df_e'$$

where df_e' is equal to the spherical area encapsulating the outer edge of the nozzle and ρ equals the mass density of the gas.

This value equals dm (the mass of propellants flowing out the nozzle in this small ring) and so

$$dm = \rho \ v_e \ df_e'$$

Integrating the value of the mass of propellant exiting the nozzle as a function of the diameter of the nozzle at its widest point, f_e, Malina expressed the mass of hot gas flowing out the end of the nozzle with the following equation:

$$\int_{f_e} v_{ex} \, dm = 2\pi R^2 \rho v_e^2 \int_0^{\alpha} \sin\theta \cos\theta \, d\theta$$

Carrying through the integration gave

$$\int_0^{\alpha} \sin\theta \cos\theta \, d\theta = \frac{(1-\cos 2\alpha)}{4}$$

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since $\int_0^{\alpha} \sin\theta \cos\theta \, d\theta = \int_0^{\alpha} 1/2 (\sin 2\theta) d\theta,$

$$\text{or } 1/2 \int_0^{\alpha} \frac{(-\cos 2\theta)}{2},$$

and $$1/2 \int_0^{\alpha} \frac{(-\cos 2\theta)}{2} = 1/4(-\cos 2\alpha)_0{}^{\alpha}$$

Because $$1/4(-\cos 2\theta)_0{}^{\alpha}) = 1/4(-\cos 2\alpha - (-\cos 2 \cdot 0)).$$

Malina concluded that

$$1/4(-\cos 2\alpha - (-\cos 2 \cdot 0)) = 1/4(-\cos 2\alpha + 1) \text{ or,}$$

$$1/4(-\cos 2\alpha - (-\cos 2 \cdot 0)) = 1/4(1 - \cos 2\alpha)$$

Malina, of course, did not show all these steps, since he was writing for other professionals. Still, it is essential to re-construct the process, for implicit in these intermediate steps was the key to his determination of rocket thrust. By referring again to Figure 6 (Appendix I), one may visualize the dimensions of $\angle\,\theta$ in terms of $\angle\,\alpha$. What Malina quite deftly accomplished was the transformation of thrust measurement for the entire nozzle (at its widest point) into a *multiple* of the measurement of thrust for one small concentric circle at the nozzle's center. Since thrust in the region of $\angle\,\theta$ is a nearly horizontal vector in the x direction, the experimenter need only calculate the velocity of particles one-dimensionally, rather than across the entire cross-section of the nozzle. The clarity, in fact, brilliance of Malina's vision now becomes manifest.

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Still, there was one more step to take, since his end result was:

$$\int_{fe} v_{ex}\, dm = \rho v_e f_e' v_e - \frac{\left(1 - \cos 2\,\alpha\right)}{4(1 - \cos\,\alpha)}$$

How did he come to (1-cos α) in the denominator of this equation? This turns out to be an important expression, for

$$2\pi R^2 (1\text{-}\cos\,\alpha) = f_e$$

where f_e both equaled the spherical cross-section at the nozzle rim and the physical state of the gas as it flows out the nozzle. (In other words, f_e was both a function of gas behavior and of the radius of the nozzle.) [399]

In order to derive this equation, one must return to the equation which described the *mass* of propellant flowing through the nozzle exit as a function of the area of the nozzle:

$$\int_{fe} v_{ex}\, dm = 2\pi R^2 \rho v_e^2 \int_0^{\alpha} \sin\theta \cos\theta\, d\theta$$

Isolating the term $2\pi R^2$—which is, of course, a multiple of the formula for the area of a circle—the amount of gas flowing out could be expressed as an integral of the area bounded by the nozzle's *center*:

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399 In order to relate two distinct parameters, one a measure of propellant characteristics (velocity of flow), the other a measure of the radius of the nozzle at its widest point (f_e), Malina wrote: $v_e = w R t_e / f_e p_o$
(Here R stands for the universal gas constant and w= the rate of gas flow.). Thus f_e equals $w R t_e / v_e p_o$, and the radius of the nozzle exhaust section also becomes a function of the velocity of the expanding gas. See: Malina, *Ph.D. Thesis* (1940), pp. 11, 16.

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$$\int_0^{\alpha} 2\pi R^2 \sin\theta d\theta$$

where $\int_0^{\alpha}$ equaled the area of the nozzle's outer rim (the 'spherical cap')

and $2\pi R^2 \sin\theta d\theta$ equaled the area of the small concentric ring at the center of the nozzle.

Essential to Malina's evaluation of the term $\int_0^{\alpha}$ was his assumption that

$$f_e' = \int_0^{\alpha} \quad ^{400}$$

Malina reasoned that if fe'(as an equation of state) = $\int_0^{\alpha}$ it must also equal the integration of the entire area of the small inner concentric rings, expressed by the function $dm = \rho ve\, fe'$. (See figure 6, Appendix I). Malina envisioned this area duplicating itself across the entire nozzle exit section (as if it were a series of ripples radiating out from a stone's impact with a pond surface.) What he really did was equate an area with a flat surface to a cross-sectional area with a semi-spherical shape. Could he reasonably do this? By defining the nozzle's characteristics in terms of the flow of gas out to space, the answer is, yes—if one assumes an equality and a fair approximation to be the same. It would have been difficult to take a cross-section of the exhaust gas at any one longitudinal point. Mathematically, it was more straightforward for him to measure the flowing gas as a bounded area with a flat limit—but he actually saw it as having volume.

So the area $\int_0^{\alpha} (2\pi R^2)\sin\theta\, d\theta$ became:

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400 Here $f_e' = \int_0^{\alpha}$ because Malina equates the area of the spherical rim of the nozzle to the nozzle's angle of expansion from the center out.

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$(2\pi R^2) \int_0^{\alpha} \sin\theta \, d\theta$ and since

$f_e' = 2\pi R^2 (-\cos\theta)_0^{\alpha}$,

consequently $f_e' = 2\pi R^2 \left(-\cos \alpha - (-\cos 0)\right)$.

But since $(-\cos 0) = -1$,

$f_e' = 2\pi R^2 (1-\cos \alpha)$

R^2 then equaled $f_e' / 2\pi (1-\cos \alpha)$.

By substituting the above expression into the equation

$$\int_{fe} v_{ex} \, dm = 2\pi R^2 \rho v_e^2 \int_0^{\alpha} \sin\theta \cos\theta \, d\theta,$$

which resolved to

$$\int_{fe} v_{ex} \, dm = 2\pi \left(f_e' / 2\pi (1-\cos \alpha)\right) \rho v_e^2 \frac{\left(1-\cos 2 \alpha\right)}{4}.$$

Finally, by canceling the 2π in the numerator and denominator of the complex fraction, the result was

$$\rho f_e' v_e^2 \frac{\left(1-\cos 2 \alpha\right)}{4} / (1-\cos \alpha) \text{ or,}$$

$$\int_{fe} v_{ex} \, dm = \rho v_e f_e' v_e \frac{1-\cos 2 \alpha}{4(1-\cos \alpha)}$$

Malina then labeled the expression $\frac{1-\cos 2 \alpha}{4(1-\cos \alpha)} = \lambda$

Thus $$\int_{fe} v_{ex} \, dm = \lambda \, m v_e$$

(Here $m = \rho v_e f_e' v_e$, the mass of propellant flowing through the exit section of the nozzle/second.)

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Malina's concept of the area of the nozzle exit cross-section as layered multiples of one small region of the nozzle was a novel and truly creative insight. Since Malina did not envision a flat surface at the end of the nozzle, but rather assumed the nozzle edge as approximately in the shape of a half-sphere, he managed to develop an expression which related the entire rocket motor's thrust to the *mass and velocity* of a small amount of gas passing through one central segment of the nozzle. This is clear from the relation of λ to mv_e, the momentum component.

Hence, the thrust of the rocket motor became

$$F = \lambda\ mv_e + (p_e - p_o)\ f_e$$ [401]

This was really an ingenious approximation. Malina had worked out an equation that related forward momentum of the rocket to the "variation of the nozzle angle" of the motor. [402] Tsien had helped him by explaining a key substitution in the process of discovery, but the lion share of the work was his own. If a rocket engineer could create a rocket motor with just the right nozzle design, he would be able to maximize the rocket's thrust. Malina proved, with a detailed and substantive mathematical analysis, that it was theoretically possible to build a rocket that met performance specifications.

Malina's unfolding of the theoretics of rocket motor thermodynamics and design were the fruit of years of undergraduate and graduate training in

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401 I am greatly indebted to Professor A. T. Vasquez of the City University of New York Graduate Center, Department of Mathematics, for his invaluable assistance in helping me to follow the mathematical process in Malina's thesis.

402 Malina, *Ph.D. Thesis* (1940), p. 19.

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calculus, thermochemistry, aerodynamics and concentrated study of extant rocket research. Every new equation that Malina wrote, every step along the path toward the definition of 'ideal' motor performance broke new ground. No one before Malina had seen the rocket motor's function in quite the same insightful way. This was his creative accomplishment. Malina's brilliance lay in combining the chemical, mathematical, and design threads of the rocket motor into a coherent fabric.

Was Malina's analysis of ideal rocket motor performance valid for a real rocket motor? Malina claimed that it was, provided certain complicating factors could be ignored. However, he never asserted that an ideal and a real operating motor were exactly alike.

> This analysis of the ideal constant pressure rocket motor was made to bring out most of its operating characteristics and to determine the relative importance of various factors that affect its operation. The assumptions introduced greatly reduced the complexity of the mathematical analysis. However, the main features of operation of the actual constant pressure rocket motor will still be the same as for the ideal motor, therefore, the simple general theory presented can be used as a guide by the designer. [403]

Malina's concluding remarks, upon reflection, qualified their assertion of equality between ideal and real rocket motor performance. He believed only that his treatment of rocket propulsion should serve as a "guide" to the future designer. By making assumptions at key points in his derivation of the ideal equation for thrust (such as the equating of $f_e' = \int_0^{\alpha}$) Malina simplified thrust measurement. The concept that one could measure thrust as a function of particle momentum through one narrow imaginary cone at

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403 Malina, *Ph.D. Thesis* (1940), p. 23.

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the center of a rocket motor nozzle was, of course, a construct. If Malina had possessed a mathematical lexicon allowing him to describe the actual molecular state of outflowing gases in the nozzle, he would have been able to define rocket propulsion much more precisely.

However, Malina did not build his theory in a vacuum—he appended several graphs to his thesis, describing experimental measurement of key thrust parameters—and these graphs indicated that the data were consistent with his mathematical treatment. [404] Unfortunately, Malina provided no information on where he obtained the data; one may only assume that they were the results of experiments carried out at GALCIT. In one graph, Figure 8, 'Universal Ideal Thrust Diagram,' Malina plotted the *thrust ratio* between the nozzle exhaust and the nozzle throat (F_x / F_t) against the *expansion ratio* of the nozzle at the exhaust and at the throat (f_x / f_t). When (F_x / F_t) = 1.0 (unity)—in other words, when the force at the throat equaled the force at the exhaust rim—the expansion ratio of the nozzle would not exceed a factor = 12. In this case, a relatively narrow nozzle would result in a "line of maximum thrust." [405] Another graph showed that as the pressure ratio between combustion chamber and nozzle exhaust [p_c / p_e] held at about 50%, the efficiency [η_i] of the burning propellant would rest just below half its maximum potential. (Figure 2, 'Variation of the Ideal Cycle Efficiency with the Pressure Ratio'.) Finally, in Figure 3 ('Variation of the Thermal Efficiency [η_{th}] with the Effective Exhaust Velocity, *c*'), at 50% thermal

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[404] See Appendix I of this thesis for these graphs and an explanation of their significance.

[405] Malina's graph also indicated that with a twelve-fold expansion ratio, gas pressure at the nozzle throat would ideally remain at 50% of the pressure in the combustion chamber. See Graph 1 (Universal Ideal Thrust Diagram), Appendix I.

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efficiency, the propellant produced about 10,000 B.T.U.s of energy—a considerable amount. [406]

By measuring motor efficiency at constant rates, Malina verified experimentally that as one varied the width of the nozzle ($\angle \alpha$)—in effect, designing a relatively narrow nozzle—one could obtain about 50% efficiency in thrust and a relatively high energy output. For an ideal rocket motor, the experimental data correlated reasonably well with Malina's theoretical conclusions. Theoretically, a rocket engineer could construct a motor with just the right nozzle angle to optimize thrust.

Although Malina had strictly created a model of reality, he was still able to validate the power of theory as a guide in machine design. More importantly, he fused design parameters to physical equations of state; this was another proof that one may bend and shape physical behavior by designing an apparatus correctly.

There are several important discoveries in Malina's thesis. The derivation of the ratio of partial pressures $\frac{p_t}{p_c} = (2/\gamma+1)^{\gamma/\gamma-1}$ which permitted Malina to create a mathematical image of how gas would flow through the nozzle throat (as a function of differential pressures between the combustion chamber and throat), opened the door to his discussion of unstable fluid behavior in the nozzle—that is, shock waves. Malina portrayed the ideal rocket motor as maintaining constant internal gas

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[406] In all the above examples, this researcher has used a constant pressure ratio (p_o/p_e) and constant thermal efficiency rating (η_{th}) of 50 % as a standard. The data are for the use of gasoline as a fuel.

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pressure while in operation, and thus each component of the total pressure (such as the exhaust or combustion chamber pressure) was an element in total fluid stability.

For Malina, the most important gas dynamics factor was pressure in the aft section of the motor—the exhaust nozzle. By measuring partial pressures in a crucial area of the motor (the throat) as a function of fuel specific heats, Malina could describe the thermodynamics of fluid flow with data accessible to any chemist. He could also evaluate the characteristics of the shock wave in terms of pressure and gas velocity on both the inside and outside edges of the wave and thus clearly depict the consequences of the shock wave for motor performance. [407]

Another invention was the creation of the term η_{th}, thermal efficiency of the rocket motor. η_i, ideal cycle efficiency, is a commonly known engineering term, but Malina converted it for use as a measure of rocket fuel kinetic energy. [408] However, rather than identify himself as the concept's creator, he wrote "The experimenter has found it more convenient to define a thermal efficiency η_{th}, of the rocket motor unit in terms of the kinetic energy of the outflowing products of combustion and the available heat energy of the fuel...." [409] There was no reference to any other earlier source of the term and, in fact, the context of Malina's discussion indicated that he himself was the first to utilize it.

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407 Malina, *Ph.D. Thesis* (1940), pp. 13-17. One equation even related the velocity of the gas in the shock wave to the mass density of the gas. $v_{s1}/v_{s2} = \rho_{s2}/\rho_{s1}$.

408 See: Cornelisse, *et al* (1979), p. 220.

409 Malina, *Ph.D. Thesis* (1940), p. 9.

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Malina's proclivity for transformation of known mathematical symbols into new ones manifested itself frequently in his Ph.D. thesis. Of course, he relied initially on well-known symbols, such as γ for the ratio of specific heats. But then there were new terms for his own discoveries: f_{xs}* for the "critical shock cross-section," and λ for the key angle of nozzle flare (divergence from the horizontal.) He was among the first rocket engineers to define effective exhaust velocity (c) mathematically:

$$c = \lambda v_e + \frac{(p_e - p_o) f_e}{m} \quad [410]$$

Effective exhaust velocity is commonly used by rocket engineers to describe rocket thrust in the atmosphere (as it is dependent upon atmospheric pressure to some extent.) [411] This equation is, in fact, a variation of Malina's crowning thesis achievement, his derived equation for ideal rocket thrust

$$F = \lambda\, m v_e + (p_e - p_o)\, f_e\,.$$

It is no hyperbole to assert that, as Frank Malina pushed forward into the theoretical unknown, he devised new terminology to provide building blocks of order where there had been before only a blank slate. Malina did not solely invent the vocabulary of rocket science where none had previously existed. He found himself surrounded and preceded by great innovators—von Kármán, Parsons, Sänger, Goddard, among them. Still, Malina's re-definition of terms and symbols meant that he could do more than

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[410] Malina, *Ph.D. Thesis* (1940), p. 19. Again v_e = gas velocity, p_e= exhaust pressure, p_o = outside ambient pressure, and m = the mass of gas particles.

[411] See: Cornelisse, *et al* (1979), p. 120.

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understand the relation of commonly accepted symbols to physical phenomena—by extension, he intended to apply the principles of those phenomena in novel ways. He undergirded his new application of these symbols with a mathematical logic—a sentence structure, so to speak—which recognized the interlocking relationship between thermodynamics and the new field of rocket combustion.

Did Malina's equations bring the essence of reality into concrete form? Was his work a case of a 'felicitous conjunction between mathematical order and the physical order of reality?' The answer is—yes, but with emphatic qualification. In some instances, (such as the statement of the ratio of pressures, p_t/p_c), an inherently beautiful image of fluid flow emerged. This equation would provide a clear window into nature's work to any rocket engineer.

However, the validity of Malina's equations only held under certain specific conditions—they assumed constant pressure during rocket motor performance; and—what is more, in building a mental picture of rocket combustion, Malina turned approximations into equalities. The statement, $f_e \cdot = \int_0^{\alpha}$ equated a condition embodying a physical state—mass and velocity of a hot gas—with a defined cross-sectional plane at the edge of the rocket nozzle, as if the two were essentially the same. They are not, and by further imagining that the volume of exhaust gas had a definite shape (enclosed within a semi-sphere), Malina constructed a static geometric image of fluid behavior, rather than a more supple one which envisaged constant flow. (Although he did integrate the area within the semi-sphere, indicating he knew the gas was expanding.)

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However, Malina's intent was to measure, quantify, and attach symbolic meaning to a physical process only inadequately understood before his time. Frank Malina was a profound student of rocket combustion and described it eloquently in mathematical terms. His discoveries have an essential validity and still are the staple of rocket engineering texts. By combining physical equations of state and the geometry of the rocket nozzle in the same equations, Malina carried out an intellectual coup. Despite their limiting factors—that is, the use of approximations and assumptions of ideal behavior—Malina's mathematical statements did serve as reasonable predictors of rocket motor behavior and as a guide to design. In Malina's hands theory became a true and necessary foundation of the development of rocket science.

Perhaps, if Malina had possessed the tools permitting him a more precise mathematical analysis, he would have advanced rocket science even further. Mathematics can state the relations among phenomena, and if these connections are essential enough, raise them to the status of physical law. The mathematical discoverer transforms his insights into definitive statements, using rich or precise enough language, with symbols combined in an appropriate logical sequence. Experiment, as well, can define and reveal the sensible phenomena of nature, if the experimenter takes into account the degree to which his experiment alters phenomena as it reveals their inner workings.

Theory and experiment each contribute clarifying insights to scientific exploration. Nevertheless, fundamental methodological differences between

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the two will always persist. Since theory rests upon the logical combination of symbols whose inherent meaning and connectivity the theorist instinctively grasps, meaning only becomes apparent to those who understand the givens of the language.

But for experimenters, the building blocks of knowledge depend upon prior experience, and upon banks of images of real constructed objects. The experimenter then manipulates or re-shapes these objects in some novel way. Although both theory and experimentation may be deeply imaginative processes, they are essentially different mental pathways. This conclusion is borne out by the fact that John Parsons' discovery of GALCIT- 53 led almost directly to a new type of rocket propellant and an entirely new generation of rockets. There was a concrete connection between Parsons' experimentation and the success of the solid propellant rocket, beginning in the 1940s. On the other hand, Frank Malina's mathematical insights would always provide guidelines for future invention, but could never define its exact structure.

In the history of rocket science, theoretical advances have elucidated areas of science for which there seemed no hope of actual realization. However, the shortcomings of theory—specifically its tendency to rely on constructs and its assumptions about invisible phenomena which are inherently quite complex—more complex than known systems of mathematical language could account for—have also limited its usefulness. In the application of novel ideas lies the proof of their worth, at least in engineering. Even Malina embraced experimentation wholeheartedly, for he knew that the ultimate test of his mathematical work was the construction of a rocket motor.

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Experiment often refined—through its feedback effect—theoretical knowledge.

Malina's 1940 thesis on the *Characteristics of the Rocket Motor....* was a profound and far-reaching accomplishment. His insights into the dynamics of rocket propulsion and combustion chamber design would lead to the construction of more efficient rocket motors. However, a rocket engineer's ultimate goal is to build a rocket, not merely define it theoretically. Theory is not incontrovertible fact. As theory works within the limits of symbolic language, there will always be a disjunction between the possible logical combinations any language can invent and the number of 'worlds' (no matter how large or small) in the universe awaiting definition. There will always be a distinction between mathematics and the physical universe, even if mathematical symbols eventually reveal some of the deepest secrets of nature.

In order for Malina to have realized the limit of his depiction of rocket motor dynamics, he would have had to abandon all his approximations and assumptions and completely build a new theory within an unknown (to him, at least) mathematical framework. Malina would have had to enter another mathematical world and make a transition from the study of deterministic phenomena, by way of direct integration and the calculus of variations, to exploration of indeterminate phenomena which defied adequate measurement by methods then available to him.

It would appear at first glance that mathematics can account for the transition from stable to chaotic behavior across a boundary condition and that the

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definition of fluid systems behavior may eventually become logically complete. However, such an accomplishment would require more than the finite development of a new mathematical language—it necessitates the introduction of computers to carry out quite complex calculations with three or more variables (linked non-linearly); such computers best aid analysis when they can graphically depict the system at work. [412] Malina himself frequently relied on graphs in order to simplify the calculation of results. [413]

Even when computers—powerful tools that they can be—manage to make short work of complex equations and groups of data, they display limitations, due to their quantum nature and the chemical base of their circuitry. Should such barriers be partially overcome with the advent of new technologies, there will still be finite limits to their speed and power. If and when mathematicians do cross the threshold to a more complete sketch of the realm of chaos and turbulence, they will continue to make certain qualifying or simplifying assumptions that will render their new languages incomplete. There may well be ultimate barriers to a complete logical understanding of physics. The structure and rhythm of language do not coincide, except fortuitously, with the physical universe.

Frank Malina and John Parsons both strode down the road to the accomplishment of their long and eagerly sought-after goal: the successful construction of a rocket motor. Yet one—Malina—trod the path of

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412 The equation $dx_1/dt = \alpha x_1 + \beta x_2 + \gamma x_1 x_2 + ... + \delta x_n$ is one such nonlinear equation. The terms x_1x_2 are nonlinear terms coupling both x_1 and x_2. Here α, β, γ and δ are constants. According to G. L. Baker and J. Gollub, only three independent variables are "required for chaos" to set in. See: Baker and Gollub, *Chaotic Dynamics*, 2nd Edition. Cambridge, the University Press, 1996, p. 3.

413 Malina, *Ph.D. Thesis* (1940), p. 15.

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mathematics, a road leading to an immense number of doors, each containing a new set of symbolic rooms. The other, Parsons, walked directly to the house of experiment, and found himself unlocking the door to a unique room—and this room contained the key to one specific puzzle among the infinite number awaiting discovery.

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Chapter 10

Building the JATO: How a Shift in Research Goals Created Breakthroughs in Rocket Propellant Technology

Creativity and determination were hallmarks of the GALCIT Rocket Team's work. The thorough, probing experimentation of John Parsons and Edward Forman throughout the history of the project, particularly from 1938 through 1941, eventually produced a solid propellant rocket motor capable of boosting an airplane off the ground faster and at a steeper angle than had been seen before in the United States. By the summer of 1941, the GALCIT crew had successfully tested its first JATOs. Liquid propellant research lagged somewhat behind. Malina had tried to work out the dynamics of a liquid propellant motor to within reasonable tolerances, but building one actually larger and more complex than the first 1936 *Arroyo Seco* motor was to require more inspiration, more testing. Still unresolved problems included cooling the motor and reducing the time lag between ignition and thrust augmentation in the combustion chamber.

If the GALCIT rocket team had followed the work of James Hart Wyld, who was studying and building regeneratively-cooled motors as early as 1935 (his first successful test came on December 10, 1938, at New Rochelle, New York), they might have decided to construct a similar motor themselves. The design for Wyld's motor first appeared in the March 1936 issue of *Astronautica*, the organ of the American Rocket Society. Malina knew of Wyld's work, but never concentrated on research into such motors until Summerfield appeared on the scene in 1940. The reasons were two-fold: the ARS was low

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on funds in the late 1930s and Wyld left the group for a position with the National Advisory Committee for Aeronautics in 1939.

No new regeneratively-cooled motor would appear again until June 1941. [414] Since ARS work on them was sporadic in the late 1930s, the GALCIT crew did not focus much attention on Wyld's accomplishment. Secondly, such motors were much more complex than uncooled liquid propellant devices, requiring pumps and an efficient coolant flow system. Goddard had developed liquid propellant motors and intricate systems of pumps and tubing to supply them in the 1930s. But GALCIT rocket engineers were working somewhat in the dark, at least as far as Goddard's research was concerned. The GALCIT team's own struggle for funds in the late 1930s and the directions toward which Parsons was pushing the group—solid propellant testing—precluded sophisticated liquid propellant experimentation.

Of course, had the JATO team at Caltech realized the extent to which German engineers were succeeding in the production of a series of prototype A-4 missiles, their reaction undoubtedly would have been shock, dismay, and admiration. The A-4 (V-2) rocket was 46 feet high, had a regeneratively-cooled motor and could carry a one-ton warhead for approximately 170 miles. [415] But the German effort was a closely-guarded secret which concentrated its efforts at pulling together diverse talents and resources in one gigantic and well-orchestrated state enterprise. In the late 1930s,

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[414] For a dramatic description of Wyld's struggles to build the regeneratively cooled motor, see Winter (1983), pp. 83-85. The Soviets, at the Leningrad Gas Dynamics Laboratory, had also worked out a prototype self-cooling motor in the mid-1930s. But the Soviet Union kept its work secret and the Purge Trials of the late 1930s decimated the ranks of rocket scientists at the GDL.

[415] Neufeld (1995), pp. 281-282.

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American rocket engineers were a disparate group, haphazardly coordinated in their efforts. Almost all of them were operating without substantial government funding.

Thus, the construction of liquid propellant rockets came more slowly to America. Still, fundamental discoveries unfolded on both the east and west coasts, and the Caltech team's struggles were to bear fruit by 1942, with the discovery of an entirely new type of solid propellant. Although Malina, Tsien, and Summerfield's theoretical work was substantive, Parsons' invention of the castable solid propellant would prove crucial to the successful mass-production of JATOs.

Remarkably, the GALCIT team's long strides toward the development of reaction-propulsion theory and the invention of a completely new type of solid propellant did not make an appreciable dent in research priorities along similar lines at Caltech—even though another Caltech physicist, at almost the same moment in time, 1941-1945, was orchestrating an effort to build ordnance rockets for the US Army and Army Air Corps. Two programs moved forward simultaneously at Caltech; but neither had permanent links to the other, even though some of the same researchers were advisory or working members of both groups. [416]

While the GALCIT rocket crew struggled along, Charles C. Lauritsen (1892-1968), an eminent professor of physics at Caltech from 1930 to 1962, led a well-

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[416] Martin Summerfield, in his 1994 Interview with J. D. Hunley, only briefly discusses a tie between GALCIT rocket efforts and those of the National Defense Research Committee at Caltech. Apparently, most of the contacts came in the mid-1940s, well after both GALCIT and the Lauritsen group had become successful. See: Hunley Interview with Summerfield (1994), p. 17.

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financed team of physicists and chemists at Caltech from September 1941 until December 1945. Lauritsen's men developed and built solid propellant ordnance rockets for the army, navy, and Army Air Force. From the start, this project, funded by President Franklin D. Roosevelt's National Defense Research Committee, had powerful and influential backing.

The NDRC's chairman from the spring of 1940 on (until the end of the war) was Vannevar Bush, formerly Dean of Engineering at the Massachusetts Institute of Technology from 1932-1938. Bush also served as the director of the Roosevelt Administration's Office of Scientific Research and Development. Although Robert Millikan, Caltech's chief executive, was skittish about losing some of his prized professors to government war planning, the prestigious Department of Physics, backed by the Institute's Executive Board, supported Lauritson. Initially, the NDRC granted Lauritsen's Caltech scientists $200,000 (in 1941). By 1944, Caltech was spending $2 million a month to build solid propellant rockets—$24 million a year—far more than the GALCIT rocket research effort was ever to receive in a single year. In fact, by 1944 the Lauritsen team had turned out its one-millionth rocket at an ordnance plant in Eaton Canyon, near Pasadena. [417]

That such a well-established and lavishly funded rocket project could exist, right under the nose of the GALCIT Rocket team's work, and not build close ties to the GALCIT crew, is an astounding twist of fate. To understand how

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417 See Judith Goodstein's *Millikan's School: a History of the California Institute of Technology*. New York, W. W. Norton & Co. 1991. pp. 241-245. Millikan had actually served the war effort during the First World War, both at Caltech and in Washington, but he was concerned that the NDRC's request for Lauritsen to head the rocket development effort would pull the physics professor away from Caltech, and so preferred to have the entire effort based at the Institute. Eventually, the primary effort *was* centered at Caltech, although Lauritsen was frequently called away to Washington.

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the two efforts could essentially shadow-box each other, the time clock must turn back a bit, to mid-1940, when Vannevar Bush began surveying American university campuses for talented scientists and administrators to join the NDRC's work.

Bush himself never made a secret of his own doubts about the future of rocketry—he was not exactly an enthusiastic proponent of long-range rockets in the 1940s. Yet, as the head of the National Defense Research Committee, he was responsible for exploring every avenue of research beneficial to the war effort. Here a bit of politics enters the story. Robert Millikan actually sought the job Bush had won. According to Judith Goodstein, Caltech archivist and author of *Millikan's School*, "Millikan hoped to be called to Washington to direct the mobilization of civilian scientists for national defense, as had been done once before, in 1917." [418] Instead, Roosevelt tapped Vannevar Bush in the spring of 1940 to head the NDRC, "a federal agency with the power to award defense contracts and conduct research on military weapons." [419]

Even so, Caltech was not wasting any time either. Earnest Watson (1892-1970), the head of Caltech's faculty board at the time, had already proposed the formation of a Council on Defense Cooperation at Caltech and had appointed Richard Chace Tolman (1881-1948), Dean of the Graduate School, as its head. Caltech was early on positioning itself to play a pivotal role in wartime research and development, and undoubtedly expected to garner some of the lucrative government contracts that were sure to be offered up. By mid-June

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[418] Goodstein (1991), pp. 240-241.

[419] Goodstein (1991), p. 241.

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of 1940, a "Report on the Possible Contributions of CIT to the Problem of National Defense" was on Tolman's desk. In the meantime, Tolman had already taken up residence in Washington and become Bush's vice-chairman, in the NDRC's armor and ordnance division. His work at Caltech had evidently impressed the NDRC chairman. [420]

Tolman had every intention of recruiting Charles Lauritsen into the project. Yet neither Lauritsen nor Millikan were in any rush to see a nuclear physicist from Caltech ensconced in the bowels of the Washington bureaucracy. So Lauritsen angled to keep his research responsibilities at Caltech. This pleased Millikan, who feared "too many prima donnas in physics at any one spot." [421] When, in June 1940, Tolman picked Lauritsen to head the Caltech/NDRC rocket project, he knew he had selected, not only an outstanding scientist, but a highly politic young man. [422]

On September 1, 1941, just three months before Pearl Harbor, work began at Caltech's Kellogg Radiation Laboratory on the construction of rocket-propelled ordnance. A nuclear physicist had been appointed to head a project building rocket projectiles. It seems odd that the GALCIT group was not

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[420] Goodstein (1991), pp. 242-243. Tolman had been an army major in World War I and had served in the military's Chemical Warfare Service at American University. Of his work there, Tolman said " [The place was] a great mixture of business, science and politics....I enjoy all of them." Goodstein went on to describe Tolman's influence on the Caltech campus, where he had been a professor of chemistry and mathematical physics since the end of the war, as "considerable."

[421] Goodstein (1991). p. 244.

[422] Goodstein (1991), p. 243.

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recruited for this project. After all, the GALCIT building was equipped with its own wind tunnel, something the Physics Department could not boast. [423]

Actually, it would not have been entirely incongruous to find a nuclear physicist in charge of rocket development. True, Lauritsen was not an expert in rocketry, but there is no nuclear physicist without some degree of expertise in chemistry. And Lauritsen, like his boss, Tolman, had another distinct edge. Both men were savvy lobbyists. By and large, despite his persuasive powers, von Kármán was far more interested in working for the war effort through commitment to engineering and aerodynamic research than he was in knocking on Congressmen's and generals' doors. This may have been one reason why Robert Millikan did not champion the Hungarian scientist to head a second rocket development effort at Caltech. Furthermore, von Kármán was the head of an entire laboratory at the school, rather than the head of a particular department. [424]

In 1941, von Kármán was sixty years old, Robert Millikan, seventy-three, and Charles Lauritsen, forty-nine. Judith Goodstein, in *Millikan's School*, wrote

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[423] Lauritsen and his group of physicists at Caltech were responsible for several primary discoveries in nuclear astrophysics, charge symmetry, nuclear decay, and solid state physics over the years. See: Caltech Archives *On Line Guide to Collections*: http://www.caltech.edu/~archives/bios/Lauritsen CC.html.

[424] A revealing note about von Kármán's reluctance to work in bureaucratic environments came from his own hand: in his autogiography, *The Wind and Beyond* (1967), von Kármán wrote of a July 1944 meeting with Army Air Force General Henry Arnold at La Guardia field in New York. Arnold had just asked the scientist to prepare a study on the possibilities of air power in the future, including supersonic aircraft and rocket propulsion. "What do you wish me to do, General ?" von Kármán asked. "I want you to come to the Pentagon and gather a group of scientists who will work out a blueprint for air research for the next twenty, thirty, perhaps fifty years." Von Kármán replied, "General, I really do not like to work in the Pentagon, but I will do this on the following condition—that nobody gives me orders and that I do not have to give orders to anybody. " see: Von Kármán (1967), p. 268.

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that Millikan was "too old to come to grips with the sheer magnitude of the rocket project. Indeed, he had little to do with the project directly." [425] It is fruitless to speculate about the influence of age in the selection of Lauritsen to head the new rocket project. Von Kármán was an active, vital man at the age of sixty. But he was not technically part of 'the inside loop' at Caltech in 1940. He had administrative authority, but his relations with Millikan remained consistently cool, rather than truly friendly. [426] Millikan saw him as an original thinker and scientist, as well as as a conduit to the still young aircraft industry in southern California. Von Kármán's ties to the Washington establishment, especially through the Army Air Force, were not to mature until 1944. Millikan wanted him just where he was, as the director of GALCIT, a school endowed by a large philanthropic trust. What is more, with Richard Tolman in Washington, Caltech already had a scientist positioned in the corridors of power.

But of Charles C. Lauritsen, his own son Thomas wrote:

> I think Charlie worked more with individuals than through organizations. I guess there isn't any other way of doing it, but one thinks of writing a letter, 'Dear Bureau of Ordnance—I have the following': But Charlie didn't do that. He kept going around until he got hold of somebody whom he could understand and who could understand him—then things got done. [427]

Lauritsen could effectively organize rocket research at Caltech, without shifting his bureaucratic base of operations to the East Coast (that is, the

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425 Goodstein (1991), p. 252.

426 The tone of Millikan's letters to von Kármán (that is, those investigated by this researcher) remained consistently formal, cordial, and professional.

427 Goodstein (1991), p. 251.

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headquarters of the NDRC.) By contrast, von Kármán was known for his somewhat irreverent (although not impolitic) nature; by and large, he ran GALCIT his own way. Von Kármán grabbed talent as quickly and as astutely as he could. He didn't particularly mind by-passing established through channels.

Von Kármán had approved the creation of the GALCIT rocket project almost overnight, and did not object to the presence of men on the team who had not even a college diploma, much less a graduate career. To some extent, he just did not fit the mold of the 'team player.' This did not mean he would never accede to policy directives from the Faculty Council or from Millikan himself. He preferred to manage GALCIT on his own, so long as his graduate students did not blow up the laboratory. By and large, Robert Milliakn let him do just that. For the NDRC to enlist the support facilities at GALCIT, including the wind tunnel, would have meant collaborating with von Kármán. Instead, Watson and Tolman worked together and took what they regarded as the less problematic road to success.

This choice was to have profound implications for the future of rocketry in the United States. If GALCIT- 53 had become the standard propellant in hundreds of thousands of small ordnance rockets, it might have gained widespread acceptance and perhaps stimulated even more innovative research in the mid-1940s. Since 1939, when the GALCIT rocket team first accepted $1000 for its research from the National Academy of Sciences, funding prospects had been steadily improving for the group. When, after July 1, 1939, the NAS Committee on Air Corps Research gave the group its

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first large-scale ($10,000) contract, von Kármán's small rocket team had established its own niche at Caltech. [428]

The amiable relationship between the GALCIT director and General Hap Arnold was key to the Rocket Team's success in the early 1940s. Arnold was interested in furthering the capability of both bombers and fighter planes. He foresaw the use of rockets as armaments on aircraft, but first priority for him meant improving the performance characteristics of the planes themselves. Von Kármán had alerted Arnold to the possibilities of "jet-assisted" take-off—that is, of the potential for rockets as boosters on conventional aircraft, as early as 1938—and Arnold displayed considerable interest in the idea. [429]

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[428] This $10,000 was authorized by NAS Contract # W535-ac-15690 (formal date of implementation, 26 September, 1940.) Another $8,000 would come along in April 1941. This researcher has found no record of a larger $22,000 grant from the NAS in July 1940, as Koppes reported in *JPL and the American Space Program*. There is no mention of any such amount in either Malina's or von Kármán's autobiographical works. Captain R. C. Miles, who wrote a history of the ORDCIT Project in 1946—ORDCIT stands for Ordnance, California Institute of Technology— mentioned the NAS $10,000 grant on page 3 of his compilation. There is no evidence of another larger grant until the summer of 1941. As for the first $1,000, it probably was no coincidence that, when this money came through for the rocket team, both Robert Millikan and von Kármán were on the Air Corps Research Committee. Major Benjamin Chidlaw, aide to Hap Arnold, once asked von Kármán in late 1939 (before the NAS had given GALCIT its first large scale grant): "Kármán, do you honestly believe that the air corps should spend as much as $10,000 for such a thing as rockets?" See: Koppes (1982), pp. 8-11. Army Ordnance Records of GALCIT's funding in the early years can be found at the National Archives (Archives II) in College Park, Maryland, (Record Group 156.) Frank Malina's 'Report on Jet Propulsion for the National Academy of Science Committee on Air Corps Research' (December 21, 1938), is located in the Library of Congress, Malina Collection, Box. 9.1. This report helped convince the NAS Committee to grant funding to the Rocket Team. See: R. C. Miles, 'History of the ORDCIT Project up to 30 June 1946, Part 1.' JPL Archives, Document # HC3-326, p. 3.

[429] Hap Arnold and von Kármán had known each other since 1936. In December 1936, Arnold became Assistant Chief of the Air Corps. (He was promoted to Air Corps Chief in 1938.) From 1931 until 1935, Arnold was commandant at March Field near Pasadena and made occasional trips to Caltech to attend meetings on developments in aerodynamics. In 1938, Arnold and von Kármán discussed "rockets as a means of assisting bomber take-off." Von Kármán wrote of Arnold: "He was a stocky, broad-shouldered West Pointer with inquisitive eyes and a blunt but acute way of asking questions, which I enjoyed answering. He was also a literate man and endeared himself to me when I learned that like my own father he had written children's stories." See: Von Kármán (1967), p. 225.

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All through 1939, 1940, and on into the spring of 1941, the GALCIT Rocket Team struggled to build a solid propellant rocket that worked reliably. With uncertainty and meager success an everyday reality, von Kármán himself surely would have been reluctant to take on additional responsibilities for his crew, this time mediated through the NDRC—in other words, through civilian authorities. $18,000 in two-and-a-half years is a fair sum of money for a tiny group of virtually unknown researchers, and the Army Air Corps was taking a big risk in financing John Parsons' explosions.

Yet there is a moment of tangible and historic irony here. Parsons and Forman, soon to be joined by Mark Mills, had been investigating various solid propellants since 1939 and had discovered that the very propellant types Lauritsen's group was evaluating would not deliver the performance characteristics GALCIT was looking for. Ballistite, the first solid propellant tested by the Army Ordnance (NDRC) project, did not have the stability needed for a storable booster rocket. There was nothing like GALCIT - 53 in any other rocket engineer's arsenal. By early 1942, with the invention of the composite solid propellant, the GALCIT team had a product that was qualitatively different than any before it. GALCIT - 53 may not have had some of the attractive characteristics of the NDRC propellants, but it was far more innovative; and it would not crack or explode prematurely, to boot. [430]

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[430] It is entirely possible that members of the GALCIT group—Parsons in particular— might have been reluctant to share their creations with another team of scientists. Parsons had applied for a patent on his new asphalt-base propellant, GALCIT- 53. There were proprietary rights to consider, and neither he nor the other members of the GALCIT Rocket Team may have wished to share their inventions with Lauritsen, at least not without compensation.

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NDRC chemists and physicists were not working in the dark, however. They well knew the qualities of their materials. Ballistite was a double-base propellant composed of nitroglycerin and nitrocellulose. It had a higher specific impulse (I_{sp}) than either GALCIT- 53 or the propellant the NDRC team eventually chose, known as NDRC 218. TheI_{sp} of ballistite was 200 lb.-sec/lb., while that of the NDRC propellant and GALCIT- 53, 170 lb.-sec./lb. Ballistite was extremely temperature sensitive, making it unsuitable for use in any storable rocket, including NDRC armaments. It also burned quite fast.

However, the temperature sensitivity of NDRC 218 was only 18%, making it a much better choice than ballistite. (Temperature sensitivity is the percent increase in chamber pressure as the outside temperature increases). GALCIT-53's reactivity to outside temperature was half that—only 9.8%. Thus, NDRC 218, while more stable than ballistite, was twice as likely as GALCIT- 53 to lose thrust as the ambient temperature dropped. (The propellant would be burning at lower pressure.) While both NDRC 218 and GALCIT- 53 were somewhat labile under specific temperature conditions, the range of acceptable operating temperatures for Parsons' invention was wider. This was quite important, as the rockets would have to function flawlessly in a variety of combat conditions. [431] GALCIT- 53 was clearly the more stable propellant—and even easier to mix than either of the powder propellants. [432]

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[431] Above 120° F, GALCIT- 53 might begin to crack. It could, however, continue to operate at temperatures hovering around the freezing point. Temperature sensitivity became crucial at lower temperatures because, if the fuel were particularly sensitive to temperature changes in the atmosphere, its internal burning temperature, and thus, its pressure would drop, lowering thrust.

[432] J. W. Parsons, and Mark W. Mills. 'The Development of an Asphalt Base Solid Propellant.' *GALCIT Project No. 1, Report No. 15* (1942), p. 4. JPL Archives, Microfilm Document # 614-251, Item 11.

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Lauritsen's rocket group had begun its work in 1940. There simply was no such thing as GALCIT - 53 that year. Parsons was busy experimenting with his own powder propellants and frequent explosions were highlighting their limitations. He would use these double-base powders in the first GALCIT JATO tests, August 1941.

These JATO tests only proved to Parsons that he had to find a new propellant. By the fall of 1941, he knew he could no longer circumvent an unpleasant and frustrating truth: his choice of powders would consistently create insurmountable storage problems. Theoretically, he could have switched to NDRC-218; but this was not Parsons' way. He invariably preferred his own indpendent path. The impasse led him to the invention of GALCIT- 53 late that year or in early 1942. The Lauritsen team, with more money in its pocket than GALCIT would see for more than two years ($200,000), continued to work with propellants that Jack Parsons had rejected. Mark Mills and Louis G. Dunn later wrote of NDRC 218, the molded propellant Lauritsen's group settled on:

> This important group of propellants is lower in specific impulse than the double-base materials, but have about the same impulse as the GALCIT propellants. They are not limited in their use by either temperature sensitivity or temperature limitations. The molded propellants are rather difficult to prepare, requiring a milling operation to prepare molding powder from the basic ingredients (ammonium picrate, sodium nitrate, and a resin binder). The molding powder must then be pressed into grains of the desired shape at a pressure of several thousand pounds per square inch. Because of this procedure, difficulty has been encountered from time to time in quality control. [433]

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[433] Louis G. Dunn and Mark W. Mills. The Status and a Future Program for Research and

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To be sure, NDRC 218 worked well, despite its more complex production process. After all, more than a million of Lauritsen's rockets, all of them packed with NDRC 218, came off the assembly line in Eaton Canyon, California (north of Pasadena). However, in terms of technological advance, the tiny GALCIT team had scored a coup, the significance of which could not yet be fully calculated, even by its own members. The US military, working directly through one unit at Caltech, had moved ahead of a civilian agency of the government, working with another department at the same institute. GALCIT- 53 could easily have been adapted for use in Lauritsen's Ordnance air-to-ground and air-to-ship missiles. But a complex maze of administrative and personal coincidences prevented both Caltech and the government from maximizing on the GALCIT discovery.

Even more remarkably, Bruce H. Sage, Caltech chemist and advisor to Parsons on the properties of GALCIT solid fuels, was also the chief chemist on the Eaton Canyon Project. In fact, Eaton Canyon became a major preoccupation for Sage during these years; he was obviously less intensively involved in GALCIT. The extent to which he shared information on the two projects is unknown, but it is not likely that he would do much swapping of technical data, as details of the Lauritsen rocket project were kept secret from most people in the Caltech community. Any government-sponsored project, after 1940, would have adhered to strict national security rules, most especially those funded by the military. In fact, Vannevar Bush, as chief of

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Development of Solid Propellants.' *ASF Ordnance Department Memorandum No. 4-5, JPL GALCIT*. (March 19, 1945), p. 10. JPL Archives, Document # 3-549. In 1945, Louis G. Dunn was a young physicist and the Assistant Director of JPL. When Frank Malina resigned from JPL in 1947, Dunn became the second Director of the Laboratory.

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the NDRC, routinely classified documents and work he considered to have the potential of falling into enemy hands. [434]

And so Caltech benefited by having two government contracts for rocket development simultaneously. But a valuable opportunity to explore the expanded use of GALCIT- 53 in other US rockets was lost. No doubt, the reputation for unorthodox operational methods at GALCIT, and the notoriety the Rocket Team had acquired for itself—with explosions occasionally popping near the GALCIT building, bedeviled them. Despite important discoveries, Malina's group still had an undisciplined streak, and even he sometimes despaired of its future. [435] Nevertheless, neither Millikan nor von Kármán had lost faith in the crew. The two scientists were, according to Malina, simply looking for solid experimental results. [436]

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[434] Von Kármán wrote in his autobiography that Bush automatically classified equations von Kármán derived on the elasticity of structures subjected to blast waves. GALCIT's director wanted the equations published in the National Academy of Sciences *Proceedings* (1940), but Bush refused. By mid-1940, as President Roosevelt ordered a massive increase in aircraft production, any work done for the NAS and the Army Air Corps was declared secret. Such regulations annoyed Frank Malina no end. Von Kármán (1967), pp. 247-248. See also: Koppes (1982), p. 15.

[435] At several points in Frank Malina's 1939 and 1940 letters to his parents, he mentioned the difficulties facing rocket research at GALCIT, and his personal frustration at the slow pace of progress. Following the March 1939 explosion of their liquid propellant test motor inside the GALCIT building, Malina came to associate every explosion on the campus with John Parsons' work. On August 30, 1939, he wrote his father of an explosion at the Caltech Chemistry lab. "We heard it while eating breakfast," he commented, "and of course, at first thought it was our apparatus again. I looked at my watch and saw it was 8:20, which I was sure, was too early for Parsons and Forman to be at work and I was correct." In several letters in late 1939 and into 1940, Malina wrote of "tough new problems in rocket research," and how "rocket research consisted mostly of repair and construction of apparatus." On March 11, 1940, he wrote home: "The difficulties we have been having with rocket research could make one skeptical of success in the near future, but I am still hopeful." Frank Malina. 'Excerpts on Rocket Research and Development from Letters Written Home by Frank J. Malina Between 1936 and 1946.' Washington, NASA History Office, Lek/1/12/3. Letters dated 27 March 1939, 21 August 1939, 3 December 1939, 19 February 1940, and 11 March 1940, pp. 27-32.

[436] Malina, Letters Home, 20 March 1939, p. 26.

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There is no doubt that political and administrative considerations took precedence over technical factors in the choice of Lauritsen's physicists as builders of the NDRC rockets. After Robert Millikan delegated responsibility for Caltech's connection to the National Defense Research Committee to Tolman, Lauritsen's personal connections to Tolman became crucial to his appointment as chief of the ordnance project. As Goodstein wrote,

> Lauritsen...headed the list of scientists Tolman...meant to recruit for national defense work. 'I want to know if you would consider the possibility of coming on here [at the NDRC] if the right opportunity should develop,' Tolman wrote Lauritsen at the end of June of 1940, adding, 'You would have to explore this very cautiously since I presume the Institute does not want to lose its top men even temporarily....Your brains and sense would make a big dent in Washington. You might even bring on your whole crew....Please write me. I miss your counsel and advice every day.' [437]

The physics professor clearly had an inside track for the NDRC contract. Still, on the face of it, von Kármán held a position at Caltech nominally comparable in prestige to Tolman's. He could have asserted his influence and lobbied harder. The reasons why this did not happen involve more than simply an assessment of von Kármán's administrative style and personality.

First, von Kármán knew the state of affairs at GALCIT quite well—Frank Malina, John Parsons, and the small band of rocketeers were in no position to absorb a rocket production contract in 1940; the team was not even able to turn out a reliable working rocket motor by then. Malina was just completing his Ph.D. thesis and Parsons had yet to develop the composite propellant,

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[437] Goodstein (1991), p. 243.

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GALCIT- 53. In the summer of 1940, when Tolman became vice-chairman of the NDRC, the GALCIT rocket group did not have a marketable product to offer anyone, much less the US government.

Secondly, Millikan's reluctance to part with von Kármán cannot be minimized—if GALCIT had acquired the NDRC rocket contract, the aerodynamicist would have had to supervise the project, both from Washington and Pasadena. Long trips to and from Washington would have become routine for von Kármán. Millikan was reluctant to release von Kármán for any length of time. He wanted GALCIT to develop into the premier aeronautics institute on the West Coast, and in 1940, drawing away its guiding force to government work would have crippled the school.

When Millikan went to Washington in May 1940, invited there by Secretary of the Treasury Henry Morgenthau, he asked his aerodynamics star to accompany him. Morgenthau had convened a meeting of scientists to explore methods of improving military aviation. Von Kármán was assigned the task of submitting a plan for the standardization of military training planes. He was only too happy to serve. But this lasted only for a period of three to four months—after which Morgenthau dissolved the committee. [438] Von Kármán later admitted that he was relieved to return to more theoretical academic work ("more exciting areas of aviation," as he put it.)

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[438] Von Kármán (1967), pp. 246-247. According to von Kármán, Morgenthau, an unlikely candidate as supporter of military aviation, called the meeting because he was convinced the US would not be able to stay out of World War II and was upset that the Ford Motor Company, which he had asked to develop war production contingency plans, had refused to do so.

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Still, his choice of von Kármán to share in the Morgenthau committee work was an astute one. Millikan had been the catalyst for the von Kármán-Hap Arnold meeting in 1936. [439] He knew of von Kármán's many contacts with the general since then, and of the GALCIT director's admiration for the military. By 1940, von Kármán had been at Caltech for more than ten years. He had heavy responsibilities, not only at GALCIT, but as an advisor to aviation firms such as Northrup and Lockheed. Von Kármán was becoming restive. Even so, Millikan had every intention of reaffirming the value of his aerodynamics chief to the Institute.

As late as 1945, after von Kármán had already effected a *de facto* separation from GALCIT, as head of the Army Air Corps' Scientific Advisory Group, Millikan was still extolling his virtues:

> In Karman and Clark [Millikan] and their younger co-workers I think we already have as able a group of engineers as can be found anywhere, and it would be disastrous to this area [southern California] if we lost them. It would be natural for them to go where they can have outstanding facilities to work with. We must find a way to provide such facilities. Big salaries cannot pull men like Karman and Clark away, but lack of adequate facilities might force them out. [440]

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439 Von Kármán (1967), p. 225.

440 Letter, Robert A. Millikan to Charles W. Nash, May 14,1945. Microfiche copy in Millikan File at National Air and Space Museum Archives, Garber Facility, Suitland, Maryland, accession # XXXX-0537. The Scientific Advisory Group became a key player in the development of American air power in the late 1940s, recommending the development of jet and supersonic aircraft for the Air Force. Von Kármán's own description of how Hap Arnold recruited his services at SAG are in von Kármán (1967), pp. 268-269.

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Meanwhile, in mid-1940, surviving on the 1939-1940 NAS grant, Malina, Parsons and Forman had only one major goal—produce a propellant that wouldn't explode unexpectedly. Von Kármán actually hoped, through his connection to Hap Arnold, that the military would soon underwrite the Rocket Team's work directly. Perhaps, with a stable source of income, and the military's deep pockets, the crew could concentrate more on technical problems and overcome them more quickly. In fact, despite Malina's dislike for the secrecy incumbent upon military-related projects, Air Corps sponsorship seemed a practicable road to follow. After all, General Arnold's Committee on Air Corps Research had been supporting the rocket crew more than a year; and during that fateful year, 1939, the GALCIT team had begun to entitle its reports 'Air Corps Jet Propulsion Research Projects.'

Malina, Parsons, and Forman prepared a technical study of more than 100 pages—the 'Final Report for 1939-40,' completed June 10, 1940. [441] This report, which drew on Malina's Ph.D. research and data from numerous experiments conducted by Parsons, provided solid evidence of technical progress in both liquid and solid propellant motor research. Undoubtedly, beyond its use as a technical document (it remained unpublished), 'Air Corps Jet Propulsion Research Report No. 3' was meant to be a blueprint for continued NAS or direct military funding.

However, the 'Final Report for 1939-40' produced no immediate fiscal gain for the group. No new air corps money came until April 1941, when the

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[441] Malina, Parsons, and Forman. Air Corps Jet Propulsion Research, GALCIT Project No. 1, Report No. 3, 'Final Report for 1939-40.' June 10, 1940. JPL Archives, Document # HC3-1206.

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Army Air Corps awarded another $8,000, most likely as an extension of the original 1939-1940 contract. [442] $10,000 in two years—this was no paltry sum in the early 1940s. Still, despite mathematical and experimental evidence of the group's competence, no JATOs had yet been built. The AAC would consider the advantages of these small auxiliary rocket motors, but was in no way fully committed to their development; in any case, theoretical assertions about the reliability of a restricted burning solid propellant were one thing; actually getting the motors to work well was another matter altogether. [443]

Thus, Parsons' 1942 invention of GALCIT- 53 would have a dramatic impact on the very structure of the GALCIT Rocket Team, transforming the group from a strictly academic and experimental band of men into a production unit. With the creation of GALCIT- 53, the rocket crew possessed a composite fuel that was eminently marketable.

Just as novel as was the invention of GALCIT- 53, the strange circumstance of its development at Caltech was even more so. By 1942, the army had cultivated two parallel tracks of rocket development at Caltech; one—Lauritsen's group—was involved Army Ordnance work through the NDRC, while the other (GALCIT) contracted its efforts through the Army Air Corps. In effect, the military duplicated its research and production facilities at the Institute. If GALCIT- 53 had been ready by the summer of 1941, it is possible that it would have become the propellant of choice for both teams. But here,

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[442] Malina, Letters Home, 16 April 1941, p. 37. Malina's only reference to the $8,000 supplement came in this letter to his parents in mid-Spring, 1941. Contract extensions were not at all unusual. A new supplement to a contract often sufficed to prolong the agreement's effective life.

[443] Malina (1969), p. 158.

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timing was of the essence. From a production standpoint, time was not on the side of the Rocket Team in early 1942.

Yet, as radical a departure from powder fuels as they were in 1942, GALCIT- 53 and its successor, GALCIT 61- C, were relatively easy to prepare. After potassium perchlorate, asphalt, and No. 10 oil were weighed out in their proper proportions (the perchlorate constituted 75% of the mixture; 0.85 Floatine No. 3 asphalt and 0.15 S.A.E. No. 10 oil, the remaining 15%), the entire brew was brought to a temperature of 350° F in a large kettle. The kettle then dipped into an oil bath, in order to stabilize the temperature. There was little danger of ignition or explosion during the process, because the asphalt mixture had a flash point of between 460° and 500° F. [444]

In a separate kettle, another mixture of asphalt and oil would be bubbling away, waiting to be poured into the JATO casing as a preparatory liner. This asphalt liner, once hardened, formed a tight seal with the propellant after the latter filled the JATO canister. The lining process could be performed by hand. The propellant was then actually scooped into the container vessel (the JATO), bounced a few times to "assure uniform settling," and set aside to cool for approximately two hours. [445] No large presses, high pressure apparatus, or exotic ingredients were required in the manufacturing process. The next step was mass-production: in order to make this new JATO fuel a marketable

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[444] Parsons and Mills (1942), Table II, p. 12. Asphalt has a chemical composition of approximately 85% carbon, 10% hydrogen, 3% sulfur, and trace amounts of nitrogen and oxygen. See: Table VII-A, p. 31.

[445] Parsons and Mills (1942), pp. 14-15.

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commodity, some type of mechanized assembly line manufacture was a necessity.

The physical and pyrotechnic characteristics of GALCIT- 53 were equally straightforward. The propellant, once cooled and hardened, was "a black plastic material, having the appearance of stiff paving tar at ordinary temperatures. It [was] not soluble in water, but [was] soluble in benzene, gasoline, carbon tetrachloride, and similar solvents." [446] By mixing these ingredients in, the fluidity of the mixture would change, and thus its storage characteristics, as well. (Too much solvent meant the hardened fuel would become too brittle.) The finished propellant had the added advantage of being safely storable under a variety of conditions. It held up well through a temperature range of approximately 100° (that is, without cracking or bubbling.) GALCIT- 53 ignited only very slowly with a match and could not catch fire when ignited by a spark or glowing ash. It was even possible to stand next to an open JATO canister containing GALCIT- 53 with a burning cigarette, flick the ashes into it and not ignite the material.

Combustion of GALCIT- 53 required, in fact, the use of an ignition squib which was inserted into the propellant. Once lit, the charge burned "slowly and evenly at a rate of about 0.031 inches per second." [447] If the ratio of nozzle diameter to combustion chamber were correct—approximately 13.62—then the propellant would burn with a chamber pressure of 1800 lbs./square inch, giving an average exhaust velocity of 5300 feet/second. [448]

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[446] Parsons and Mills (1942), p. 16.

[447] Parsons and Mills (1942), p. 16.

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The ignition squib had an ingenious design and bears some description. It required a thin outer layer of combustible material, pasted on to the outside of the GALCIT- 53 charge. This pasty mixture included powdered aluminum, pine tree resin, and acetone. The squib itself was little more than a short piece of nichrome electric wire with a small pellet of liquid phenolic resin coated in fire clay, so that only its inner surface burned. The JATO assembler actually inserted the squib into the pasty ignition mixture prior to installation on an aircraft (or storage in some facility.) When the aircraft's pilot wished to fire the JATO unit, he merely touched a button in the craft's cockpit, sending a six-volt charge through the nichrome wire. The pellet burned at the inner end of the squib, and the resulting jet of flame ignited the GALCIT- 53 charge. The whole ignition process took less than half a second. [449]

By 1945, two improved versions of the asphalt base propellant, GALCIT 61- C and GALCIT- 65, had been developed. Both of these newer propellants demonstrated equivalent or greater specific impulse; GALCIT 61- C had an exhaust velocity of 6000 ft./second. The only difference between GALCIT 61- C and GALCIT- 53 was the addition of Texaco No. 18 asphalt to the mixture. GALCIT- 65 included a bit of potassium nitrate, but with the exception of this chemical, was essentially the same as its predecessor. [450]

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[448] Parsons and Mills (1942), p. 16. In addition to providing a satisfactory exhaust velocity, GALCIT 53 burned at an average rate of 1.25 inches/second. Thus a JATO unit 24 inches long would burn, on average, for more than 19 seconds. (These figures assumed a chamber pressure stated above, 1800 lbs/sq. inch.) The diameter ratio of 13.62 refers to the width of the nozzle at its throat, in relation to the width of the combustion chamber at its widest point. Thus, the very same factor which Malina had considered crucial to successful liquid propellant burn—the diameter of the nozzle at its throat—also pertained to solid propellant motor design.

[449] Parsons and Mills (1942), pp. 21-22.

[450] Louis G. Dunn and Mark W. Mills. The Status and a Future Program for Research and

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GALCIT 61- C and GALCIT- 65 even boasted an improved range of safe operating temperatures, from -20° F to 140° F, improving the limit by 60° F.

Even though the propellant was not exactly smokeless (due particularly to perchlorate combustion), it was so reliable that by the summer of 1942, GALCIT had its first contract with the navy for the production of JATO units. The Guggenheim laboratory benefited financially from this contract. Meanwhile, key members of the Rocket Team, (von Kármán, Parsons, Malina, and Forman) incorporated as the Aerojet Company in March of that year. Aerojet, too, was to negotiate several lucrative contracts with the Army Air Corps for JATOs.

However, this success did not arrive overnight. The new composite propellant's discovery was certainly not influenced by rivalry between two independent rocket research groups at Caltech. After all, Lauritsen's team and the GALCIT engineers had little communication with each other. GALCIT- 53 was the fruit, not only of Parsons' fertile and imaginative mind, but of necessity. The Army Air Corps was financing the team's work and expected concrete results. In the spring and summer of 1941 Parsons, Forman, along with GALCIT technicians, were busy stuffing double-base or black powder propellants into metal canisters.

While refinement of these units made successful tests possible, the first JATOs were not trouble-free. In fact, enough of them exploded while being tested on the ground (or in the air) to force Parsons and his men to press the

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Development of Solid Propellants.' *ASF Ordnance Dept. Memorandum No. 4-5* (1945), p. 21. JPL Archives, Document #3-549.

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powder into the JATOs no earlier than the night before their expected test. What was worse, the units could not be stored safely for more than a few days. If temperature conditions changed dramatically overnight, the crew had to discard the prepared units or risk explosions. Nevertheless, the dramatic increase in take-off speed—more than 25%—and the steep angle of ascent of the small single-engine training aircraft (known as the Ercoupe) while under JATO assist initially impressed both navy and Army Air Corps observers of the JATO tests. Eventually (by May 1942), the navy ordered 100 of the units from GALCIT. Navy men foresaw their use on board carrier-borne aircraft. However, this order only came through after the successful testing of GALCIT- 53, not directly after the August 1941 Ercoupe tests. In the midst of creation lies the misery of expectancy and delay. [451]

The Army Air Corps was even more noncommital. The army was unsure what use it might make of the JATOs and was so concerned about storage problems that it preferred to see what progress GALCIT might make with liquid propellant JATOs. The Air Corps leaned more in 1942 toward the latter—once Parsons had developed RFNA as an oxidizer—because Air Corps planners thought it might be possible to ship the units empty to a theater of operations and then load the JATOs when needed. In 1942, the Air Corps considered the very real possibility of an island-hopping campaign against the Japanese. Short runways on many of the Pacific islands might necessitate

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[451] This first navy contract with GALCIT was Contract No. NXs 5879, running from May 25 June 25, 1942. The JATO units had a manufacturing cost of approximately $400 each. See: John Parsons and Mark Mills, 'Progress Report on the Development of 200-lb. Thrust Solid Propellant Jet Units for the Bureau of Aeronautics, Navy Department.' Air Corps Jet Propulsion Research, GALCIT Project No.1, Progress Report No.1, Contract No. NXs 5879, August 30, 1942. JPL Archives, Doc. # Microfilm # 651-245, Item 23. On June 25, 1941, the GALCIT Rocket Research Project received $115, 500 from the NAS/Army Air Corps Research Committee for fiscal year 1942, two months before the Ercoupe tests.

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booster rockets, particularly for heavy bombers. JATOs would also increase the actual air speed of bombers enroute to their targets. However, the AAC was in no sense clear about its objectives, for in favoring liquid propellant oxidizer combinations such as RFNA and gasoline (later RFNA-aniline), the problems of handling and transport of toxic materials would have proven quite formidable. The Ercoupe tests, for all their technical success, failed to wholeheartedly win over Air Corps planners to the value of rocket technology. This frustrated the GALCIT team, but in no way halted continued testing.

Back in 1936, Malina, Parsons, Forman and Bollay had formed the GALCIT Rocket Research Team to investigate the development of a *sounding rocket,* and not to produce rocket units for the military. The sounding rocket was intended to explore meteorological conditions in the upper atmosphere—its design was entirely for scientific research, and not meant for military use. Yet by 1941, the team was gearing up to produce, *en masse,* small rockets for war.

The GALCIT men had not intentionally lost sight of their original goal, but had drifted away from it, realizing early on that to build an entire rocket would have proven too ambitious a project. Subsequently, by fashioning a rocket motor that worked, whether solid or liquid propellant, they threaded their attention through the needle-eye of propulsion system performance, rather than toward the broader ultimate objective of rocket construction.

In truth, Parsons' incessant testing of solid fuel motors had won the day. No doubt, Malina wanted to test and build rockets, too. When, in December 1938, Malina argued the case for NAS support for rocket work at GALCIT, he had

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successfully petitioned for the $1000 grant from the standpoint of theoretical potential. Experiments had so far failed to provide any clear breakthrough beyond short duration test firings. Parsons' explosions drove von Kármán and Malina back to their notebooks, if only to show that a solid propellant motor could theoretically function for more than a few minutes. The result was the report, *Characteristics of the Ideal Solid Propellant Rocket Motor,* (Jet Propulsion Laboratory Report No. 1-4, 1940) which demonstrated to their own satisfaction that Parsons' experiments were not in vain. [452]

Before 1939 neither Malina nor Parsons openly confronted the possibility that their efforts would lead them into military contracts. But when military applications of their research blossomed into the dominant reality in the early 1940s, the GALCIT team had no intention of halting its work. Quite the contrary, for Malina, Parsons, von Kármán, and others on the team soon profited from it.

To say, however, that these young men had lost their idealism would be inaccurate. The change in the direction of their research to new applications was more subtle. After all, they were working under a professor who, despite his dislike of military bureaucracy (based on his own memories of service in the Austro-Hungarian army during the First World War), wanted to be of service to the US military. Von Kármán was a complex individual, one whose mistrust, or at least his bemused annoyance with military inefficiency, could be coupled with admiration for the uniform, the idea of service, and

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452 Von Kármán and Malina. 'Characteristics of the Ideal Solid Propellant Rocket Motor.' JPL Report No. 1-4, 1940. In: von Kármán, *Collected Works, Vol. IV* (1956), pp. 94-106. This monograph bore the appellation 'Jet Propulsion Laboratory,' although, when it appeared, the name had probably been applied *post hoc*.

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loyalty to his adopted country. Nor could he forget the anti-Semitism which had partly driven him from Germany and the reality of the Nazi invasion of France in May 1940. GALCIT rocket work became a component of von Kármán's contribution to the United States and to its imminent participation in World War II.

In 1939, von Kármán's crew discounted military interest in their rocket work. "Army Ordnance, we were led to believe, had no use for rockets whatsoever," von Kármán later wrote. [453] Actually, this proved an oversimplification, for the attitude among military men toward rockets as weapons was rather, an ambivalent one. Artillery and infantry preparation may have been foremost in army tactical thinking, but ordnance officers could not have been ignorant of the value of rockets as weapons. After all, rockets had been used against American military forces as early as the War of 1812. Beginning in 1942. NDRC battlefield rockets produced at Caltech would be filling Army Ordnance warehouses in large numbers.

More ironic, however, was the Air Corps' hesitancy to embrace the rocket. Not every general was as perspicacious as Hap Arnold. Many were frankly skeptical about the future of the rocket and did not relish sidetracking funds for aircraft research to such an exotic device. Even after the Second World War, as eminent an advocate for air power as Lieutenant General James H. Doolittle (1896-1993) admitted that "we [the Air Force] sort of dragged our feet....We knew the rocket was coming, [but] we were reluctant to turn over to

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[453] Koppes (1982). p. 8.

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the missile people and their supporters all of the funds that we had been receiving for the development of the airplane...." [454]

On October 24, 1938, however, an officer from Army Ordnance visited the Caltech campus. This occurred on the heels of Hap Arnold's visit to von Kármán in May of that year. The officer, whom Malina never named, downplayed the chance that the army might help underwrite GALCIT's rocket experiments. Malina wrote home to his parents the same day:

> A "big shot" from the Army Ordnance division was here today. He told of the army's experience with rockets and thought there was little possibility of using them for military purposes. I silently rejoiced, however Parsons, who is about broke, was not so happy about it, as he hoped to get some funds for research from the army. [455]

This officer's belief may have permeated throughout the ranks of army planners or it may have reflected one school of thought in an ongoing debate. More to the point was that in mid-1938, GALCIT had no workable rockets to display. The problem with military acceptance of rockets in 1938 America was that relatively few successful demonstrations had taken place to convince Ordnance of the new technology's worth. [456]

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[454] Eugene Emme and W. D. Putman. Historical Interview with James H. Doolittle, Lt. General, USAF (ret.), 21 April 1969. Montgomery, Alabama, USAF Historical Research Agency. Oral History Interview #625. Document No. 3-7429-49, p. 5.

[455] Malina, Letters Home, 24 October 1938, pp. 22-23. Malina did not speculate as to why an Army Ordnance officer should appear at GALCIT five months after Hap Arnold's visit. It is possible that Ordnance had heard of General Arnold's enthusiasm for rocket work at Caltech and wanted to investigate more for itself.

[456] Army Ordnance was definitely interested in *studying* rocket propulsion in 1938, for two Ordnance researchers published reports on "rocket propulsion of projectiles" used by Malina in his own investigations. The men were H. P. Hitchcock and Leslie A. Skinner. Skinner served as liaison officer from army ordnance to the GALCIT project in the mid-1940s. See: Malina's 'Characteristics of the Ideal Solid Propellant Rocket Motor' (1940), in von Kármán *Collected Works, Vol.IV* (1956), p. 106.

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All the way through 1939 Malina rarely mentioned much in his letters home about military interest in his work. Instead, he commented on how "I must get the rocket research going again," or how "the Rockets are almost forgotten." [457] On December 10, 1938, he had written of his upcoming trip to Washington to present his report before the National Academy of Sciences Air Corps Research Committee and told his parents not to mention why he was going to the capital. In his letter, Malina added, "This secret and confidential business is very childish, but there is no point in bucking it at present." [458] Apparently he was beginning to think about the ramifications of his work somewhat differently. No longer were sounding rockets paramount in his mind. His annoyance evident, it seemed to Malina that military requests for secrecy were a nuisance, but one he could not completely ignore.

In a July 1939 letter to his parents, Malina mentioned nothing about the $10,000 NAS grant, only commenting that "the rocket research is coming along." [459] Malina accepted the need for that very secrecy which made him so uneasy, but not fully. Since his college days, Malina had sent home postcards, laconic missives about his studies, grades, vacation plans, etc. Sharing written details of his work with his mother and father was unusual for him.

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457 Malina, Letters Home, 22 January 1939, and 29 January 1939, p. 25.

458 Malina, Letters Home, 10 December 1938, pp. 23-24.

459 Malina's report to the NAS Committee on Air Corps Research included the frank admission that current research on solid and liquid fuel boosters did not bode well for the future of assisted-takeoff rockets. The black powder and double-base powders with which Parsons experimented had not yet produced the propulsive thrust needed to boost a plane into the air. Of course, this assessment was to be out of date within two years. See: Chang (1995), p. 83.

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He preferred terse, to the point statements, revealing his situation, but little about the complex range of emotions he might be struggling with at the time.

By the end of the 1930s, Malina had begun to admit one very uncomfortable truth to himself: it was better "not to buck" the system. Several forces were coalescing in favor of the military link. If he made his discomfort public, surely von Kármán would not have taken too kindly to his protest.

By 1940, with war on the horizon for America, military aircraft design and military involvement in aerodynamic research were open avenues of growth. The GALCIT rocket men were anxious for funds, and the source, even if morally troubling to any of them, was an abundant one. They were in search of recognition and a pathway to continued discovery. If rockets could help to prepare the United States for war with Japan and Germany, then rocket research was more than justified.

The plain fact was that, despite Malina's misgivings about involvement in war-related work, he did not openly protest the military connection with his research (at least not to his parents, with whom he was in regular contact). Von Kármán's association with the military had assured that the Air Corps would remain interested in rocket development. Looming conflict with fascism only served to further legitimize the war-related work. In effect, the JATO was born in 1939, well before the GALCIT research crew had fully realized that it would spend the next six years designing and constructing rockets for the army.

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The 1939 contract for $10,000 with the National Academy of Sciences Committee on Air Corps Research marked the official start of the JATO project. Transformed into the Army Air Corps Jet Propulsion Research Project, or GALCIT Project No. 1, by 1940, the work of the team ostensibly was directed toward the application of "super-performance" aircraft. Malina defined the project's goals as:

a) shortening the time and distance required for takeoff;

b) temporarily increasing the rate of rate of climb, and,

c) temporarily increasing level flight speed. [460]

Such performance enhancements could have applied equally well to civilian aircraft. In August 1939, Malina discussed just such a joint venture with Consolidated Aircraft of San Diego, California, endorsing take-off assist rockets and receiving $100 for preparation of a report on them to the company's president. Nothing ever came of this attempt. The NAS grant specifically pinpointed its sponsor: the army, and consequently it was clear that the JATO was destined for military application.

Between a dream and its fruition there is often a wide gulf. The first solid propellant JATO tests took place at March Field, near Pasadena between August 6 and August 23, 1941. But nearly two years of frustrating struggle preceded success. In July 1939, just after receiving the $10,000 grant, Parsons contacted Hercules Powder, for whom he had worked as a teenager, and began consulting with its experts about powder propellants. The company supplied him with GALCIT 1-A and GALCIT 2-B. Hercules Powder even installed a

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[460] Malina (1969), p. 158.

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press at its plant in Pinole, California and Shelby steel canisters, the heart of the rocket test motors, were fully charged there and shipped to Pasadena. Both these powders were essentially black powder mixtures, with potassium nitrate the main ingredient of GALCIT 1-A and sodium nitrate, the key to GALCIT 2-B.

Problems kept cropping up. The potassium nitrate powders burned too fast. The sodium nitrate mixture, which was "very slow burning," left solid residue and seriously eroded the nozzle. [461] Then there were the explosions. The very first test with GALCIT 1-A resulted in an explosion which severely damaged the test stand out in the *Arroyo*. Most of the powder pellets had arrived from Hercules Powder in "poor condition, many containing cracks and being crumbled on the edges." [462] This wouldn't do. Cracks in the propellant would inevitably bring on explosions. GALCIT 2-B (sodium nitrate) proved more satisfactory, provided that the ratio between chamber and nozzle throat diameter was less than 12:1. However, a fast burning powder would only provide thrust for a short period of time (usually around one second or so.) Parsons and Forman would have to start over. A new type of powder had to be tried.

Bullseye Dust seemed to fit the bill. It was a double-base powder, actually the leftover residue of the manufacture of smokeless powder; nitroglycerine (39%), nitrocellulose (60%), and diphenylamine (1%), a hydrocarbon with the formula $(C_6H_5)_2$, were its main components. [463] When mixed with

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[461] Malina, Parsons, Forman (1940), pp. 79-81.

[462] Malina, Parsons, Forman (1940), p. 81.

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potassium nitrate, the propellant was slow burning. Too slow, in fact. The propellant burned in layers for about 26 seconds. [464] Even with a conical hole impressed into the powder to insure even burning, Bullseye Dust just wouldn't provide the needed thrust.

Eventually a variant of the double-base powder called GALCIT-27, with an amide component (containing a $CONH_2$ group), became the propellant of choice for the JATO. Before the invention of GALCIT- 53, double-base propellants were the best improvement over black powder that Parsons could find. Experimentation with various combinations, coupled with feedback from chemists at Hercules Powder, slowly evolved a propellant that would work. But the trial-and-error process begged some important questions: in the spring of 1940, with Parsons plugging JATO canisters with one propellant powder after another, with explosions echoing across the *Arroyo Seco*, Malina and von Kármán grew irritated and tried to devise a mathematical answer to the troublesome question: would a solid propellant rocket really work? Could it fire for longer than a few seconds without exploding? So far, experiments had not given them much hope. Perhaps theory could sustain the drive Parsons was currently on, and soothe their frayed nerves in the process. And so the two engineers set out to solve equations, this time complex non-linear equations, to prove that a restricted burning solid propellant motor could actually perform. [465]

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463 Dean (1973), p. 7-198. Diphenylamine is a water-soluble but toxic hydrocarbon.

464 Malina, Parsons, Forman (1940), pp. 83-84, 86.

465 These four equations first appeared in print in von Kármán and Malina's JPL Report No. 1 4 (1940). The term JPL did not routinely appear on reports until November 1943.

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Malina, who prided himself on being a theoretician as well as practitioner of the art of rocketry, approached the failures and the frequent explosions of Parsons' powder rockets from the vantage point of the underlying physical dynamic. In his own words:

> As far as was known, no black powder or smokeless powder rocket had ever been constructed to meet these specifications of thrust and duration. *(that is, a constant thrust of 1000 lbs for a period of 10-30 seconds.)* Experts we consulted were very dubious about the possibility of doing so. It was generally believed that the combustion chamber pressure of a restricted burning solid rocket unit would continue to rise from the moment of ignition until any combustion chamber of reasonable weight would burst. In other words, it was thought that such combustion was inherently unstable. [466]

This instability troubled Malina and von Kármán. They did not look at the problem with an eye to replacing one propellant with another. Malina wanted to delve into the basics of combustion and the ensuing engineering challenges, so that he could evolve general principles of rocket combustion dynamics, regardless of the fuel chosen. Malina believed there must be a theoretical solution to the problem of instability in the rocket motor, even if only a partial one. In this he was correct.

Von Kármán provided him with four differential equations designed to prove that a constant thrust solid propellant motor could "fly." He loved to work late at night. According to Malina, von Kármán went into his study one evening in the spring of 1940, at the very least annoyed by Parsons' exploding rockets. He may also have felt challenged by the doubts of the

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[466] Malina (1969), p. 163. Note that material in italics has been added by this researcher. As has been earlier described, a 'restricted burning motor' burns only at its aft end, rather than throughout the propellant. Therefore, it must burn more slowly.

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experts, before writing out the four equations. (The equations appear in Appendix I of this thesis.) He then asked Malina to solve them. According to Malina, "Much to our relief we found that, theoretically, a restricted burning unit would maintain a constant chamber pressure as long as the ratio of the area of the throat of the exhaust nozzle to the burning area of the propellant charge remained constant, that is, the process is stable." [467]

But these equations themselves were no easy hurdle. Since they were non-linear equations whose solutions depended very much on the specific mathematical functions chosen for them, not all functions would work. Malina wrote: "We found that...a restricted burning unit would maintain a constant chamber pressure." But this did not mean von Kármán and he had jointly worked out the equations.

Malina struggled and eventually solved the equations on his own, although how long it may have taken him to do so, he never mentioned. He may have consulted with von Kármán while working on the equations, but the likelihood is that GALCIT's director, realizing that his graduate students had the raw data needed to solve them from their many experiments, allowed Malina to 'fill-in-the-blanks.' As it turned out, the process really was not so simple. To solve the equations analytically, one must define specific sets of functions for each derivative. This could be a time-consuming process. Malina may have chosen to find graphic solutions, using the data he already had from the group's many solid propellant rocket tests. [468]

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[467] Malina (1969), pp. 163-164.

[468] Malina may well have used data from previous experiments not performed at GALCIT. In the bibliography of his 1940 paper, (as mentioned above) Malina cited works by H. P.

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Since Malina did not make it abundantly clear that he consulted anyone else in working through these difficult functions, there is no way to know whether or not he may have enlisted Tsien's help. After all, he wrote that von Kármán gave them to *him* specifically. In their 1940 paper, *Characteristics of the Ideal Solid Propellant Rocket Motor,* Malina and von Kármán mentioned no one as an additional collaborator. It would have been highly uncharacteristic of either of them to deliberately deny a fellow researcher credit. Malina was, in fact, quite careful to recognize Tsien's contributions to his Ph.D. thesis.

Whatever the case, the equations' solution remains a clear achievement for any graduate student. The four differential equations provided the young engineers with a framework for understanding the burn rates of a solid propellant motor. They did not represent the real combustion characteristics of such a motor, but they did allow Malina to approximate the motor's functioning somewhat more precisely.

Malina always intended to contribute to the theoretical structure which, in his mind, underlay rocket science. All his training as an engineer bent him toward the firm belief that, without solid theory, there was no adequate foundation for understanding experimental data. It is likely that von Kármán, expert as he was, knew that basic experimental information was there, was familiar with the symbolic vocabulary of rocket combustion, and so sat down at his desk and thought out—or visualized—the dynamics of the

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Hitchcock ('Ballistics of a Rocket Propelled by Smokeless Powder') and L. A. Skinner ('Fourth Partial Report on Research on Rocket Propulsion of Projectiles'). Both men worked for Army Ordnance.

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rocket motor's operation. (That is, what the propellant burn rates must be for specific propellants, how great the volume of the gas expelled through the nozzle might be, how much empty chamber volume would then be left, etc.) Von Kármán knew his equations would be difficult to verify, and may even have advised Malina to solve them as directly as possible—graphically. What exactly happened is still a mystery. No notes, either in the hand of Malina or von Kármán, have come to light to clarify the matter. But Malina was confident that he had created a sufficient theoretical base to proceed. [469]

The truth is somewhat more complex. The solution of the equations yielded an important insight: the direct relation between surface area of the burning propellant and the diameter of the nozzle throat. If the proportion of burning propellant did not exceed the capacity of the nozzle throat to channel the hot gas in a uniform flow, then burning would remain a stable process. This realization gave the GALCIT engineers confidence to go on. The question of whether or not Parsons was impressed by their solution is another issue altogether. He would not have stopped his experimentation, even if Malina had never tackled the theoretics of solid propellant behavior.

In effect, the four equations legitimized once again the theoretical methodology by which Malina and other Caltech engineers were trained. However, through simple substitution of one propellant for another, Parsons discovered just the right propellant-oxidizer combination. His method was sound and positive and the results were clearly just a matter of time. A joint effort had brought the GALCIT Rocket team to the brink of success. But a

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[469] The author is greatly indebted to Professor Yuri Sereda, formerly Associate Professor of Mathematics at the Kharkov State Aviation Institute, Ukraine, for his many in-depth insights into the mathematics of Malina and von Kármán's discoveries.

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troubling issue lurked in the background, for theory seemed less a guide to experimentation than a justification for it.

In fact, the spring of 1940 was one of those "moments" in the Rocket Team's history when experiment seemed to fade in importance; soon rocket testing would again prove more efficacious than rocket theory. Malina's theoretical work did not alter the method of invention Parsons used; it did, however, provide reassurance to the mathematically-inclined graduate students that they were on the right track. Theory provided them with the incentive to keep on trying. There were to be many such points at which the limits of the unknown appeared to have been reached, whereupon some new vantage point, some new discovery would suddenly suggest another avenue of approach, and the work would again proceed.

And so it did. But before dramatic flight tests in August 1941 proved the value of auxiliary rockets to a group of assembled armed forces observers at March Field near Pasadena, a new infusion of funds literally flooded the Rocket Team's account. Even with JATO propellants facing an uncertain future, the Army Air Corps decided to push ahead with its commitment to research at Caltech. On June 25, the Air Corps delivered a Letter of Intent to the Institute, guaranteeing $115, 500 "to cover the cost of research, pending receipt of a formal contract." [470]

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[470] Miles (1946), Part I, p. 5. This contract, Army Air Corps Contract W535-ac-20260, became known later as JPL-1, the first contract issued to what would become the Jet Propulsion Laboratory. The contract was also significant in that it marked a shift to direct army funding at GALCIT.

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This news astounded the crew. Hap Arnold's interest in the JATO project had reaped rewards again, just at the right moment. The old 1939-40 contract had expired and cash was dwindling. The new contract called on GALCIT to "furnish and deliver a report in triplicate covering research on jet propulsion for airplanes." [471] The Air Corps fully expected in-air testing of the JATO, as well as development and "investigation of methods of quantity production" of more powerful motors—on the order of 1000 lb.-thrust capacity.

Their spririts lifted by this news, the rocket crew prepared for the Ercoupe JATO tests, then only two months away. A difficult and suspenseful summer ensued, because new and much higher expectations had been placed at their door, and still Parsons' powder propellants remained unpredictable. The Army Air Corps was taking a huge risk. This small eager group of experimenters had never seen such a sum of money before. To place in their hands nearly ten times the funds the team had received just two years before meant the connection to Caltech was solid, and the army was impressed with the earnestness of Malina and the team. The Army Air Corps grant could not have been purely a political move, the result only of a friendship between two highly placed individuals. The army must have sensed that experimental success for the Rocket Team was only a matter of time.

During mid-summer 1941, Lieutenant (later General) Homer Boushey of the Army Air Corps flew the Ercoupe monoplane from Wright field in Dayton, Ohio, to March Field. The plane had an ideal design for the first JATO tests.

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[471] Miles (1946), Part I, p. 5.

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It had the appearance of a small Piper Cub, or similar single- engine light aircraft, with a twin, rather than a single tail fin. Its wings were swept low on the fuselage. The plane's construction was aluminum, "with the exception of the outer wing panels, which [were] covered with doped fabric." [472] The Ercoupe's engine was rated at 65 horsepower at 2300 rpm. This was more than enough to lift the 1175 lb. plane into the sky. With two assemblies of three JATO units attached, one under each wing, the plane picked up an additional 187. 2 lbs. (Each JATO unit weighed 10. 7 lbs., without its mounting bracket.) [473]

Once Boushey had flown in to March Field, the challenges—and excitement—began. First the group had to design an installation assembly frame for the JATOs. No one individual took credit for this work in the report on the Ercoupe flight Tests, but Forman, Parsons, and Boushey himself may have played a role in its design. [474] Forman had a knack for practical and efficient machine construction, and Lieutenant Boushey undoubtedly would have wanted to offer some input into the units' installation. The assembly included mounting brackets for the JATOs, a track frame for the two separate wing installations, reinforcement brackets, and a battery for

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[472] Malina and Parsons. 'Results of Flight Tests of the Ercoupe Airplane With Auxiliary Jet Propulsion Supplied by Solid propellant Jet Units,' *Air Corps Jet Propulsion Research. GALCIT Project No.1. Report No.9 . September 2, 1941*, p. 4. JPL Archives, Document # HC3-64.

[473] Malina and Parsons. 'Results of Flight Tests of the Ercoupe Airplane....'(1941), pp. 7-10. Although the JATOs themselves only weighed 64.2 lbs, the mounting brackets, track frames, wing reinforcers , and battery added an additional 123 lbs. See p. 10.

[474] A support staff of enlisted men and civilians from GALCIT also aided in the installation and assembly of the rockets. These included a Private Kobe, Corporal R. Hamilton, and a certain F. S. Miller. Clark Millikan also participated in the tests, actually flying a spotter plane alongside the Ercoupe. Forman rode in the spotter plane alongside Millikan, taking color motion pictures of some of the flights.

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ignition. [475]

Only Homer Boushey received special credit in the report. Malina and Parsons acknowledged his courage in flying with rockets attached on the underside of his plane's wings with a rather understated comment: after the failure of a JATO unit under the right wing on August 8, the report noted that "The pilot deserves credit for his willingness to continue flight tests as soon as the airplane was repaired." [476] This particular mishap involved damage to the outer segment of the wing, a 10 inch hole in the fuselage, and a sheared bulkhead—a JATO had exploded even before take-off, forcing Boushey to bring the Ercoupe to a halt.

Two of the more ingenious design twists on the JATO units were, of course, the electric squib ignition system and the shear bolts at the rear of each unit. Should the propellant misfire, the back of the rocket, including the entire nozzle, would quickly blow off, preventing a massive explosion of the unit. This last invention forestalled disaster on more than one occasion. But the majority of the first JATO tests were successes. Of sixty-two different test runs, both on the ground and in the air, there were only eight misfires, including the August 8 mishap. The first successful flight, with six JATO units tucked beneath the wings, came on August 12. Just 13% of the units installed failed, a notable safety record. And, as the crew became more proficient at installing fresh units, the number of explosions or failures dropped precipitously. [477]

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[475] Malina and Parsons. 'Results of Flight Tests of the Ercoupe Airplane....' (1941), p. 11.

[476] Malina and Parsons. 'Results of Flight Tests of the Ercoupe Airplane....' (1941), p. 15.

[477] Malina and Parsons. 'Results of Flight Tests of the Ercoupe Airplane....' (1941), pp. 13-19.

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The key to success, (even more than Boushey's courage and the team effort involved from start to finish,) was the discovery that, the shorter the storage and transfer time from packing case to airplane wing, the greater the chance the JATO would fire without a problem. With powder propellants the hitch was lengthy storage. Even in controlled temperature conditions, cracks in the propellant tended to develop, the longer the unit was on the shelf. Chemical deterioration during storage proved the demise of the powder propellant JATO and led Parsons to re-think the entire propellant base.

Malina, Parsons, and the JATO crew never lost sight of their original goals in building and installing these JATOs. The army had demanded quicker take-off and a higher cruising speed from aircraft using auxiliary rockets. Unquestionably, the first JATOs dramatically improved the Ercoupe's performance. In two separate sets of flight tests, the plane, with JATOs ignited, saved an average of 252 ft. of runway upon takeoff, a savings of approximately 31.7 %. To make the job even harder for themselves, the crew set up a 50 ft. high obstacle across the runway and tested the Ercoupe's ability to clear it with JATOs firing. The takeoff distance required to clear this obstacle shrank from 950 ft. (normal takeoff, no JATOs installed) to 550 ft, or by 42.1 %. [478]

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478 Malina and Parsons. 'Results of Flight Tests of the Ercoupe Airplane....' (1941), p. 26. Essentially these results demonstrated that the plane could lift off the runway well before it reached the obstacle. Malina and Parsons commented in the report that the savings in takeoff distance fell 20% short of expected results. Theory had predicted the JATOs would cut takeoff distance by over 50%. The men concluded that, were Boushey given more time to improve "his takeoff technique," the distance would have shrunk.

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Just to push the limit of their experiments a bit further, and to "satisfy curiosity," the GALCIT team conducted two flights of the Ercoupe at 11,400 feet, near the small airplane's flight ceiling, and fired off its JATOs. During the second test, the plane's airspeed increased from 62 to 90 mph; the Ercoupe even climbed a bit. But the team quickly downplayed the significance of the results (a 56% increase in airspeed), saying that they could not be sure that the plane had maintained level flight during the test firings. [479] Still not satisfied with this incursion into the unknown, Malina and Parsons ordered up a test in which the Ercoupe, towed down field by a truck at 25 mph, bounced into the air, propeller-less, with JATO units blazing. "The plane climbed about 20 feet into the air. It was a small first." [480]

Actually, it was not quite that. The Russians had experimented with rocket-powered aircraft in the mid-1930s. Beginning in 1936, Sergey Korolev and engineers at the Gas Dynamics Institute in Leningrad had designed manned liquid-fuel rocket aircraft which flew (how successfully is not clear) twenty one times in 1940. The aircraft frame was a two-seater, the RP-318-1, powered by a sophisticated motor with a water-cooled jacket; the oxidizer-fuel combination was nitric acid and kerosene. Maximum flight time was approximately 3.8 minutes. [481] In the late 1920s, Hugo Junkers, the German aircraft designer, had dreamed of using rocket motors "to assist the takeoff of

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[479] Malina and Parsons. 'Results of Flight Tests of the Ercoupe Airplane ....' (1941), pp. 27-28. These tests took place on August 12 and August 21, 1941. Since the tests had only begun on August 6, the men had either gained a great deal of confidence in their rockets, or were being somewhat foolhardy.

[480] Koppes (1982), p. 12.

[481] I. I. Kulagin (1968). In: Durant and James (1985), pp. 97-102.

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heavy airplanes." The Germans had plans to place rockets in the tails of their production models as early as 1935 and actually flew a rocket plane, the Heinkel *He 176*, in 1939. The little Ercoupe, however, was still the first American airplane to lift off the ground under the thrust of rocket-assist motors. [482]

There was no way Malina and Parsons—even von Kármán—could have known of the German and Soviet accomplishments, given state secrecy shielding the work in these two nations. Ironically, for all von Kármán's travels in the 1930s and 1940s, inspecting aircraft plants in Japan, China, Italy—even Russia (1937)—he never had an inkling that such striking developments were taking place. [483] Again, Malina's men were working in the dark. If what the GALCIT experiments with the Ercoupe had to demonstrate was the ability of an airplane to fly aided by rocket power, then these objectives had been amply met. There seemed no need to set up additional barriers, such as the 50 foot high tape across the runway. But the rocketeers were not just being cavalier. They were pushing the limit, in order to discover how quickly and at what angle a propeller-driven plane could accelerate when under rocket assist. This would be a characteristic not easily missed by navy observers, who were evaluating the JATO's use on an aircraft carrier deck.

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[482] See: Neufeld (1995), pp. 10, 48, and design drawing of the He 176 following p. 82. In the 1930s, the Russians were also experimenting with ramjet engines as auxiliary power assists on fighter planes. A ramjet is not technically a closed system rocket motor, however, being more akin to the German V-1, or 'buzz-bomb.'

[483] Von Kármán personally toured the ZAGI, the Central Aero-Hydrodynamic Institute in Moscow that year, and came away with a mixed impression of Soviet progress in new forms of propulsion. In his memoirs, he even asked some strangely ironic questions: " How much of Russia would I see on this brief visit? What would I learn of Soviet scientific developments?" The answer is, not nearly as much as he might have. Von Kármán (1967), p. 185.

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Despite the first JATOs' obvious limitations—the result of using double-base propellant—the navy showed marked interest in the tests. The navy aeronautics expert attending the August tests, Lt. C. F. Fischer, left convinced of the novelty and potential of military value of the JATO. [484] But the navy soon insisted that these units deliver a 200 lb.-thrust, much more than the 28 lbs. produced by the original JATO units, and fire for 8 seconds each. These requirements set off a mini-crisis that at first stymied the team. JATO units generating a minuscule thrust were not what the army or navy needed. Besides, these units could not be stored for more than a few days, under optimal conditions. GALCIT certainly could not ship the August 1941 vintage JATOs to an aircraft carrier, where humidity and temperature conditions would vary from day to day, and where the rockets might not be installed until the carrier was days out to sea. Only in the summer of 1942, after Parsons had invented GALCIT- 53, could the team deliver on its contract.

The muted tone of the September 1941 report on the Ercoupe tests, the air of humility in Malina's writing (despite the grandstanding evident in some of the Ercoupe tests), was more than simply characteristic of his style—it suggested a revelation, of sorts. Outwardly, the GALCIT men rarely displayed either elation or disappointment. There had been too many near disasters and setbacks for premature celebration. Until 1945, despite technical and financial success during the war years, they had always maintained an air of restraint about their accomplishments. Inwardly, they may have experienced

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484 Malina (1969), p. 170.

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great pride, but not until the team had launched a sounding rocket 240,000 feet above the earth (fall 1945) did they truly revel in their success.

In the summer of 1941, moreover, neither Malina nor Parsons was satisfied with the JATO's propellant. The group had built a rocket motor, tested it under an airplane wing, gotten it to work successfully, worked through the theoretical analysis of jet-assisted takeoff, and still, there was more to be done. [485] If Malina and Parsons had reflected on how far their work had come, they would have admitted to themselves that the powder-propellant JATO was no small achievement for a group of recent Ph.D.'s, semi-professional rocket builders, and a young army pilot.

Von Kármán and Clark Millikan's participation notwithstanding, the team had achieved several firsts: construction of a small-scale solid fuel rocket unit capable of mass production, invention of an installation assembly under an airplane wing, and the snaring of a production contract with the US Navy. The GALCIT Rocket Team had come a long way since 1936.

Even the construction of solid fuel JATOs with GALCIT- 53 as the propellant did not represent the culmination of the team's work on rocket units. For, along with solid propellant research surging forward from 1940 to 1942, the

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[485] It was actually Homer J. Stewart and Clark Millikan who carried out the mathematical analysis of an aircraft's flight characteristics under rocket-assist power. Their preliminary report, whose formulae they incorporated into the Ercoupe study, first appeared in Air Corps Jet Propulsion Project Report No. 5, 'Aerodynamic Changes of Take-off and Initial Climb as Affected by Auxiliary Jet Propulsion,' January 14, 1941. This paper, mentioned in Malina's 1969 memoir, is in the JPL Archives, Doc. # Microfilm 614-251, Item 5. Several of the meeting notes and papers from JPL's early years are actually missing. However, Clark Millikan's Report No. 21, 'The Application of Jet Propulsion to Assisted Take-Off and Super-Performance of Military Aircraft,' May 28, 1943, is available. JPL Archives, Document # H-3-545, Vol. I.

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GALCIT team conducted tests of liquid propellant rocket motors which did not have to be discarded, but could be refueled after installation. The Army Air Corps, in particular, was looking for permanent rocket assist motors on heavier airplanes—bombers, in fact.

One major hurdle in constructing such motors, again, was the choice of fuel and oxidant. Since 1939, red fuming nitric acid (RFNA) had been the oxidizer of choice at GALCIT, ever since Parsons had discovered its potential as a substitute for liquid oxygen. The problem was its toxicity. RFNA was quite corrosive and could not be stored in JATOs for any length of time. This meant an entire corrosion-resistant tank would have to be 'on standby' for each liquid propellant JATO test. But an experimental test was not a combat flight. Either the GALCIT researchers would have to come up with a lined tank (as had the Russians, who used nitric acid regularly in their early tests and lined their motors with gold) or the army would just have to accept the awkward fueling characteristics of RFNA. It was the latter solution which became the norm. To complicate matters more, the army continued to use RFNA into the early 1950s, when it tried to deploy its first operational tactical missile, the Corporal. The result was a missile system that never fully met army needs. [486]

There was another problem with liquid propellant motors which GALCIT engineers had not solved by late 1941. This was the question, how to start them quickly, without producing an exhaust jet that would eventually become unstable and explode. The answer to this dangerous riddle was the

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[486] For an engaging description of the Corporal's development at JPL, and problems of its deployment, see Koppes (1982), Chapters 3 and 4, pp. 30-63.

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hypergolic propellant, one capable of instantaneously igniting. Malina and Ray Stiff would settle this issue after February 1942, but in 1941 there was still no assurance the Army Air Corps would wait until some as yet unknown solution popped up.

Malina had two advantages on his side in struggling through this dilemma. There was Parsons, whose mind was always actively turning over new solutions to serious problems. And, after the summer of 1940, Malina could look to Martin Summerfield, newly arrived at GALCIT with his Ph.D. in hand. Malina turned, perhaps naturally, to Summerfield.

Shortly after earning his Ph.D. in physics from Caltech in 1940, Martin Summerfield began his work on liquid propellant motors. He did not exactly appear to be the best choice for this work, since his graduate work in physics had been in optics, or in his own words, "x-rays and infrared radiation." [487] Summerfield had taken the job with GALCIT only temporarily, but he went on to say, "It became a life-long career." He quickly proved his worth to Malina by presenting his detailed calculations on the relation of rocket thrust to chamber volume and nozzle dimensions. Of course, Malina was pursuing similar insights into the proper design of the exhaust nozzle on his own, in his thesis (1940).

Malina then put Summerfield to work on liquid propellant research full-time, and Summerfield set out to investigate the problems cropping up with unstable burning in the liquid propellant motor. This effort engaged him,

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[487] Hunley Interview with Summerfield (1994), p. 8.

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along with several other complexities of the combustion process, for much of his professional life. Once Malina had realized the connection between RFNA and aniline in the late winter of 1941-42, the possibilities of reducing unstable burning in the motor improved dramatically. [488] Having transformed Ray Stiff's suggestion to add aniline to gasoline (to boost its ignition potential) into a simple substitution of aniline for the gasoline, Malina and Summerfield had a fuel which they expected to perform much more reliably. But even with a hypergolic liquid propellant, there still remained sizable hurdles to overcome before a liquid fuel motor could be placed into production. [489]

Beyond the question of storability, which was never solved to the army's satisfaction, the problem of cooling the motor still lay on Summerfield's plate. Summerfield then started to pursue the design of regeneratively cooled motors on his own. He had heard about James Hart Wyld's invention from Frank Malina by 1940, but admitted in an interview with J. D. Hunley (1994), "I didn't have any access to the published literature about regenerative cooling. I didn't know about regenerative cooling." [490] Since Wyld's self-cooling motor had been public knowledge since 1938, the only explanation for Summerfield's inattention to this important breakthrough was his focus on gaining his doctorate in a field other than rocketry. Perhaps, from hindsight, his inexperience with rocket motors prior to 1940 served him well. He started

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[488] The formula for aniline is $C_6H_5NH_2$; the formula weight is 93.13 gm./mole.

[489] The liquid propellant motors went into production once Aerojet had procured contracts with the Army Air Force in 1942. Aerojet built these motors at its plant in Azusa, California.

[490] Hunley Interview with Summerfield (1994), p. 7. Summerfield commented that he never met Wyld personally, but that Malina had talked with the inventor of the American regeneratively cooled motor, and relayed information to Summerfield.

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from scratch, and like Parsons, began with what he knew; Summerfield tried different possible metals in cast nozzles until he found what worked.

The first liquid propellant motors for JATO rockets had cast steel nozzles. These were not regeneratively cooled. Summerfield and his assistants knew the heat capacity of steel and decided to use it to absorb the heat of the rocket exhaust. But this did not work too well. As Summerfield said, "The JATOs were developed as 1000 lb. thrust, 25 second duration [motors]. And by that time, they were cherry red. They were getting hot." [491] The nozzle throat of these first steel motors was one and three-quarters of an inch in diameter. 25 seconds of thrust could carry a plane up a good distance. But not at the expense of red-hot motors. These would prove much too dangerous, and not particularly durable. So the only way to proceed was to find a more heat-resistant substitute for the steel, or some way to cool the motors down during operation.

Summerfield turned again to copper, which, when milled pure, had a remarkably high heat capacity. [492] The next liquid propellant JATO motor was cast copper, with a 200 lb.-thrust capacity. [493] Copper could theoretically be cast as the base-metal for a self-cooling motor, with spiral channels constructed around the combustion chamber and nozzle. Summerfield and Howard Seifert, one of his classmates at Caltech and a GALCIT engineer after

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491 Hunley Interview with Summerfield (1994), p. 9.

492 Copper has a melting point of 1083° Centigrade.

493 Hunley Interview with Summerfield (1994), p. 9.

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1942, began to investigate this design in earnest that year, but soon found themselves stymied. [494]

There were two obstacles to be overcome. One was an engineering problem, the other, logistical. The engineering hurdle was straightforward but difficult to solve: in order to circulate fuel or oxidizer around the motor walls before injecting it into the chamber, the engineer had to design high-speed centrifugal pumps capable of withstanding extreme pressure, as well as mechanical and chemical stresses. [495] The GALCIT project began to develop such pumps in 1942 and within a year had produced a 10,000 rpm. aniline pump capable of jetting 20 gallons of fuel/minute into the combustion chamber. [496] However, since the team had decided on RFNA-aniline as the oxidizer and propellant of choice, designing a pump that could tolerate the corrosive qualities of red fuming nitric acid was quite difficult.

Technically the aniline could serve as a coolant, circulated in ducts around the nozzle before it entered the combustion chamber—so the difficulty appeared to be surmountable. Then, Seifert discovered another characteristic of heat transfer in the rocket nozzle that directly affected cooling. He realized

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[494] Seifert, who died in 1976, gained the Ph.D. at Caltech in 1938 and worked as a research physicist at the Westinghouse Corp. in Pittsburgh from 1940-42. At some point between 1940 and 1942, Summerfield wrote and told him of the need for more researchers at GALCIT, and Seifert then came to work alongside him. Seifert eventually became an expert in liquid propellant motors and co-authored the *Physics of Rockets* (1947), along with Mark Mills and Summerfield.

[495] Actually, the Russians decided to try an alternative to pumping the fuel and oxidizer into the combustion chamber—they used pressurized gas to force it in. This worked better with small rocket motors than with large. Goddard also employed this method, as did the Germans. See: I. I. Kulagin (1968). In Durant and James (1985), p. 97, and Neufeld (1995), pp. 34-35.

[496] Malina (1969), p. 182.

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that propellant pumped in ducts around a hot nozzle might not draw off sufficient heat before the liquid itself evaporated. Seifert discovered that "a marked scale effect exists for cooling. The rate of coolant flow increases linearly with motor thrust, while the motor area to be cooled increases less rapidly." [497]

In effect, then, the more powerful the motor, the faster the coolant will flow around the nozzle. But while the flow of coolant increases, the area of the nozzle cooled does not increase quite as quickly. The area to be cooled is a conical surface, rather than a flat surface where heat dissipation would proceed evenly. So a small motor will not cool as efficiently as a large one. [498] Since the liquid propellant JATO motor had a ceiling of 200 lb.-thrust initially (later increased to 500, then 1000 lb.-thrust capacity), there was no point in regeneratively cooling it. Seifert concluded that the JATO motor was just too small. Regenerative cooling at GALCIT would have to wait for the development of larger scale motors, those with a thrust on the order of 50,000 lbs. [499]

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[497] Frank J. Malina and Howard S. Seifert. 'The Liquid Propellant Rocket Motor-Present Status and Direction of Development.' *ORDCIT Project Memorandum No. 4-7. JPL-GALCIT.* August 28. 1945, p. 15. JPL Archives, Document # 3-146.

[498] In other words, a small motor's thrust would not propel the coolant-propellant around the chamber and nozzle fast enough to cool the entire nozzle adequately.

[499] Malina and Seifert (1945), p. 16. The technology has improved so that small, as well as large liquid fuel motors can now be regeneratively cooled. Seifert also learned from thermodynamic tests that even uncooled motors could operate for short periods of time (up to 75 seconds) despite the initial 4000° F temperature inside the combustion chamber. This is possible because "there is a large temperature drop across the boundary layer between the main gas stream and the motor walls." Thus, the motor does not melt, as its equilibrium temperature is never reached. See: Seifert, Mills, Summerfield. *The Physics of Rockets* (1947), p. 132.

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While Seifert and Summerfield were encountering a technical wall they could not penetrate in 1942, the project also faced pressure from its growing affiliation with the military. The GALCIT team had achieved enough success with the Ercoupe flights and solid-fuel JATO performance to diversify and organize its research more formally. Rather than simply recognize what was a *de facto* bifurcation of research—the somewhat informally conducted research of Parsons and Forman and the more established work of Malina, Summerfield and Tsien—the GALCIT rocket team formally planned in 1941 to divide into two smaller units; the purpose, to develop a 1000 lb.-thrust liquid fuel motor as quickly as possible. One group would continue to carry out studies of motor design; the other was assigned to design a liquid propellant JATO "suitable for installation on an aircraft." [500] The objective was to attract an Army Air Corps contract for the construction and installation of JATOs on larger airplanes.

By February 1942, Malina and Summerfield had solved the problem of motor throbbing enough to make construction of a larger liquid fuel motor feasible. Now, the team intended to expand its development and production, so as to contribute to the war effort. In September 1941, a 500 lb.-thrust motor went through its 'shakedown cruise,' permitting the crew to fine-tune some of the technicalities of construction. There was, however, "one explosion which should have been a warning to us." [501] The danger, again, was unstable ignition. Malina's insight into the new combination of RFNA and aniline

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[500] Malina (1969), p. 178.

[501] Malina (1969), p. 178.

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had come to light the following February; despite this important discovery, the problem remained not quite solved.

A self-imposed pressure to build new motors shrouded the team's work. War was only three months away in September 1941, and secrecy ruled at the laboratory and test site. The army, which had not yet placed a formal deadline on its GALCIT work, still expected results. If, by 1942, no liquid or solid propellant JATOs had passed flight tests, the Air Corps funds would run out, and the GALCIT rocketeers might be left without either a reliable JATO or a financial cushion. Fortunately, the May 1942 navy contract for 200 lb.-thrust JATO units helped relieve some of the pressure. But economic security was not yet around the corner, because the team still had not delivered a single production-line JATO. When Malina later mentioned (1969) that "the spring of 1942 was one of desperation for those concerned with development of a solid propellant JATO unit," he was not exaggerating. [502]

Financial considerations were not the only ones on the minds of Malina and von Kármán. Parsons may have been thinking of how he might benefit from larger Army Air Corps contracts, but Frank Malina was concerned, as well, with political and military events overseas. [503] Von Kármán was acutely aware of what Nazi domination of Europe meant to the Jewish people. Tsien,

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[502] Malina (1969), pp. 18-19.

[503] Malina's March 22, 1942 letter to his parents indicated his attitude. The Rocket Team had just successfully completed tests using Parsons' new GALCIT 53 propellant. Malina wrote home: " I have not written to you for some time mainly because of the pressure of work during the past hectic two weeks. One does not live through many similar periods, at least I hope not. The most important fact is that this week-end the outlook for the future is the brightest it has ever been. We now have something that really works and we should be able to help give the Fascists hell !" Malina, Letters Home, 22 March 1942, p. 42.

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too, knew that his work would contribute to the struggle against the Axis—he was always conscious of the Japanese conquest of his homeland and the painful reality that he was safe and sound, 5000 miles on the other side of the Pacific. These men, not merely technicians in the service of science, had developed a sense of mission about their work. Their drive transcended the thrill of pure discovery; political awareness had become, in time, a deeper component of their lives.

Late in 1941 the pace of work intensified, along with the frustrations. Although GALCIT had installed a test stand for liquid propellant motors in the *Arroyo* in February 1941, no JATO units were successfully fired on it until the spring of 1942. As usual, there were mishaps along the way. In addition to the destruction of a 500 lb.-thrust motor, other explosions wreaked havoc on the test site area in the *Arroyo*. In May 1941, a blast caused "considerable damage" to the test equipment and started a brush fire. The men were constantly worried, lest a large scale fire "race up the mountains toward the Mt. Wilson Observatory above us. A fire did once break out, but it was stopped with the help of all hands at the Project." [504]

The tests continued and eventually a motor design took shape. The exhaust nozzle was carved out of a copper block and used the same type of safety bolt attachment to the firing chamber installed in the solid propellant JATO. Pressurized nitrogen gas tanks forced the fuel and oxidizer into the combustion chamber through injectors with six separate tiny spray nozzles.

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504 Malina (1969), p. 177.

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A shielded spark plug ignited the mixture. [505] The motor was relatively small, weighing about 90 lbs., and had a rated thrust of 2000 lbs.

Malina, who was the director, Summerfield, Walter B. Powell, and Major Paul H. Dane, the test pilot, took charge of the testing phase at Muroc air base in March 1942. [506] Nearly twenty men, all told, supported the test preparations. Beverly Forman, Edward Forman's cousin, actually flew aboard the test plane, (a Douglas A-20 bomber weighing 18,000 lbs., fully loaded with JATO units) and fired the rockets. The first test took place at Muroc on April 15, 1942. Malina wrote in his memoirs, "During the flight tests the JATOs were fired 44 successive times without failure." [507]

The rockets performed spectacularly well. The JATO units, attached near the rear of the bomber's fuselage, decreased takeoff time and distance by approximately 30 %. In level flight at 5000 ft., firing the JATOs increased the plane's speed from 252 to 300 mph. The acceleration was even greater at 10,000 ft. The men reacted triumphantly. Von Kármán noted that the Muroc base tests heralded "the beginning of practical rocketry in the United States." [508]

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505 Malina (1969), pp. 176, 178. The use of gas pressure to force-feed oxidant and propellant into the combustion chamber indicates that GALCIT designers had independently come up with solutions similar to those of the Russians and Germans.

506 Paul Dane had been a former student of von Kármán's in the 1930s. This may have influenced his choice as test pilot.

507 Malina (1969), p. 189. Ironically, the first liquid propellant JATO motors had the same thrust (200 lbs.) as did the solid propellant motors under contract for the navy.

508 Koppes (1982), p. 14.

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Several Army Air Corps and navy personnel either worked at the test site or came as observers. There was even a representative of the Civil Aeronautics Administration, one L. A. Brady. Navy Lieutenant C. F. Fischer flew in from his base in San Diego on an April day to witness the testing. The project's momentum had definitely picked up. Fischer, who was at the August 1941 solid propellant JATO tests, intended to report back to his chief on the applicability of liquid propellant JATOs to carrier operations. He flew in to Muroc in a navy fighter plane, and once down, met von Kármán on the tarmac. He offered the scientist a chance to sit in the cockpit of his plane. This was a mistake. Malina tried to warn off Fischer, knowing "von Kármán's reputation for leaving experimental apparatus in a mess after visiting a laboratory." But it was too late.

> Von Kármán climbed into the cockpit, and Fischer explained the purpose of the various instruments while I [Malina] stood on the lower wing and watched. Von Kármán pointed at a handle by his foot and, as he asked what it was for, pulled the lever. I heard a crash behind me on the wing and scampered away from the airplane. When I looked back I saw a balloon under each tip of the upper wing slowly descend and inflate—von Kármán had released the water flotation gear. Fischer exclaimed: 'My Lord, how will I explain this at my base—flotation gear activated in a desert!' [509]

Working within the GALCIT Rocket Group could be dangerous, no doubt, but in its own way, possessed a certain charm, too. The near-fatal explosions, the fires, the risks posed by experimentation in an unknown realm, the occasional absent-mindedness, even impulsiveness, all of these were part of the team's story. From time to time, it seemed miraculous that the crew survived its own indiosyncracies.

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[509] Malina (1969), pp. 188-189.

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Funding from Caltech and the National Academy of Sciences for the GALCIT project totaled $115,500 in 1942. After the successful A-20 tests at Muroc Dry Lake in April of that year, Malina, von Kármán, and the team members anticipated that the Air Corps would place an order for the new JATOs, just as the navy had done following the tests at March Field. But this was not to be.

Malina was exultant, but the Air Corps was not convinced. In the intervening year between the start of experimentation (1941) and the JATO flight tests, the army had discovered that it no longer needed JATOs to boost takeoff speed. As Clayton Koppes noted in his study of JPL, "The Army Air Force...found that bulldozers could often carve out runways long enough to obviate the need for rocket-assisted takeoff." [510]

Thus, the market for liquid propellant JATOs just did not materialize as expected. The Battle of Midway (June 4-5, 1942) had changed the course of the war in the Pacific and encouraged American strategists to begin the long struggle to recapture out islands along the rim of the Japanese Empire. This meant that, with each new island taken, engineering battalions could build extended runways from which to launch bomber attacks closer to Japan. The need for JATOs diminished, especially as the logistics of shipping them to Air Corps fields in the war zone might prove costly and difficult. Liquid propellant JATOs, with their nitric acid oxidizer, were just too dangerous and awkward to use routinely. Special crews would undoubtedly have been required to load them.

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[510] Koppes (1982). p. 16.

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Even so, the navy contract held, for carrier runways are far shorter than island landing strips. Besides, GALCIT had promised the navy solid propellant JATOs, which now could be loaded with GALCIT- 53. The loss of the Army Air Corps contract was a setback—even if temporary—but enough hope remained to continue experimentation.

If more government research funding had not been forthcoming, the JATO project might have stalled by mid-1942. Caltech not only was unable to support it indefinitely, but had not committed itself to officially adopting the Rocket Team. The Institute's ambivalence reflected a five year history of bemused skepticism and outright mistrust of the rocketeers by Caltech faculty. Moreover, Caltech was simultaneously helping to sponsor Lauritsen's rocket project. But as usual, the resourcefulness of the GALCIT crew ferreted out a solution. Frank Malina realized that, if the government would not purchase JATOs from Caltech directly, then it might do so from a corporation.

On February 16, 1942, Malina wrote to his parents that, on a trip back from Washington, von Kármán and he "laid out the set up for a company. The next month will decide if we will or will not go through with it." [511] The corporation, founded on March 19, 1942, took the name of the Aerojet Engineering Company. [512] Aerojet was incorporated in Delaware, where corporate taxes were low. Theodore von Kármán became its first Director and

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511 Malina, Letters Home, 16 February 1942, p. 42.

512 Aerojet records in the Frank Malina Collection at the Library of Congress, inspected by this researcher, show that Malina's first thoughts for a company name included the choices "Air Superpower Corporation," and the "Airesearch Corporation." See Library of Congress, Malina Collection, Box 16.12.

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Frank Malina, its Treasurer. Total capitalization was $1200, $200 each from the six original investors. (These included von Kármán, Malina, Summerfield, Parsons, Forman, and Andrew Haley, a lawyer and friend to von Kármán.) The corporation "opened offices on east Colorado Street in Pasadena in a ground-floor room...formerly used by a fruit juice cannier, Henry Z. Gibbel, Vita Juice Dispenser." [513] The GALCIT Rocket Team had overcome too many obstacles to halt its work. Even initial government ambivalence would not slow the forward momentum gathered by the development of the JATO.

It cannot be denied, however, that 1940 through 1942 were tumultuous and decisive years for the GALCIT rocketeers. The co-existence at Caltech of another, much more lavishly funded rocket research project—operating in secrecy and thus isolated from GALCIT work—fit a pattern which had persisted throughout the Institute's history. During World War II, Caltech committed itself to national defense work through a formal liaison with civilian authorities in the National Defense Research Committee. Since Robert Millikan had performed similar work for the National Research Council and the War Industries Board during World War I, the Caltech chief executive felt quite comfortable following in the footsteps of his earlier experience. [514]

Meanwhile, GALCIT's rocket creations crawled forward, underfunded; the rocket group only gradually forged a direct link to the military. The von

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513 Von Kármán (1967), p. 258.

514 Goodstein (1991), pp. 73-74.

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Kármán-Arnold rapport was key here: both men were forward-looking independent thinkers who struggled against administrative resistance to change within their organizations. While von Kármán's belief in military research as key to the future of air power became stronger and stronger, the NAS/Army Air Corps grants of 1939 and 1940-41 created a simmering conflict in Frank Malina's mind. Ideals would henceforth be bent to the needs of war-related research and production. The army and navy were footing the bill and beginning to set priorities for design and construction of rockets.

Although Malina's discomfort was not shared by most other members of the Rocket Team—Parsons, Forman, even Tsien were capable of tolerating the shift from pure rocket research to military-sponsored work, provided they could continue their own thread of investigation—Frank Malina still served as a political barometer for the group. Malina squelched his nagging doubts about military secrecy, but the conflict simmered within him. In the 1940s, the United States was becoming a dominant global power. But the era of American supremacy—the 1950s—had not yet come, and Malina had not yet fully accepted the painful reality of his internal conflict. In the mid-1940s, he began to doubt that technology would have the glowing and positive effects on society which he had earlier assumed it might. He now knew viscerally that rockets could destroy as well as explore. But Malina could not act on his developing awareness, because his own future was linked to the military success of his engineering efforts.

Malina's school also evidenced a deep ambivalence about its wartime connections with the military. Caltech wished to benefit by an influential position as a research center, while at the same time maintaining its distance

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from the small group of rocket builders who were pulling the military right into the Institute's backyard. However, a new institutional shape began to mold itself around the old image of the university as research center. Throughout the war years, 1941-1945, and almost contrary to its own plan, Caltech would be in the vanguard of a transformation in outlook. The Institute had begun a permanent alliance with the military. Ironically, Millikan's school was quite unable to recognize the strange and powerful import of that alliance.

Even more significant was Parsons' breakthrough discovery of the composite propellant, GALCIT- 53. The notion of using common paving asphalt as a rocket fuel proved a somewhat startling development, and a quite fortuitous one. Parsons' invention gave added momentum to a project mired in theoretical and experimental difficulties. John Parsons and Ed Forman, the least likely candidates for inclusion in an academically-oriented engineering team, literally saved the team from possible dissolution. By the summer of 1942, GALCIT was poised to metamorphose once again—this time into a large scale producer of rocket motors.

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Chapter 11

The GALCIT Project in Action: Into the Mid-1940s....

The building of the Jet-Assisted Take-off Rocket (JATO), from concept to fruition, once merited little more than an asterisk in the history of rocketry. The design of large missiles, now capable of traveling thousands of miles in a short half-hour, landing pin-point fashion within yards of their target, overshadows the construction of so small and simple a rocket motor. Clearly, the JATO did not appear overnight—and so there is an historical thread between the development of this relatively uncomplicated rocket motor and the million-ton thrust booster that sent Saturn rockets off toward the moon in the 1960s and 1970s.

Technologically, the JATO motor and the Saturn V liquid propellant boosters that lofted Apollo astronauts into space cannot be compared. But the history of rocketry is not a story of spectacular discoveries, punctuated by long periods of inactivity and dearth of invention. The reality is that a slow, incremental process unfolded, and this process began with the patient, frequently frustrating explorations of men like Goddard, Korolev, Malina, von Kármán, Parsons, and Summerfield.

Slowly but surely the JATO became the step-child of a military, political, and business effort which transformed rocket technology in the war years, 1941-1945. The sounding rocket, the dream of Frank Malina, even of Robert Millikan when the GALCIT Rocket Research Project began in 1936, all but lost its place to the military rocket motor. The imprint of war and anticipation of

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conflict etched itself into the success story of the JATO. But the JATO was not simply a child of war; this small motor represented the culmination of five years of effort toward the solution of technical, theoretical, and chemical hurdles blocking its design and operation. Success breeds the expansion of an idea. By December 1942, Frank Malina and his tiny band of men had expanded their number. Now Aerojet, his new corporation, had a production team of more than 150. [515]

While this represented a striking reversal of fortune for the members of the GALCIT Rocket Team, most of whom were struggling financially, it was not a change entirely out of character for them. The memory of their collective poverty in the 1930s and early 1940s had never left them. Even Parsons, who lived in a large house on South Orange Grove Avenue (a rather lush street lined with several mansions) for much of the 1940s, could not be said to have been independently wealthy—his home was rather the center of activity for the Church of Thelema's rituals in Pasadena, which likely financed his existence there.

Conscious of the importance of their creation, and with six years of rocket work behind them, the GALCIT men needed no convincing about the future of rocketry. Now, in 1942, their chance had finally come. The confluence of events, particularly the successful experiments they had conducted, the insights into rocket motor performance discovered by Malina, Summerfield, and Mills, and the pressure of war-time demand for weapons, created a unique opportunity for this group of young Caltech scientists.

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515 Von Kármán (1967), p. 260.

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For the Rocket Team to capitalize on its success, relations with its parent, Caltech, had to be clarified. This turned out to be a more complex task than even von Kármán might have imagined. Malina, Aerojet's first Treasurer, had at best a lukewarm professional relationship with Robert Millikan's son, Clark. The latter eventually saw the financial opportunity presented by the formation of a corporation but disliked Malina's political stance; before Weld Arnold had put the GALCIT rocket group on a more secure financial footing back in 1937, the younger Millikan had openly scoffed at the notion of rocket development at Caltech.

Malina never quite forgot how Clark Millikan had derided his choice of rocketry as a thesis topic in 1936. In the mid-1940s, despite their professional collaboration—the younger Millikan was a professor of Aeronautics at Caltech—he and Malina developed an icy relationship, one which occasionally burst into open hostility. Millikan distrusted Malina's left-wing politics; Malina disliked Millikan's political conservatism, his hesitant acceptance of the rocket project, and his administrative meddling.

Some few years later, at a Jet Propulsion Laboratory board meeting on October 25, 1945, Malina became so angry at what he considered Millikan's interference in JPL's work that he blew up, slammed his papers down on the table, and threatened to resign his position as Acting Director, a position he had held since the fall of 1944. He quickly withdrew the threat, but the signs were clear. Over the years, Clark Millikan and Frank Malina had developed a deep animus toward each other, and their intense mutual dislike seemed to mirror the ill-defined relationship of Caltech to the Jet Propulsion Lab. By

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1946, Malina was to leave Caltech for a post at the United Nations Educational, Scientific, and Cultural Organization (UNESCO) in Geneva, Switzerland. [516]

Robert Millikan, who, by the mid-1940s was beginning to relinquish his grip on day-to-day activities at Caltech, still saw his Institute as the liaison for government contract work done by its professors. However, Millikan disliked the idea of Caltech as a permanent center of government-sponsored research. Wartime liaisons were one thing; the idea of a US military presence on the campus during peacetime, quite another. [517] However, government projects, especially in wartime, were lucrative for the Institute, and Millikan never lost a chance to place his school in an advantageous position.

Technical development contracts with the military meant prestige for Caltech, put its scientists both at the crux of a patriotic effort and at the seat of power. Von Kármán understood quite well how focused Robert Millikan was on funneling new research contracts and grants to Caltech. [518] He knew that Millikan was not only interested in the prestige and prowess of his school's theoreticians, but that he wanted research to turn into successful—and lucrative—applications. Von Kármán realized full-well that the elder

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[516] Malina (1971), pp. 339-341. See also: Chang (1995), p. 108.

[517] Goodstein (1991) astutely covers Millikan's last years as Caltech's Chief Executive. Her story of how Millikan was eased out of power by 1944 provides a sad, dramatic tale of the transfer of authority at a major university. See Goodstein, pp. 264-266.

[518] Von Kármán (1967), pp. 120-121. Von Kármán's comments on Millikan's occasional failures of perspicacity are noteworthy. For example, he mentioned in his 1967 autobiography how Millikan (and Caltech) "failed to realize in time that leadership in nuclear physics, unlike leadership in other fields of science, required a heavy investment in large machinery." p.178. Caltech did eventually build a high energy particle accelerator, a synchrotron, in 1957. See: Goodstein, pp. 272-273.

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Millikan prized him as the pre-eminent aerodynamicist of his day and would be loathe to let him leave Caltech. But von Kármán never let a new opportunity for scientific advancement slip by. He and Malina just did not see themselves as serving Caltech and Caltech only.

An academic's loyalty to his institution may be likened to a shifting sand dune. Caltech and its beautiful campus, its quiet, idyllic locale, proved a great attraction for many productive scientists. But academic work instills a certain resistance to bureaucratic function and university administration. After all, the school is a scholar's employer, and although the relationship provides, at least theoretically, ample space for free thought, there are competing demands put upon a college teacher's time and conscience.

Von Kármán had developed, from 1938 on, his own inimitable relationships with US government personnel, particularly with General Hap Arnold. He did not rely strictly upon the influence of Robert Millikan to boost his cultivation of government officialdom. He had an independent reputation as a scholar of world renown, and, gregarious as he was, preferred to develop his own ties to Washington. He did not ignore Caltech's administration; he consulted with Millikan on a regular basis and was intensely loyal to the Institute. But Caltech was not von Kármán's only conduit to power.

On April 8, 1941, a little less than a year before the founding of Aerojet, Malina wrote his parents in Brenham that his von Kármán had begun to work as a consultant for a local Southern California aircraft company. He added, almost as an afterthought, that "the school refused to hear of him resigning from the Institute." Malina even foresaw the possibility that he

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might take over some of von Kármán's administrative duties, and teach an additional course. "My salary would go up," he admitted. [519]

Von Kármán was no newcomer to academic administration. He had begun his administrative career during his days at Aachen, while professor of aeronautics at the *Technische Hochschule.* [520] By 1941, von Kármán had been GALCIT's Director for eleven years. He was simply tiring of his academic duties at Caltech and longed for fresh horizons. He had entrusted Frank Malina with numerous responsibilities as representative of GALCIT in Washington, first at the National Academy of Sciences and later at Annapolis, working with the navy's rocket expert, Robert Truax. Yet, Malina did not see himself so much as Caltech's loyal representative as von Kármán's, and as an ardent spokesman for the realization of rocketry's scientific and exploratory potential. [521]

When von Kármán and Malina created Aerojet, deep ethical and academic issues shadowed them. They were researchers and members of an academic community and had cultivated military contracts through Caltech. Now they were founders of a corporation whose goal was to sell rockets to the army and navy. General Benjamin Chidlaw, an aide to Hap Arnold, touched the pulse of one problem when he bluntly chided von Kármán in the summer of 1942,

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519 Malina, Letters Home, 8 April 1941, p. 36.

520 Von Kármán's tenure at Aachen lasted from 1912-1929, with the exception of the war years, 1914-1918, and his occasional trips overseas.

521 Robert Truax early became interested in rocketry and built several prototype liquid propellant motors while a midshipman at Annapolis in the late 1930s. During the war, he led a research team for the navy at the Annapolis Experiment Station, working alongside Robert Goddard, in the construction of liquid fuel rockets. He is known as a pioneer in the field. See: Ted Wilbur, 'Space and the United States Navy,' in *Naval Aviation News*, November 1970. Washington, DC, published by the Chief of Naval Operations.

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some few months after the March 19 incorporation papers established Aerojet,

> Dr. von Kármán, now wait a minute. Are you in here [Chidlaw's Pentagon office] to see me about a research and development matter, or as I said, are you another industrialist sitting across the desk who wants the contract? And he [von Kármán] said 'Yes, I think I must be called an Industrialist.' Well, I said, take off your homburg, and put on a derby hat, and the next time you come in here, I don't want to see you in a homburg. You put on a derby hat. First of all, I'd go home...and think long and deeply before I would try to head up an industrial organization. [522]

Chidlaw's strong opprobrium was a shock to von Kármán, something to which he was just not accustomed. He and Malina had flown hurriedly to Washington after the Army Air Force refused to renew its May 1942 JATO contract with the fledgling company. [523] The cancellation had perplexed von Kármán. He looked on the future of Aerojet with optimism, even though the company began life with minimal capitalization. Now, a prime contractor had suddenly interrupted the flow of business, and until von Kármán's confrontation with Chidlaw in Washington, no explanation had

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[522] Col. Murray Green, Interview with General Benjamin Chidlaw, Colorado Springs, Colo., December 12, 1969. US Air Force Historical Research Agency, Montgomery, Ala. IRIS # 1103068 1103083 microfilm reel #43819, Index 4, pp. 20-21. Interestingly, Von Kármán's recollection of this startling conversation differed from Chidlaw's. The professor remembered that Chidlaw told him, "We like you very much, Doctor. But only in cap and gown to advise us what to do in science. The derby hat of the businessman doesn't fit you." Von Kármán (1967), p. 259. At one point in his 1969 interview, Chidlaw remarked, "Oh, I forgot to mention, Dr. von Kármán was practically in tears, but I was pretty blunt with him because it was a great decision for him to have to make after spending all his life as an eminent scientist, then to become a kind of hard businessman."

[523] This contract, Aerojet's first *production* contract, called for the development of 60 liquid propellant JATO units, each generating 1000 lb. of thrust, and for the construction of droppable units, both for the Army Air Force. Each unit was to cost $3450, for a total of $207,000. This was a large contract for a company as young as Aerojet, in 1942. See: Tom Sprague: *The Achievement of Aerojet— The First Fifty Years.* Unpublished Monograph, 1991. Available through A&M Services, Inc. 949 Cataract Avenue, Unit 1, San Dimas, California 91773, p. 48.

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been forthcoming. Working with the Federal Government during the war might just prove more problematic than working for Caltech. For his part, Chidlaw doubted the business acumen of his professorial colleague. He preferred to go though more traditional channels.

In reality, the issue was both one of social perception and of practicality. Academics were not expected to cross the threshold into the world of business in the 1940s. The function of the academic was the creation of ideas and instruction of a new generation, not for his immediate profit, but for the benefit of humanity or the intellectual well of knowledge. Chidlaw may simply have been verbalizing his own particular bias. Hap Arnold's adjutant would not have canceled Aerojet's contract with the Army Air Corps on his own. Chidlaw's commanding general could easily have overruled him. Arnold apparently did not, despite his friendship with von Kármán.

Arnold wanted Caltech to be the site of future rocket research, and he did not doubt the GALCIT Team's ability to produce. In fact, ever since the mid-1930s, Arnold believed in the future of West Coast aeronautics. But his was not the only voice shaping the direction of Army Air Corps policy. There was already an army rocket testing center at Aberdeen, Maryland. And McCook Field (changed during the war to Wright Field) in Dayton, Ohio was a center for Army Air Corps aeronautical research.

Then, too, there were civilian pressures on the Air Corps. The National Advisory Committee for Aeronautics (NACA) had a large testing facility at Langley Field in Hampton, Virginia, and a second testing laboratory south of

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San Francisco, at Moffett Field, authorized by Congress in 1939. [524] NACA's director, George W. Lewis, believed the future of aeronautics lay in civilian control. He and von Kármán differed on this issue. Von Kármán had no qualms whatsoever about allying GALCIT with the Air Corps. Since there was a war on in 1942, nearly all research had become in some way intertwined with military ends. But that did not automatically give von Kármán an edge over Lewis.

The Army Air Corps could not cavalierly disregard George Lewis and NACA, although Hap Arnold did not particularly regard Lewis' attitude as progressive. In December 1946, the general wrote that "NACA's treatment of the 'problems of flight' might be compared to the use of a government subsidy solely for the technical development of the automobile without paying any attention to roads and other complementary facilities." [525] However, like Vannevar Bush, chief of the National Defense Research Committee, Lewis was a force to be reckoned with. Even von Kármán knew that.

Essentially, the issue lay beyond competing inter-service politics and personality differences of men in power; the problem, put simply, was that von Kármán's Rocket Team had not clearly and honestly defined its own status. To act as businessmen, procurers of army contracts and university researchers all at the same time meant that the team's chief client—the

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524 Anderson (1981), pp. 2-6.

525 Henry H. Arnold. 'Science and Air Power.' *Air Affairs*, December 1946, p. 193. Also quoted in Dik Daso's *Architects of American Air Supremacy: General Hap Arnold and Dr. Theodore von Kármán*. Maxwell Air Force Base, Montgomery, Alabama. Air University Press, 1997, p. 118.

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army—would not be dealing with just one contractor. The military might have to negotiate with both GALCIT and Aerojet. Since both the laboratory and Aerojet would be conducting research, while Aerojet concentrated on filling production orders, the issue of choosing and prioritizing contracts could become muddled.

Von Kármán certainly wanted new business for his company. But he could not afford to draw money away from GALCIT without discomfiting Robert Millikan. The balancing act, then, had to involve satisfying both Caltech and the armed forces. In the early 1940s the field of research and development for the army was still new and open to definition. Neither Caltech nor von Kármán had delineated the precise limits of the relationship between business, the scientific community and the military. The manner in which contracts would be negotiated was still very much an open issue.

Even more troubling than the issue of 'changing hats' was the ethical gray area which the GALCIT Team entered when it sought to turn a profit from its creations. Given the financial hardships which many inventors and academics face in the early days of their struggles to invent and create, the reality that their work has commercial value often re-shapes its design. This was one of the sources of Frank Malina's disquiet over his research for the Army Air Corps. It was also a particularly strong impetus toward further effort. Malina had misgivings about the direction of his work: he preferred to build rockets for peace, rather than for war. But at the same time, he did not mind earning a fair living from his inventions.

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Malina had first explored the possibility that GALCIT's rockets could be installed under the wings of commercial airliners, rather than fighter planes and bombers. But when his attempt to sell the JATO concept to Consolidated Aircraft failed to take hold in 1939, he could, to some degree at least, justify his work for the army and navy with the rationale that he was helping to defeat fascism. In fact, none of the GALCIT rocket crew doubted that their technical expertise was being put to good ends. There was a war to win, and GALCIT intended to do its part.

During wartime, the ethics of invention become part of a boiling kettle of hopes, decisions, fears, and pressures that obscure the intended consequences of invention, and yet, simultaneously, highlight the contradictory elements of working for the military. An invention's success often depends upon the support it derives from a powerful state apparatus. In fact, the technical refinement of invention actually accelerates when state funding supports it. But the ethical dimension of creation becomes distorted when war and a demanding state apparatus re-work the end result.

This is the second and potentially more damaging aspect of wartime contract work that academics undertake for the government. Frank Malina's original hope became lost, entangled in the mesh of war priorities and goals. No longer was he concentrating on purely engineering goals and dilemmas. By 1941 GALCIT rocketeers formed the nucleus of a war-production team. Just as was the case for Los Alamos physicists building the atomic bomb, the equation expressing the future survival and well-being of human society

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shifted toward the destructive side with the development of the military rocket.

However, JATO construction did not tip that equation dramatically away from peacetime research goals. The JATO contained a very small, simple rocket motor. In 1941 and early 1942, when Malina and Parsons' solid propellant rockets were produced in batches totaling only 100 units or less, armed forces contracts were both manageable and profitable. [526]

Malina had always questioned some aspects of the coupling of technology to the military, particularly the need for secrecy in his own work. He would always be a scientist at heart. He wanted to believe in the beauty and power of his work to advance the cause of science. But in the world war that had swallowed the future for the 1940s, there was no such thing as pure science. Three decades later, his position as independent artist more secure, Malina publicly voiced his hope that scientific ideas would someday flow freely throughout the world. In the first issue of *Leonardo*, the journal he founded in 1968, Malina wrote,

> There are overtones of secrecy in both the arts and the applied sciences for the protection, principally, of technical discoveries and improvements. However, in the sciences it is generally conceded that a 'secret' is good for a matter of months or, at most, a few years. The evils of 'secrecy' are especially apparent from the point of view of education....Needless re-discovery and repetition of errors can only be minimized by the free disclosure and exchange of information and experience gained by the practitioners of art and of science. [527]

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526 Aerojet's first contracts with the Army Air Force were strictly for *experimental* liquid propellant JATOs, and did not evolve into a full production contract until 1943.

527 Frank Malina. 'Aims and Scope of *Leonardo*,' *Leonardo*, Vol. 1, (1968), pp. 1-2. Collection of Mrs. Marjorie Malina.

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Malina's dislike of security restrictions matured into a belief that in the post-war world, there would be a need for international cooperation, promoting free exchange of technology and scientific ideas. In a 1968 interview with historian R. Cargill Hall, Malina expressed his concerns more freely than he felt he could in the 1940s:

> We live in a world of so-called sovereign states and this means there's great suspicion, there is a sort of competition between the countries. Sometimes perhaps competition is good. But when it's carried to the extremes that this world of ours is in now, it seems to me as very dangerous. [528]

By the 1960s, Malina had come to the conclusion that some form of international organization had to supersede the nation-state: "The concept of the sovereign state as it developed in the nineteenth century...is now reaching its limits of possibilities." [529]

Of course, in 1943 Malina understood and accepted the exigencies of war and the limits secrecy regulations placed upon him. In the early 1940s what really troubled him was the necessity for adjusting to an unwieldy bureaucratic apparatus such as the army's. He expressed distaste for the atmosphere of suspicion that pervaded war-related work. "We have to suspect spies under every piece of paper," he commented. [530] And in a letter to his parents, October 24, 1942, Malina wrote from Washington, "When one is here, one is

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528 Frank Malina, Interviewed by R. Cargill Hall, October 29, 1968. JPL Archives, Oral History Collection (filed alphabetically under Malina), p. 21.

529 Hall Interview with Malina (1968), p. 22.

530 Koppes (1982), p. 15.

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lucky if he gets six hours of sleep a night. During the day one goes from office and officer to office and officer and at night the main work is done." [531]

An entirely new management style requiring lengthy commutes between the capital city and his home base began to discomfit Malina. Von Kármán regularly made the long trip between the West Coast and Washington beginning in the late 1930s—now Malina was following in his path. He usually did not complain about it, but tried to use the air or rail time to catch up on his work. After 1942, the schedule of trips to Washington and back could be grueling.

Malina had spent almost his entire graduate career in Pasadena. For the most part von Kármán helped him skirt the inconvenience of bureaucratic inertia; Malina even remembered his first year at Caltech as one in which he stretched the rules, receiving a scholarship to which, as an aeronautical engineering student, he was not technically entitled. Von Kármán protected him from most of the administrative friction at Caltech which plagued the elder statesman of engineering. He had shielded Malina and his tiny crew from the probing curiosity and occasional wrath of other academics on campus—even when there was an explosion near the GALCIT building—as best he could.

Now that their invention was of strategic use to the United States government, an entirely new set of demands faced the GALCIT Rocket Team. Long seen as outcasts at Caltech, they were, by 1942, in the peculiar position of

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531 Malina, Letters Home, 24 October 1942, p. 47.

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being 'point-men' for Millikan's institute, of claiming a share of Caltech's academic and bureaucratic resources—and at the same time they were founders of a company. Both Malina and von Kármán knew that Millikan would object to their becoming businessmen, just as Chidlaw had, unless the venture retained some affiliation with Caltech. [532] The question came down to determining how members of the GALCIT Rocket Team might serve two masters: they were building JATOs for the armed forces (independently of their research and teaching responsibilities), while still working to secure GALCIT research contracts with the Army Air Corps. The situation could easily compromise GALCIT's director and the young Ph.D.s who owed so much to both the aeronautical laboratory and to the Institute as a whole.

As individual researchers and teachers, some within the Rocket Team had attained not only respectability, but a certain notoriety as well. In *The Thread of the Silkworm*, Iris Chang wrote that Tsien, in particular, was known at Caltech as a difficult, demanding teacher. His presentation of material, most of it mathematical formulae neatly written in tiny script on his class' blackboards, proved so difficult that a group of students formed a committee and protested to von Kármán. Tsien then told the GALCIT director, "I am not teaching kindergarten! This is graduate school!" Von Kármán eventually replaced him with a more sympathetic instructor. [533]

Tsien's difficult character was just another facet of the kaleidoscope of personalities which comprised GALCIT's rocket builders. Not all of them had

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[532] According to von Kármán, Robert Millikan once told William Zisch, an Aerojet accountant (later the company's comptroller), "Dr. von Kármán is an excellent scientist, but he is not a businessman, and his venture will end in disaster." Von Kármán (1967), p.261.

[533] See: Chang (1995), pp. 101-102.

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Tsien's reputation (or Parsons', for that matter.) Clark Millikan, in fact, was a far more engaging teacher, at least according to 'Amo' Smith, who studied advanced theoretical aeronautics with him. [534] But the team's new-found status derived, not merely from their iconoclastic style, but more from their scientific accomplishments and from their affiliation with the military. No longer simply struggling graduate students, Tsien and Malina (along with the others) were now engineers, and in Malina's case, an entrepreneur.

In 1939, the GALCIT Rocket Team had assumed quasi-official status when Malina dubbed the Army Air Corps JATO research, 'GALCIT Project No. 1.' [535] By 1942 Army Air Force and navy contracts became attractive goals for the group. [536] In November 1943, the Jet Propulsion Laboratory was conceived, on paper at least. Meanwhile, Aerojet had already been business for a year and a half and had signed twenty-seven contracts with the army and navy. [537] In fact, JPL did not even really exist, for the first mention of it came only in a memorandum written by von Kármán, Tsien, and Malina—'JPL Memorandum No. 1.' [538] While GALCIT was slowly metamorphosing into

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534 Interview with 'Amo' Smith (1996). See also: Bulletin of the California Institute of Technology, Vol. 51, No.1, Interim Catalogue, January-July 1942. Pasadena, January 1942, p. 231.

535 Von Kármán balked at this designation at first, telling Malina that GALCIT Project No. 1 reminded him of the Chinese nomenclature for a house of ill-repute, 'House No. 1.' See: Malina (1969), p. 158.

536 The Army Air Force was created officially in 1941. Between 1926 and 1941, it took the name, Army Air Corps. See: Daso (1997), p. 7, *nb.*

537 'Experience *Aerojet*—The Accomplishment.' Marketing Brochure (1948). Available through the Aerojet History Group; c/o Philip Umholtz, A&M Services, San Dimas Ca. 91773, pp. 57-60.

538 The report was a collaborative effort and combined two separate papers under one cover: Von Kármán's ' The Possibilities of Long-Range Rocket Projectiles,' and Tsien and Malina's 'A Review and Preliminary Analysis of Long- Range Rocket Projectiles.' Unpublished monograph,

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JPL, Aerojet's start-up and production demands continued to tug at the men's time and energy.

Ideally, since the goal was the delivery of JATOs to the army, GALCIT and Aerojet would each contribute its unique expertise to the project. In effect, Malina and von Kármán, though never quite articulating this hope, expected that the laboratory and Aerojet would work in tandem to construct rockets. Engineers from GALCIT could migrate to Aerojet and share the fruits of their research work with company engineers. The link between the laboratory and the company, however, was not intended to vitiate GALCIT. Rather, von Kármán and Malina envisioned the tie growing stronger.

The two engineers expected Aerojet to become both a rocket design and a production line plant. Since its only client was the US military, the company would then serve as one of the first government-funded research and development centers (as well as a factory.) Aerojet soon began to siphon off talent from GALCIT. The initial concept, that of a harmonious balance, with GALCIT conducting primary research and Aerojet handling production, was not what came to be. GALCIT engineers hoped to benefit from both, financially and professionally. But the problem for many GALCIT engineers, Malina and Tsien not the least among them, was that they had to wear two hats, just as Chidlaw had foreseen. This could and did prove a sticky situation.

Malina could not clearly decide whether he was working for GALCIT or Aerojet when he pushed forward on research. To whom was he expected to

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California Institute of Technology, November 20, 1943. JPL Archives, Document # 3-1203.

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be most loyal? The answer, of course was, both. As a scientist, his inclination was to concentrate on research. As an inventor and company founder, he had a definite interest in the success of his creations. It was a difficult task to satisfy—simultaneously—the demands of the two. 'Amo' Smith worked as an Aerojet production supervisor in 1943. In effect, he left his GALCIT connection behind. So, for Smith, the loyalty issue was relatively easy to solve. In his 1996 interview with this researcher, Smith complained that records at Aerojet were usually sloppily kept. Apparently, the company was riddled with administrative inefficiency in its early years. By the close of 1943, Smith had left for Douglas Aircraft, never to return either to GALCIT or Aerojet. [539]

Meanwhile, any civilian liaison to the military (in the form of a governmental authority like the National Advisory Committee on Aeronautics) found itself crowded out of the picture. At the August 1941 March Field tests of the first JATO units, and at Muroc Field, in the spring of 1942, NACA representatives had been present. There was, at this early stage of development, just a chance that JATO production might still have picked up civilian support, rather than become solely a military tool.

With the war heating up, the JATO developed exclusively as an assist for military aircraft. As war-time pressures mounted in 1942, the point was to produce JATOs as reliably and as quickly as possible. Since neither von Kármán nor Hap Arnold were impressed by NACA's sluggish acceptance of new research in aeronautics—most especially rocket development—the

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539 Interview with Smith (July 1996).

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melding of the GALCIT Rocket Team to the Army Air Force seemed a closer fit than did an alliance with the civilian branch.

The tightening alliance between Aerojet's founders and the military created a peculiar predicament for the GALCIT rocket group. Rapidly expanding into industry, the group was still administratively under Caltech's control. The team would eventually have to be recognized as a part of Caltech's organizational structure, or be cut loose completely. The university could not afford to ignore the rocketeers, for to do so might mean both lost funding and prestige. Yet, there was no formal way for Caltech to recognize the evolving status of the GALCIT crew in 1942. The tension inherent in the evolving relationship between the Institute and some of its least well-integrated academics served as a springboard to the formation of the Jet Propulsion Laboratory. In its own way, 1942 was a crucial year for the yet-to-be-created JPL.

Aerojet's birth actually accelerated the process of defining the status of the rocket crew with Caltech. But only in an indirect manner, as the company experienced a very rocky start. Oddly enough, the first official Aerojet contract was not for solid propellant JATOs at all, even though these were the only really marketable products the company possessed. Rather, in the spring of 1942, shortly after the Muroc liquid propellant JATO tests, the Naval Engineering Experiment Station at Annapolis put in an order for the development of a liquid propellant motor. The navy paid $20,500 for the experimental motor and expected the work completed in six months. [540] The Annapolis Naval Experiment Station, of course, was home to Robert Truax

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[540] Sprague, *The Achievement of Aerojet*, p. 48.

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and Ray Stiff, both Malina's colleagues in the creation of the aniline-RFNA hypergolic propellant. Von Kármán and Malina were perplexed at the army's initial hesitancy, but Aerojet took what contracts it could get. Von Kármán believed that GALCIT's creation would be of value to the military and should sell itself. However, since the army was in no way committed to long-term JATO use in 1942, this proved an incorrect assumption.

In the spring of 1942, it certainly looked as if Chidlaw's admonition to von Kármán to steer clear of business might bear the ring of truth. Even the navy preferred to work directly with the Guggenheim Laboratory when the question of JATO production first arose. After all, the laboratory had invented the JATO and GALCIT men would know best how to produce the rocket—without an intermediary.

But von Kármán and Malina refused to give up. The truth was that the laboratory was not a production facility—only the test stands and a small number of buildings in the *Arroyo Seco* served as GALCIT's experimental arena in 1942. Von Kármán knew that Hap Arnold had supported his rocket group's research since 1939. He also knew that Arnold was thinking, not merely of experimentation, but of mass production, for President Roosevelt had ordered a speed-up in military aircraft production in 1942 to 60,000 planes. [541] Long an advocate of air power as the strategic leading-edge of a modern military, Arnold—a superb organizer of men and material—was ready to support von Kármán, if the GALCIT group's inventions could

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[541] James MacGregor Burns. *Roosevelt, The Soldier of Freedom.* New York, Harcourt Brace Jovanovich, 1970, p. 190.

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somehow tie into the war effort. In 1942 Arnold hoped that the JATO would increase his bombers' range. [542]

Still, when the Army Air Force signed its first contract with Aerojet (for the development of a 60 lb. thrust experimental liquid propellant JATO) in May 1942, the agreement was strictly for research. Perhaps Chidlaw's skepticism had influenced his commanding officer. Such a short-term order seemed to indicate that Arnold was not fully convinced. However, the Army Air Force chief loved new gadgets, and the JATO certainly was one of these. [543]

Simply put, the JATO had not yet fully proven itself as a mass-produced auxiliary power source. More flight testing was necessary, and the Army Air Force just had not made up its mind what to do with the rockets. The truly remarkable fact was that, despite the reliability of solid propellant JATOs, the Army Air Force was more interested in refuelable liquid propellant motors. Yet, GALCIT engineers had been unable to perfect them, as the corrosive effects of nitric acid oxidizer just could not be overcome. The air force, even with a forward-looking pilot at its helm, still could not quite grapple with the reality of rocket-propelled aircraft.

Meanwhile, GALCIT did not exactly remain a passive spectator: the Navy Bureau of Aeronautics contract with the Guggenheim Laboratory for the production of 100 units of a 200 lb.-thrust solid propellant JATO came almost in tandem, even as the asphalt-base propellant, GALCIT- 53, was still under

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542 Daso (1997), pp. 66, 70.

543 Green Interview with Chidlaw (1969), p. 22.

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development.[544] The first JATOs produced in quantity for the armed forces (mid-1942) were not even Aerojet creations; they were built, after much testing and refinement, entirely under GALCIT auspices. [545]

By May 1942, two months after its incorporation, Aerojet finally procured its first production contract with the Army Air Force—for the assembly of sixty 1000 lb.-thrust liquid propellant units for the A-20 bomber. [546] While not a particularly large order, 60 liquid propellant units meant that the company could begin building rockets on an assembly line.

Despite this good fortune, administrative problems still dogged the firm. Contracts became very complicated. Several of them had up to three identifying numbers: an Aerojet number, an army or navy number, and a secret number used to confuse possible enemy agents. [547] Aerojet might wind up producing rocket motors of several different thrust ratings under one contract. GALCIT engineers worked alternately at Caltech and at Aerojet. Some of them (like Tsien) took temporary consultative positions at the company. [548] Aerojet's early contract history was seldom straightforward. Its

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544 Malina (1969), p. 170. See also: Parsons and Mills, JPL-GALCIT Progress Report 1 -1 (1942).

545 Parsons and Mills (1942), Pages 7-10 discuss the contract and research development specifically. The contract for these units was No. NXs 5879, covering May-June 1942, between GALCIT Project No. 1 and the Navy Bureau of Aeronautics.

546 This was Contract No. W535ac-31254, for 60 25AL-1000 nacelle-type liquid propellant JATOs " for experimental static testing and installation in the A-20 airplane." See: Aerojet Marketing Brochure (1948) 'Experience Aerojet: The Accomplishment.' Completed Contracts, p. 57. See also: *Aerojet-The Creative Company*. Los Angeles, Stuart F. Cooper, Co. 1997, p. III-5.

547 This information provided by Dr. John Bluth, JPL Archivist, December 16, 1997.

548 Chang (1995), pp. 97, 146.

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first years reflected a financial struggle to survive, an uncertain contract procurement outlook, and difficult, if not shoddy accounting.

In one sense at least, the Rocket Team had over-reached itself in the spring of 1942. The navy was impressed with the performance of JATO units, but the team could not guarantee the rocket motors' reliability. Malina knew that Parsons was working on the replacement for early double-base (GALCIT- 27 and GALCIT- 46) JATO units, but the point was to produce JATOs that could be shipped out and stored on the hardware shelf until needed. The project's success, both experimentally and financially, were temporarily, but clearly at risk.

The Aerojet venture had been designed initially to promote the GALCIT Rocket Team's research efforts, to secure profits for some of the Team's members, and to advance the cause of rocket development. Self-promotion and struggle under the shadow of ostracism by the academic community had steeled Malina, Parsons, and Tsien (ever looking to advance his career) to be resourceful. The Team had a phoenix-like quality: the near conflagration of experimental failure, banishment from campus—any adversity just pushed the men to try a new strategy. In 1939, Malina had expressed relief when hearing of the army's skepticism about the future of military rockets. He did not particularly relish the idea of creating military hardware. In 1941, with the JATO's success, Malina changed his tack. He now "shared the opinion of Parsons and Forman that after the efforts we had made during the previous five years we should participate in the exploitation of our ideas." [549]

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549 Malina (1969), p. 194.

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Parsons, ever hungry for more funds in the early 1940s, had again pushed the limits of Malina's horizons. Yet Malina and von Kármán, themselves no strangers to risk-taking, also simply decided that the GALCIT crew had paid its debt to poverty and should welcome the recognition and money available through government contracts. Complex individuals, they did not live either in the world of dreams or of pecuniary lust. An opportune historical moment had arrived and they could not miss the chance to turn it to their benefit. Von Kármán, Malina, just as was the case with Tsien, and later Summerfield, were ever-mindful of the need for challenge and new horizons in their work. They may have been fine scientists; they were also ambitious men.

The problem was that Malina had not thoroughly worked out his ambivalence about his new status as entrepreneur/scientist. Turning science to lucrative ends appealed as much to him as it did to Parsons; but his dream of creating a rocket for the love of it—and for the sake of scientific accomplishment—just would not let him be. Malina's gamble with Aerojet proved a very astute and successful one. But he was not at all happy with the ethical compromises he had decided to make.

Von Kármán and Malina now divided their time between GALCIT, where they both had administrative, teaching, or research responsibilities, and their new company, whose main hurdles were twofold: staying alive financially and developing the assembly-line technology capable of producing JATOs.

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Parsons' 1941-1942 invention of GALCIT- 53 paid off, both for GALCIT and for the fledgling company. Between 1942 and 1944, Aerojet produced more than 10,300 solid propellant units for the armed forces, most of them for navy carriers and PBY seaplanes. [550] In November 1942, Aerojet signed contract # X35AL-6000 with the navy for the development of liquid propellant JATOs for the PBY. In January and June 1943, the army signed contracts with Aerojet for research and development of the *Rotojet* and *Centrojet*, experimental rocket motors designed to rotate and pump fuel into the firing chamber by centrifugal force. In both devices, nozzles were arranged so that rocket motors or the entire combustion chamber could rotate, providing torque for cooling the motor jacket and pumping fuel. Neither motor went into production. [551]

By late 1942, both GALCIT and Aerojet were deeply immersed in the pursuit of military contracts for rocket development. The year before, GALCIT's history of research grants through the National Academy of Sciences had ended. Thus, the June 1941 contract with the Air Corps (the Air Technical Services Command, to be exact), came more than a year before the navy's contract for 200 lb.-thrust JATOs. [552]

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[550] *Aerojet-The Creative Company* (1997), Chapter IV 'Solid Rocket Activities,' p. IV-4 . A table listing JATO unit designations, quantity produced, and approximate production dates appears on this page.

[551] The contracts were : X35AL-6000 for the PB2Y3 JATO (number of units unknown), Contract # XCALR 2000A-1 (January 1943) for the *Aerotojet* (*Rotojet*) and a R&D contract for a 500 lb\ thrust Centrojet. Both *Aeroto jet* and *Centrojet* engines were to be RFNA-aniline and fulfuryl alcohol based. Major problems arose with the mechanics of the motor and the corrosive effects of using RFNA as an oxidizer. See: *Aerojet-The Creative Company* (1997), p. III-5 Figure III-2, 'Chronology of Early Liquid Rockets at Aerojet.'

[552] Miles (1946), Part 1, pp. 5-6, p. 87. This Letter of Intent never became a Definitive

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This crucial contract not only provided for testing of the Ercoupe JATOs; contract W535-ac-20260 (JPL-1) also allowed Aerojet to link its production of rocket motors to GALCIT research. This important formalization of the rapport between Caltech and a military contractor led to the development of the 1000 lb.-thrust liquid propellant motor. (Aerojet's first motor built for the new Army Air Force in 1942.) Some of the $115,500 also went to pay for an expansion of facilities in the *Arroyo Seco*, to the west of the original GALCIT test area. This entire test site became the nucleus of the Jet Propulsion Laboratory.

Aerojet, ironically, did not immediately benefit from the implementation of this secret contract. What actually happened was that Caltech, adhering to the new contract's terms, turned around within four months and used some of the money from JPL-1 to lease more land in the *Arroyo Seco*. The Institute had already signed a formal 3-year lease for about 4.2 acres in the *Arroyo* with the Pasadena Water Department in August 1940. [553] Money for this acquisition had come from the 1939-40 NAS grant. $10,000 was enough to instill confidence in the rocket project and justify an expansion of facilities in the canyon four miles west of Pasadena.

But along with the construction of additional test stands in the *Arroyo* came pressure to devote more time, more men to the rocket work.

Contract, although many supplements were added to it over the next 5 years. The contract finally terminated 30 June 1946; over these years, the Air Technical Services Command paid $2,154,264 to Caltech for GALCIT rocket research and the expansion of test and research facilities in the *Arroyo Seco*.

553 Miles (1946), Part 1, p. 4.

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As the group grew in number, with men like Summerfield, Mills, and Powell coming aboard in 1940, with more technicians signing on to the project each year, Aerojet could draw increasingly on the human resources GALCIT had been marshaling. [554] Aerojet at first depended completely upon GALCIT for talent and research data. GALCIT's contacts with the military benefited both the laboratory and Aerojet, because GALCIT could not mass-produce rockets over the long term and Aerojet had as yet no research facilities.

In 1942, Aerojet was a very small concern, with little plant space and a low capital budget. The entire company operated out of a small office at 3330 East Colorado Street in Pasadena, its first home, in March 1942. [555] It made sense to let GALCIT secure the research contracts, while the company became the conduit for production of models beyond the prototype. Before too long, however, as Aerojet's facilities increased in size, the new company was conducting research on its own, usually under military contract. In 1943 Aerojet purchased 86 acres of land in Azusa, California from the Day and Night Water Heater Company, for approximately $25 an acre, and an additional 46 acres from the Azusa Foot-Hill Citrus Company, for $200-$300 per acre. For about $11,950, Aerojet now had land to develop a plant and test facility, independently of the Caltech-army site in the *Arroyo Seco*. [556]

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554 Malina (1971), p. 339. 85 engineers had joined the GALCIT rocket group by mid-1943.

555 Confidential Memo of the Aerojet Engineering Corporation, March 30, 1942. Papers of Andrew Haley. National Air and Space Museum Archives, Suitland, Maryland, accession # XXXX-0200 (1998).

556 *Aerojet: The Creative Company* (1997), pp. I-18. I-21. See also: Haley papers, NASM Archives, accession # XXXX-0200. Haley Letter to William J. Hickey, Real Estate Division,

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Still, without a manufacturing plant land was of little use. Initially, General Frank Carroll, chief of research and development at the army's Wright Field in Dayton, Ohio, refused to approve Aerojet's application for funding of a new plant. He claimed that "government resources couldn't be wasted on a new organization." [557] Finally, after a meeting with Colonel Leslie Skinner, who had himself experimented with solid propellant motors in the 1930s, Carroll changed his tack. The Army Air Force authorized the government's Defense Plant Corporation to allocate $149,000 for the construction of Aerojet's first large-scale manufacturing facility early in 1943 and a pilot plant began operation in September of that year. The Azusa site finally began to fulfill its potential. [558]

Negotiations between the Army Air Force and the GALCIT-Aerojet team were usually attenuated and complex. The AAF wanted the liquid propellant JATO, not to boost aircraft into the sky, but (as Hap Arnold had hoped) as a rocket-assist to increase the speed and thus the range of its bombers in flight. But the process moved slowly. The Army Air Force usually waited before signaling its intent to purchase JATOs, pending additional tests and approval from its research and development center at Wright Field in Dayton, Ohio.

Defense Plant Corporation, March 14, 1943. The letter attests to an agreement Aerojet signed with the Azusa Foot-Hill Citrus Co. for an option to purchase land for a "reduced" sum of $10,000.

557 Von Kármán (1967), p. 260.

558 Von Kármán (1967), p. 260. See also: *Aerojet, the Creative Company* (1997) p. I-22. The Defense Plant Corporation, a branch of the Reconstruction Finance Corporation, was authorized to finance the construction of armaments and related plants during World War II. The RFC, in existence since January 1932, had been authorized by Congress in June 1940 to "finance the construction, expansion, and equipment of plants to be leased to contractors." T. Harry Williams, Richard N. Current, and Frank Freidel. *A History of the United States Since 1865., Second Edition*, New York, Alfred A. Knopf, 1965, p. 598.

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Within four months of the company's founding, forward momentum ceased. The Air Corps balked at signing future contracts with Aerojet. Lack of definition of the boundary between academic research and the business of producing rockets created a temporary snag—one which brought Malina and von Kármán much heartache. "The problem of 'hats' haunted us during the next years," Malina wrote in his 1969 memoirs. [559] With von Kármán at the company's helm, relations between Aerojet and the Air Corps were becoming increasingly strained.

Finally, on August 26, 1942, Andrew Haley (1904-1966), a lawyer and friend of von Kármán, became the second President of Aerojet. In effect, the company replaced its venerable scientist-administrator, transforming him from President to Chairman of the Board. Haley had known the Caltech aerodynamicist since 1941, when he had helped von Kármán's sister, Pipo, to obtain a permanent immigrant visa to the United States. [560] As von Kármán described the mechanics of Haley's elevation to the presidency of Aerojet, it would appear as if a *quid pro quo* were at work. Early in 1942, Haley was busy trying to convince the Federal Power Commission to grant an operating license for a hydroelectric plant in Iowa. The state of Iowa objected, and Haley had to prove that the project was safe. He called on von Kármán to work out some calculations; Malina, Tsien, and von Kármán stayed up all night corroborating the technical details of the dam's design. The next morning, von Kármán cabled Haley with the results, and the Power Commission

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559 Malina (1969), p. 195.

560 Stephen Doyle, 'Astronautics Loses an Original: Andrew G. Haley,' *Astronautics and Aeronautics*, November 1966, p. 61. Washington, NASA History Office, Lek 1/8/4.

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approved the application. In return, according to von Kármán, "Haley kept his end of the bargain by coming to California." [561] The Board of Directors then elevated von Kármán to the Chairman's post. Founding and running a corporation, which none of these academics had ever done before, had proven difficult for the professor. No new major contracts were coming in.

Having a lawyer at the helm of Aerojet also placated General Chidlaw of the Army Air Force. Since 1939, he had been a skeptic of rocketry's potential. His abrasiveness occasionally collided with the stubborn vision of his superior officer. In 1941, Arnold directed Chidlaw to begin top-secret research on jet engine aircraft. One engine, the plans for which had originated with England's innovative aeronautical engineer, Frank Whittle (1907-1996), was under construction in Lynn, Massachusetts, with collateral teams working at General Electric facilities, at Westinghouse, and another sponsored by NACA. [562] Arnold ordered Chidlaw to push the jet project forward, while protecting its secrecy. Chidlaw responded, "Good God, General Arnold, how do you keep the Empire State Building a secret?" Arnold simply said, "You keep it a secret." [563]

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561 Von Kármán (1967), p. 257.

562 Frank Whittle was a Commodore in the Royal Air Force and the inventor of one of the first turbojet engines (1932). He founded a company in 1936, Power Jets, Inc., to construct turbine powered jets. Plans for his engine were secretly imported to the United States in 1941. Col. Benjamin Chidlaw became the head of an AAF team designated by Hap Arnold to build America's first jet-powered fighter plane (using Whittle's design plans), the XP 59-A. The XP-59A had its maiden flight, October 2, 1942. See: Frank Whittle. 'The Birth of the Jet Engine,' *The Jet Age: Forty Years of Jet Aviation*, Eds., Walter J. Boyne and Donald S. Lopez. Washington, Smithsonian Institution Press, 1979, pp. 3-24.

563 Dik Daso. ' The Origins of Airpower: Hap Arnold's Command Years and Aviation Technology, 1936-1945.' in *The Air Power Journal*, Fall 1997 issue. http://www.cdsar.af.mil/apj/apj97/fal97/daso.doc. p. 4.

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For all his skepticism, Chidlaw was only echoing a prevalent attitude among Army Air Force staff—and Arnold proved no exception. This attitude decried heavy expenditures on futuristic weapons while the task at hand was to win the war. Arnold hesitated to support new projects that were not short-term. And yet, his contacts with von Kármán confirmed his own long-range vision. For Arnold, the future of war lay in air power. And the future of air power lay in new technologies. Arnold had to walk a tightrope between winning the war with existing technologies and promoting a new 'cutting edge' Air Force. Before mid-1944, when American forces captured the Mariana archipelago and built air bases from which B-29 Superfortresses could strike at Japan, the airborne JATO unit seemed a feasible way to increase a bomber's range. After 1944, the JATO would be unnecessary, at least as far as the Army Air Force was concerned. [564]

But in 1942, a successful island-hopping campaign in the Pacific had not begun. Since the navy and the Air Corps were both interested in purchasing JATOs, but could not foresee for how long they might need the rockets, rocket motor development for the armed forces proceeded only in fits and starts. By late that year, the JATO unit had become a reliable tool, but it was still only an appendage of main strike weapons, the fighter plane and bomber. Between May and December 1942, the only contracts Aerojet secured with the Army Air Force were small in scale, usually for spare parts and a few experimental units. Finally, at the end of that year, the company extended its first production contract (No. W535 ac-31254), producing both liquid and solid

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564 Daso. 'Origins of Air Power '(Fall 1997), p. 3. This researcher is indebted to Dr. Daso for his insightful comments on the complexities of the air force/von Kármán relationship.

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propellant JATOs. This change came just as Aerojet was negotiating to buy land in Azusa for a larger factory. More than 1400 units of both types came off the production line between February and October of 1943. 600 of these units were delivered after the Azusa plant began production. [565]

In 1942, von Kármán was a consultant for Jack Northrup's aviation company and was developing closer and closer ties to the Army Air Force in Washington, to which he was called away with ever increasing frequency. Summerfield became a vice-president at Aerojet. Parsons and Forman, whose ties to Caltech had been tenuous at best, also became vice-presidents of the company, and after 1943 began to spend more time there as explosives consultants. Always the independent one, Parsons drifted away from Aerojet by 1946. When, in the fall of 1950, the FBI was investigating his attempted removal of classified documents from the files of Hughes Aircraft, Parsons never admitted that he had pilfered secret information. After all, he reasoned, he had been privy to many secrets as an a rocket experimenter. There was no harm in availing himself of material he had helped to create. By 1951, the FBI investigation cleared him of any espionage charges. [566]

Hsueh-shen Tsien, however, never served as more than a consultant to Aerojet. He never became a stockholder, nor a member of the firm's directorship. Just before December 7, 1941, Tsien had finally obtained security

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565 See: Aerojet Marketing Brochure (1948) *Experience Aerojet: The Accomplishment*. Completed Contracts, p. 58. The contracts are all listed on this one page, from Contract No. W535ac-31254 to Contract no. NOa(s) 327.

566 FBI File # 65-59589, Subject: John Whiteside (Jack) Parsons. FBI Bureau File, November 2, 1950, p. 1. JPL Archives. This file may be found by referring to *Parsons' FBI file*. Parsons intended to use the documents as supporting materials for an application to the American Technion Society. Evidently, he was contemplating working in Israel as an explosives expert.

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clearance from the US Provost Marshal General's Office in Washington. He could then work on secret contracts, along with his friend and colleague, Malina. [567] If Tsien had become a participating member of Aerojet's Board, his ties to Caltech and southern California would have cemented further. As it was, Tsien left Caltech to work alongside von Kármán in the Army Air Force's Scientific Advisory Group, traveling first to Washington early in 1945, and then to Europe, where he helped interview Adolphe Busemann (creator of the swept-back wing design adopted by the US in the air force's B-47 production line bomber), before heading for MIT in August 1946.

Tsien was very concerned with remaining in the United States toward the end of World War II. In *Thread of the Silkworm*, Iris Chang wrote of a kind of "panic" that set in among Chinese graduate students at GALCIT after von Kármán agreed to seek a leave of absence from Caltech in October 1944, in order to work more closely on Air Corps matters. In a letter to von Kármán, they asked him to help them obtain jobs at other American universities. [568] Apparently, none of them wished to return to China, which was again descending into civil turmoil as the World War came to a close. There is no mention, either by Malina or von Kármán, of Tsien ever being offered stock in Aerojet. The reason could not have been that Tsien represented a security risk, for he had already obtained his clearance and was working on secret rocket projects through Aerojet, and at Caltech, by 1943. The mystery of

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[567] Chang (1995), p. 92. Colonel M. S. Battle, "chief if the personal security branch of the internal security division of the Provost Marshal General's Office," granted Tsien his security clearance on December 1, 1942.

[568] Chang (1995), p. 108.

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Tsien's absence from Aerojet's Board of Directors is only deepened by the Chinese scientist's silence. [569]

After 1943, the Army Air Force finally resolved the dilemma of simultaneously working with Aerojet and GALCIT by choosing to sign most of its future research contracts with the new and expanding Jet Propulsion Laboratory. JPL was just beginning to consolidate itself in late 1943 and the army was taking a great deal of interest in funding it. But this arrangement created obvious tension for Frank Malina. Malina undoubtedly envisioned JPL strictly as an arm of Caltech, for two years later he formally proposed that the Institute incorporate JPL into itself as a jet propulsion study branch.

Since GALCIT scientists and experimenters like Fritz Zwicky, 'Amo' Smith, Tsien, and Parsons were working alternately at either Aerojet or GALCIT, it looked as if GALCIT and Aerojet were actually competing with each other for armed forces contracts. In reality, they were working in tandem, since the owners of Aerojet were all GALCIT men. The question of loyalties and organizational responsibilities was a complicated one. Most Aerojet engineers, including Smith, Homer Joe Stewart (1915 -), and Tsien, veterans of GALCIT, saw their Aerojet tenure as only temporary. [570] They shifted their

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[569] It is possible that Tsien was simply not invited to join the Board of Directors at Aerojet. He was not a US citizen, and the directors may have had some doubts about the wisdom of his being a part-owner of an American company.

[570] Homer Joe Stewart received his doctorate from Caltech in 1940. He worked on the GALCIT/JPL project throughout 1940's. In 1958, Stewart was involved in the design of Explorer I, America's first earth satellite. His collaborative work on the aerodynamics of the WAC Corporal sounding rocket, along with Frank Malina, are the subject of extensive discussion in Chapter 13 of this thesis. See: *The Birth of NASA: the Diary of T. Keith Glennan.* J. D. Hunley, editor. NASA History Series, Washington, NASA History Office, 1993, p. 360.

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affiliation between the company, GALCIT, and other interests throughout the early 1940s.

The founding of Aerojet accelerated a process which would have sooner or later manifested itself within GALCIT: a redefinition of research and administrative roles. A major change was taking place. Rocketry was expanding to play a central role at Caltech's aeronautical laboratory. By the end of 1943, the new Jet Propulsion Laboratory began to siphon off additional administrative and teaching staff from Caltech. Robert Millikan had initially expressed skepticism about the GALCIT scientists' ability to, in Malina's words, "manage a commercial organization successfully." [571] Aerojet's record of achievement would dispel his doubts.

The years from 1939 to 1942 had changed the GALCIT Rocket Team. No longer simply scientists and rocketeers, they were now facilitators of advanced technological change, and could not remain isolated from the political pressures concomitant with their new status. By 1944, the men formed the nucleus of a research and production management group, committed to supplying rockets to the military and to earning a decent profit. In the midst of so much change, they still nursed their long-cherished dream of designing and building true high altitude rockets. The mixture, needless to say, did not settle well.

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[571] Malina (1969), p. 194.

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Chapter 12

The Military Link to GALCIT Tightens: The Creation of The Jet Propulsion Laboratory and its First Missile, The ***Private A***

The June 1941 Army Air Corps Letter of Intent for research and development of JATO units, (later known as contract JPL-1), was an important marking point for GALCIT; in addition to $115,000 in new funding, government oversight increased for the laboratory. Previously there had been navy and Air Corps observers at rocket motor test firings. With this contract, the army became directly involved in rocket research and production at GALCIT.

At the heart of this vortex of change, Caltech itself would not be left out of the action. The Institute had no intention of formally dissociating itself from Malina's group, and while the Rocket Team had established a corporation to market its inventions, the Caltech administration still expected to shape the executive board of the Jet Propulsion Laboratory. Caltech regarded the new facility as an arm of the Institute. In effect, the Institute's Executive Board wanted a key role in JPL's management.

Caltech was in a peculiar position, for army contracts had paid for all of the propulsion laboratory's buildings, not to mention the lease on *Arroyo Seco* land on which JPL originally sat. Caltech may have seen itself as JPL's administrator, but could would be hard pressed to call itself the laboratory's owner. Worse still for Caltech and the army, some of the land upon which JPL sat still belonged to the City of Pasadena. Needless to say, the issue of

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administrative governance over the new laboratory devoted to rocket research was a bit murky in 1943. [572]

Despite this complex arrangement, Caltech and the army both asserted authority over JPL. Neither had defined precisely how the laboratory would be run, except that the Jet Propulsion Research Project would conduct military rocket research during the war. The army expected operations at the laboratory to remain classified, but in fact, could not even decide which of its own branches—the AAF or Army Ordnance—would be JPL's primary contractor. Since June 1941 the Air Technical Support Command had supported initial financing and construction leading to the evolution of JPL. By late 1943, a dispute arose over exactly how to finance and build additional research facilities.

The Jet Propulsion Laboratory, whose name harkened back to the days when both engineers and the public casually regarded the term 'rocket propulsion' as too far-fetched to merit widespread acceptance, would eventually occupy much of the north end of the *Arroyo Seco*. By the late 1950s, JPL would become the premier research center for exploratory probes and satellite development for the United States. But, early in 1944, JPL was experiencing a difficult and uncertain start.

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572 Miles (1946), p. 19. By 1941, the Jet Propulsion Research Project had leased approximately 8 acres of land in the *Arroyo* from the Flint Ridge Realty Co. of Pasadena in 1940 and 1941. When project facilities began to expand in 1943, Caltech asked the realty company to lease it an additional 15 acres, but Flint Ridge would consider only a sale. Thus, of more than 65 acres occupied by JPL in 1945, only about 8 acres were still leased from Pasadena. Of course, Army Air Corps money had paid for this land. pp. 6-7.

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The Army Air Force wanted the Defense Plant Corporation, which was coordinating wartime purchase and leasing of land for defense contractors, to finance the the Jet Propulsion Research Project's expansion. Army Ordnance, beginning to get seriously interested in a rocket project at the new laboratory in 1943, wanted the corps of engineers to clear more *Arroyo* land, and construct more Quonset huts and tests stands. The Corps of Engineers, discovering the Army Air Force's resistance to construction by army men, suspended its contract and awaited developments, expecting to leave construction up to the DPC. Negotiations dragged on for three months in 1944 (from July to September), causing consternation among GALCIT researchers. Caltech's assistant comptroller, W. R. Stott, neatly summarized the Caltech administration's impatience in late August:

> It has now been some six weeks since negotiations were started with the Defense Plant Corporation. During this period, we have been unable to do any work on these facilities because of legal restrictions imposed upon the Los Angeles office of the Defense Plant Corporation by law; it does not seem to us as though we can complete contractual arrangements with this agency in less than six weeks, if at all. [573]

Eventually, in September, the Corps of Engineers was called back to continue its work. However, this happened only when Army Ordnance had begun contracting work with JPL for the development of a long-range missile. [574]

One of the site problems with the *Arroyo Seco* is water drainage. The canyon tends to drain poorly in winter, when short heavy rainfalls hit, and water frequently flowed from its hillsides toward GALCIT buildings. The question

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573 Miles (1946), p. 11.

574 Miles (1946), pp. 12-13.

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of whether or not the Corps of Engineers would level and construct on the site may well have been an important one. The Defense Plant Corporation, after all, was not directly involved in the business of land clearance and flood control.

But there was another deeper issue behind the inter-service struggle for contract management at the Jet Propulsion Research Project. The Army Air Force was simply unsure of how far it wanted to go in rocketry. Its first three contracts with the GALCIT facility were for JATOs, a hydrobomb (an underwater rocket), and ramjet propulsion research. Although these contracts (JPL-1 through JPL- 3) extended into late 1946, only the call to develop a 1000 lb.-thrust motor demanded a new scale of rocket work. In early 1944, however, Army Ordnance became very interested in long range rockets. In 1946, just two years later, total AAF contract expenditures at JPL amounted to $4,207,514. Army Ordnance contracts came to $7,294,253—$3,086,739 more. [575] In addition, Ordnance's contracts were for true missile development.

The Army Air Force was still spending a considerable amount of money at JPL, but supporting fewer visionary projects. The long range rocket might change the nature of warfare dramatically. The JATO, the hydrobomb, and the ramjet just would not. The dispute between the Army Air Force and Army Ordnance over construction at JPL proved a harbinger of future rivalry between the army and air force over ballistic missile development in the

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[575] Miles (1946), pp. 87-91.

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1950s and 1960s. In fact, the entire question of defining administrative control at JPL hung like a thunder cloud over its early years.

Certainly, the ultimate cause for this poorly defined situation was the war itself. In 1942 and 1943, no one expected the union of academic research, industrial war production and military procurement to outlast the conflict. The lease on *Arroyo Seco* land was due to expire at the war's end. [576] None of the players in this drama realized that, in groping for temporary solutions, they had in fact created new policy initiatives which would alter the permanent status of relations between academia and the military. The rocket was no ordinary weapon. The exigencies of its production would re-define the essential limits of the university's independence from the nation-state.

While the Manhattan Project could sculpt the dynamics of the atomic bomb in nearly absolute secrecy in the New Mexico desert, its scientists toiling in the lonely splendor of their theoretical sub-atomic realm, the government had to ally itself to a research institute, and one with a proven record in aeronautics and rocketry, in order to build war rockets. Since this was the case, and since the military could not completely isolate Caltech's contract engineers, administrative and ethical conflicts sometimes flared up, usually in an unpredictable way. Caltech, the army, GALCIT's engineers—all were in uncharted territory in 1942 and 1943. And since the first Chairman of the JPL Executive Board by 1945 was Clark Millikan, and its first Acting Director was Frank Malina, personal friction was inevitable. [577]

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[576] Miles (1946), p. 17.

[577] The Caltech Executive Council confirmed the appointment of Malina as Acting Director of JPL on January 3, 1945. See: Miles (1946), p. 14, *nb*.

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Caltech wanted its link to JPL solidified, not weakened. But the Institute could not—and undoubtedly would not—wrest control of funding from the army. The armed forces, sole contractor with the Jet Propulsion Research Project since 1940, were the *de facto* owners of its successor, the Jet Propulsion Laboratory. Caltech, in effect, was relegated to a caretaker's role. JPL may have been born in the light of intense scientific research, but was the child of war. Because there were few American engineers qualified to teach rocketry, even fewer prepared to build rocket motors, the problematic administrative and moral questions were often buried.

To Frank Malina's credit, he did not easily accept the blurred re-definition of research that the Caltech-Army Air Force link stamped upon the GALCIT rocket project. He was uncomfortable with it, and this may have been the source of his increasing irritation with Caltech's attempts, through Clark Millikan, to assert its authority over JPL. But resolving the dilemma created by his triple affiliation with GALCIT, JPL and Aerojet was not to be so easy for Frank Malina. Simply living with his conscience as his guide was no easy task in the war years, as it had been in 1936.

Each of the members of the GALCIT Rocket Research Team had by 1942 become actors in a portentous drama: the intervention by a vigorous and proud government in shaping the direction of their work. Individually, the men were responsible for the decisions they made to continue their research, knowing that it was destined for military uses. Collectively, they became part of an emerging system that interlocked university research and military end-purposes.

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The tight web these men wove around themselves, the web of military-related research, would soon entrap them. Parsons had been and remained, until his life's close, skeptical of authority. Malina—in 1943, a business man who longed to return completely to the world of hard science—found himself an administrator, shuttling back and forth across the continent. Summerfield and Tsien were both dedicated young scientists, fascinated by and committed to completing the engineering puzzle of the rocket motor. Like Malina, they had consciously rejected or ignored careers in aviation technology, careers which would have given them greater financial and personal stability in the late 1930s and early 1940s. They had chosen to work in the arid and dusty *Arroyo Seco*, in the open, or in Quonset huts and block buildings of simple design, with few amenities. Yet, in accepting the burden of success, long sought after, Malina and his crew also entered a larger, more complex world, one in which the pressure of realized dreams blurred the ethical boundaries of their work.

Since the future of rocket research, for all its promise, remained uncertain in 1943, the real struggle was to convince military planners to think long-term, rather than simply to plan for termination of hostilities against the Axis Powers. That very year two liaison officers for the Army Air Force Material Command and Army Ordnance, Colonel W. H. Joiner and Captain Robert B. Staver, pinpointed the problem quite well. Malina noted in his 1971 memoir that both these men had expressed concern that "the historic tendency of the government to drop potentially important research for military purposes when a war ended" was beginning to reassert itself, even before the Second

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World War drew to a close. [578] If Army Ordnance had not stepped in with its own contract (JPL-4) by mid-1944, JPL might not have built any long-range rockets during the war. Frank Malina's cherished sounding rocket could have remained just what it began as—a dream.

In 1944, Army Ordnance took up the standard and began to trumpet the rocket's true potential. By late that year, the 'Ordnance Project, California Institute of Technology (ORDCIT),' the progenitor of JPL, had tested and produced a solid propellant rocket capable of firing more than eleven miles down range with a payload of 60 lbs. This was the Private A, the first of a series of ordnance missiles GALCIT and JPL were to design for Army Ordnance beginning in 1944.

The story of the Private A is rich with the complexities of intra-service rivalry, the adventures of a middle-aged aerodynamics professor (von Kármán was 62 in 1943), who was becoming more and more devoted to the future of aeronautics—not at Caltech, but through his consultant work for aircraft firms and his friendship with Hap Arnold—and happenstance.

By 1943, Hap Arnold had thoroughly cemented his ties to GALCIT's chief, keeping von Kármán in Washington on an almost permanent basis (von Kármán had actually installed himself in a hotel in the capital). The Army Air Force commandant was formulating his plans for the next stage of air power development, and he needed von Kármán's advice, especially as the flights of the XP-59 experimental turbojet fighter had shown what paths the revolution in aeronautical technology might take. But as usual, Arnold had

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578 Malina (1971), p. 344.

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to balance his plan for future projects with the policy of winning the war through massive air power. Rockets were just not the weapons in 1943 that they would be in 1962, when Minuteman ICBMs could hurl a warhead halfway around the earth in 30 minutes..

Arnold wanted von Kármán's expertise and vision allied to the young and increasingly powerful Army Air Force. He wanted a blueprint for future development of the air force, even before the war ended. Arnold believed that air power would play a dominant role in future wars and had to build a case for support of rocket research. This is undoubtedly why he asked von Kármán, in September 1944, to chair a committee of scientists who would devise a plan for air research for the coming years. This became known as the Scientific Advisory Group, and included Tsien, Hugh Dryden (1898-1965) an aeronautical engineer then working for the National Bureau of Standards and later Deputy Director of NASA (1958-1965), and Major T. F. Walkowicz of the Army Air Force, among others. [579]

At the same time, Arnold knew that the Aberdeen Proving Ground in Maryland remained a key center of rocket development for the AAF. Close to Washington and the coast, and situated near Annapolis, where Robert Truax and Robert Goddard conducted their own rocket tests for the navy, Aberdeen seemed a logical choice for continued rocket experimentation. Out on the West Coast, the Jet Propulsion Project was producing JATOs. Research was progressing along several innovative lines at Caltech, but in 1943 and 1944,

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579 Von Kármán (1967).pp. 268-269. See also: Michael H. Gorn. *Hugh Dryden's Career in Aviation and Space.* Monographs in Aerospace History, No. 5. Washington, DC, NASA History Office, 1996, pp. 6-7.

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the concept of the rocket as an offensive weapon had not yet fully permeated the thought of policy makers on the AAF general staff.

Still, the irony is that the idea of a tactical rocket built in California originated with the Army Air Force. Colonel W. H. Joiner, the AAF liaison to GALCIT, had actually suggested that Tsien and Malina investigate (probably in the fall of 1943) the feasibility of a long range rocket—he thus prompted the very study that launched the Jet Propulsion Laboratory. Joiner had sufficient faith in the possibility of building an American tactical rocket to urge the two GALCIT engineers to prepare a design and performance analysis for "rocket projectiles" with a range in excess of 100 miles. Of course, Joiner was not the only far-sighted government official to advocate further rocket development in 1943. Both the National Advisory Committee on Aeronautics and the National Research Council—distinct from the NDRC—were well aware of current research. In December, NACA had even done a preliminary analysis of rocket fuels for the Army Air Force's Air Material Command at Wright Field at its Aircraft Engine Research Laboratory at Cleveland, Ohio. [580]

However, it looked as if the NACA report was a bit late—for in mid-summer, Army Ordnance had sent von Kármán fuzzy photographs of what appeared to be German rocket launching sites along the French west coast. (The sites

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[580] National Advisory Committee for Aeronautics, 'Preliminary Analysis for the Army Air Forces, Material Command: Fuel-Oxidant Combinations for Use in Rocket Motors, December 2, 1943.' Malina Collection, Library of Congress, Box 13, folder 13.14. The National Research Council, separate from the NDRC, was an outgrowth of World War I organization by university scientists and administrators to direct science toward the national interest. Its first head was astronomer George Ellery Hale. The National Research Council had written to Colonel Gervais Trichel (whose name the author, the famed aerodynamicist William T. Durand, had misspelled as 'Prichel') forwarding the report both to Army Ordnance and to the Air Material Command at Dayton, Ohio. Letter of December 29, 1943, Library of Congress, Malina Collection, Box 13, folder 13.14.

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turned out to be V-1 buzz bomb launch tracks.) The army was alarmed and wanted von Kármán to interpret them. [581] On August 2, GALCIT's director sent a four page single-spaced reply to the AAF Material Center at Wright Field in Dayton, commenting, not only on the photos, but on follow-up reports supplied by British Intelligence. Yes, the Germans were investigating liquid and solid propellant rockets; in fact, they had already built a "large projectile of unusually large weight and dimensions." Von Kármán noted that "its {the projectile's] weight is mentioned at being between 60 and 100 tons and its range 150 to 310 miles." [582]

This was startling news. While the US Army had been testing small rockets as take-off assists, the Germans were building guided missiles which could strike targets hundreds of miles away. If secrecy had not been the norm for all war-related work in the United States in 1943, von Kármán's report might have set off a firestorm of political protest. Indeed, the news galvanized NACA, the National Research Council, the military, and GALCIT into action.

Since Frank Malina and the GALCIT team had already been conducting rocket research continually for more than five years (since 1938), the Caltech group had a head start. Von Kármán asked Malina and Tsien, his best theoreticians, to write a proposal which could then be circulated among army circles—undoubtedly, Army Ordnance, the AAF's Air Material Command and the Air Technical Services—with a copy forwarded to Hap Arnold—demonstrating

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581 Von Kármán (1967), p. 264.

582 Von Kármán, 'Letter of Transmittal to Commanding General, Air Material Center, Engineering Division, Wright Field, Dayton, Ohio: Evaluation of British Intelligence Reports, August 2, 1943.' The letter is four pages long. Library of Congress, Malina Collection, Box 13, folder 13.14, Section II. p. 1.

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the possibilities for long-range rocket research in the United States, and not incidentally mentioning the Air Corps Jet Propulsion Research Project and Aerojet. [583]

Von Kármán wrote a four-page preface, entitled 'A Memorandum on the Possibilities of Long-Range Rocket Projectiles' and attached it to the Malina/Tsien report, 'A Review and Preliminary Analysis of long-range Rocket Projectiles.' The combined report appeared on November 20, 1943. Malina had at least twenty five copies run off, and at the top he wrote "Jet Propulsion Laboratory." JPL had arrived, at least on a cover-sheet. [584]

The next step was Colonel Joiner's. He soon gave von Kármán's paper to his to his friend and counterpart, Army Ordnance Captain Robert Staver. Staver picked up where Joiner had left off. The report found its way to the desk of Staver's boss, Col. Gervais Trichel, then chief of the Rocket Development Branch at Ordnance. Undoubtedly, Joiner had also transferred a copy of the report to the AAF Air Material Command, as this was his unit. Remarkably, the Army Air Force begged off. In fact, in Malina's words, "the Air Forces did not respond to our proposal." [585] Both Joiner and Staver had realized the import of the JPL Memorandum on Long-Range Rocket Projectiles, but only

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583 By 1942, Hap Arnold had become commanding general of the Army Air Force. It was clearly appropriate for von Kármán to send a copy of the proposal to him.

584 Von Kármán, 'Memorandum on the Possibilities of Long-Range Rocket Projectiles', and H. S. Tsien and F. J. Malina, 'A Review and Preliminary Analysis of Long-Range Rocket Projectiles,' California Institute of Technology, November 20, 1943. JPL Archives, Document No. 3-1203. Von Kármán's references to the Jet Propulsion Research Project and Aerojet appear on p. 2. The report appears first under von Kármán's name, in order to lend prestige to its appearance.

585 Malina (1971), p. 344.

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Stavers' superiors in Army Ordnance acted upon the report. Why did the Army Air Force back off, to the amazement of both Malina and von Kármán? A hint may be found in Malina's own comments on the crisis:

> The response of the military services to the two documents on the development of long-range missiles planted a seed for the bitter inter-service military rivalry that took place in the 1950s....Those concerned with Army Ordnance said that, since long-range guided missiles followed a ballistic trajectory like a gun projectile, such missiles were clearly the responsibility of the Ordnance Department. Those responsible for long-range aircraft bombers said that, since a long-range guided missile needed aerodynamic control during the first phase of flight in the atmosphere, the Air Forces should be responsible for their development, and they called the missiles 'pilotless aircraft.' [586]

What appeared to be a mere semantic distinction, in reality, revealed deep doubts among AAF officers about the potential of the rocket. The major problem was guidance. Since Army Air Force planners had defined the long-range rocket as a guided missile, as had Ordnance, they knew full-well that there was no suitable guidance system for such a rocket available in 1943—not in the United States, at least. Such a key component could not be left out of long-range missile development. (In fact, it is the component that in effect differentiates a rocket from a missile.) This is the likely reason why the Army Air Force stepped out of the picture. No guidance system meant "a pilotless aircraft" that could not be controlled. Evidently, the AAF was simply accustomed to airplanes that *could* be controlled.

So, in a step that did not seem as momentous in 1943 as it was to become for the armed services in the 1950s, the Army Air Force transferred momentum

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586 Malina (1971), p. 344.

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on the Caltech long-range rocket project to another branch of the service. Malina probably would have been more satisfied, had the AAF wholeheartedly embraced the new technology. After all, his boss was close to the Army Air Force chief, and the Jet Propulsion Project had been working with the Air Corps since 1939. [587]

Army Ordnance was quite eager to pick up where the AAF left off. By mid-1944, Ordnance had assigned Colonel Skinner as liaison officer at Caltech. [588] The von Kármán report, with Tsien and Malina's detailed analysis attached, had catalyzed a major change in weapons development strategy for the American military. Known as 'JPL Memorandum No.1,' the paper called for "the design of a 2000 lb. rocket projectile propelled by a liquid propellant jet unit....This projectile would carry an explosive load of 200 lbs. for approximately 12 miles." [589]

But locking in the GALCIT/Army Ordnance relationship was only the first step. On January 15, 1944, two months after the issuance of 'JPL Memorandum No. 1,' Colonel Trichel wrote to von Kármán, expressing "not only interest in the proposed program, but a desire that Caltech undertake a

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587 According to Malina, von Kármán had to officially request AAF release from the long range missile project. even after a contract had been signed with Army Ordnance in January 1944. Von Kármán wrote to Major General Frank O. Carroll, chief of the engineering division, AAF Material Command, in Dayton, Ohio, asking about continued AAF interest in "pilotless aircraft traveling at transonic speed." On February 17, 1944, the AAF Material Command "cleared the Ordnance project to proceed at GALCIT, so far as the AAF was concerned." Malina (1971), p. 345.

588 Miles (1946) Chapter VII, p. 92. Skinner's chief in Army Ordnance was General Gladion Barnes.

589 Von Kármán (1943), p. 2.

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more intensive program than originally outlined." [590] Trichel envisioned a contract with Caltech on a "cost-plus-a fixed-fee basis," not to exceed $3,000,000 for the first year.

Naturally, the value of this new contract with Caltech (and with Aerojet, because Aerojet would be responsible for actually producing the power plant for the missile) shocked Malina. The shock did not register only because the rocket team had come so far from its early days of struggle. When Malina wrote in 1971 that "Trichel's letter threw us into a proper dither!" he never really explained the import of his comment. [591] Directly afterwards, his memoirs described how the GALCIT Rocket Team mobilized to revise their proposal and submit it to Robert Millikan for approval. Actually, Malina was concerned over how GALCIT could possibly administer such a large project. However, confirmation of the contract seemed to galvanize him into action.

And so, JPL-4, the first Army Ordnance contract with the new Jet Propulsion Laboratory (contract no. W-04-200-ORD-455), took effect on June 29, 1944, running through June 30, 1946. The value of the contract, including a subcontract w[illegible]he Sperry Gyroscope Corporation of Hyde Park, New York, for development of an internal guidance system for future missiles, came to $3,283,000. [592] Beyond generating some 42 reports and memoranda on rocket propulsion, JPL-4 also produced America's first extended range rockets,

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590 Malina (1971), p. 344.

591 Malina (1971), p. 345.

592 Miles (1946). Part 1, pp. 89-90. There were additional subcontracts tacked on in 1945, including one for collaboration with Bell Telephone for the development of the NIKE anti-aircraft missile, as well as with Douglas Aircraft and RCA, for component parts.

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Private A and the WAC Corporal. [593] The fledgling JPL was ready to accept the challenge. The Jet Propulsion Research Project, so named since 1941, already had contracts in force with the Army Air Force, JPL-1, JPL-2, and JPL-3. [594]

Even before the November memorandum had appeared in print, in May 1943, what once had been GALCIT Rocket Research Project began to prepare for its expansion, unveiling a new organizational structure, with several interlocking layers of research, design, and administration in operation. During the spring of 1942, some twenty men had participated or assisted in the A-20 JATO tests. One year later, the Air Corps Jet Propulsion Research Project boasted a Director, a Chief and Assistant Chief Engineer, and Army Air Force Liaison, a Technical Aide (actually an architect), and several consultants. These individuals guided research sections (such as Design, Data, and Materials, Liquid Propellants, Solid Propellants, etc.) for the study and testing of rocket motors, while a separate laboratory devoted entirely to design and materials testing inaugurated its work.

Several engineers and technicians worked in these various sections. The Director of Jet Propulsion Research Project No. 1, immediate predecessor of the Jet Propulsion Laboratory, was, of course, Theodore von Kármán. Frank Malina served as Chief Engineer, Martin Summerfield as his assistant; H. S. Tsien, Clark Millikan, the chemists Bruce Sage and Linus Pauling were

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593 P. T. Carroll (compiler). 'Bibliography, JPL-4 ORDCIT Project, Jet Propulsion Laboratory, GALCIT, California Institute of Technology', January 1971. Library of Congress, Malina Collection, Box 13.12.

594 These were the contracts for JATO, hydrobomb, and ramjet research, mentioned earlier. Miles (1946), pp. 87-88.

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consultants. [595] Pauling worked only occasionally on the GALCIT Project. As Caltech has a small and accessible campus, engaging the help of professors in allied departments was not difficult.

Von Kármán's influence, intrinsic interest in the rocket project, previous reliance on the Chemistry Department, and the Rocket's Team's reputation all helped draw researchers into the circle, even if only temporarily. [596] E. M. Pierce, an architect and old friend of Frank Malina's, was the Tech Aide. Pierce was the designer of JPL's first layout in the *Arroyo Seco*, near Devil's Gate Dam. He and a Caltech liaison, W. Hertenstein, head of maintenance and buildings at the Institute, decided where buildings, primitive as they were, should stand.

Beyond serving as Frank Malina's assistant, Martin Summerfield also led the Liquid Propellant Section at JPL. Walter B. Powell, who had helped Summerfield solve (even if only partially) the riddle of regenerative cooling in 1940 and 1941, continued to work under him. John Parsons, logically, ran the Solid Propellant Section, with Mark Mills, Assistant Project Engineer, working directly under his authority. Remarkably, in a May 1943 flow chart diagramming the organizational structure of the Jet Propulsion Project, there is no indication that Edward Forman worked alongside Parsons. [597] He was listed as an employee of Jet Propulsion Project No. 1 prior to May, but by then,

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595 In a letter home to his parents in the summer of 1942, Malina wrote that he had just been appointed "a chief engineer," adding that "the Institute finally appointed me Assistant Professor—for the duration." Letters Home, 20 (?) June 1942, pp. 45-46. At this point, Malina's status as *de facto* administrative leader of the GALCIT Rocket Team became official.

596 Malina (1969), p. 159.

597 Flow Chart: Air Corps Jet Propulsion Research, California Institute of Technology, GALCIT Project No. 1 (May 1943.) The Library of Congress, Malina Collection, Box 12.4.

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his name was missing from the chart. Forman had not disappeared from Pasadena, for he worked at Aerojet in 1943.

There is no clear explanation for Forman's absence from Jet Propulsion Project No. 1. after mid-1943. The reasons may have been entirely personal. According to Liljan Darcourt Wunderman, the Malinas had little to do with Ed Forman after work; he was not among the group of academics with whom the Malinas associated. Mrs. Wunderman indicated the presence of personal friction between herself and Forman, and he may have tried to steer clear of projects guided directly by her husband. [598] Yet, Forman continued to work as a technician at Aerojet that very same year, and, in fact, continued to live just down the street from the Malinas on South Hudson Avenue in Pasadena. [599]

Since the expansion of operations into a formalized organizational structure took place under Caltech's aegis, after 1943 the Institute faced the problem of redefining its administrative relationship to GALCIT. In 1943, many changes were in store for the GALCIT Rocket Research Group. Von Kármán was more than a nominal head of the new laboratory: he was responsible for final approval and direction of research and was initially Caltech's link to the new unit. He chaired meetings of the Jet Propulsion Project when in Pasadena. Even though von Kármán's relation to Caltech had become attenuated by

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[598] This researcher held a second interview with Mrs. Liljan Wunderman, October 16, 1997, New York, NY. Transcripts of the interview have been placed on deposit with the JPL Archives in Pasadena, NASA Archives, Washington, DC, and the Caltech Archives, Pasadena.

[599] Frank and Liljan Malina lived at 800 South Hudson in 1942; Ed Forman resided at 710 South Hudson. See: Agreement for the Incorporation of the Aerojet Engineering Company, March 20, 1942, p. 7. Library of Congress, Malina Collection, Box 16.12.

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1943, he still cultivated links between the military and GALCIT. Not until 1949 did the elder statesman of aeronautical engineering officially become professor emeritus at Caltech.

In fact, von Kármán often had to make some last minute telephone calls to General Arnold to keep several of the Jet Propulsion Research Laboratory's technicians away from the Selective Service. Frank Malina's own draft status was occasionally uncertain between 1942 and 1944. Only through von Kármán's intercession did he remain out of the regular army, on the grounds of the essential nature of his work. [600] Both the Jet Propulsion Research Laboratory and Aerojet regularly lost men to the draft. Most of those called up were lower in echelon on the organizational scale, technicians to be exact. Since the army depended upon the research lab fulfilling its contracts, developing JATOs, and later, long range rockets, depleting the laboratory's manpower would defeat its very purpose.

Into this rapidly unfolding, even murky situation, the Jet Propulsion Research Project plunged, in the early summer of 1943. Its mission was incompletely defined; its projects represented the cutting edge of rocket technology for the 1940s, but its long-term future looked uncertain. Its military clients, however, were somewhat more sure of their stance on the thin ice of this new technology the lab was creating. For instance, in 1942, the navy's objectives had been limited to the purchase of JATOs for carrier-aircraft take-off.

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[600] Malina, Letters Home, 14 December 1941, 17 January 1942, 20 (?) June 1942, 24 October 1943, pp. 46, 53. see also: Malina (1971), p. 350. In particular, the letter of 20 (?) June 1942 mentions how von Kármán wired a certain AAF General Echols in Washington, who then told Malina to wire in his resignation as Reserve Officer to the Adjutant General.

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As far as strategic planning was concerned, the Army Air Force had other plans: develop new forms of transonic and supersonic power plants, concentrating on piloted aircraft. Hap Arnold wanted superior air power and range for his bombers and fighter aircraft; but the AAF commandant was also willing to look ahead to the next quarter, even half-century, and envision how the Army Air Force would structure itself to meet future threats. Arnold could not commit the air force to purchasing rocket technologies from Aerojet or GALCIT, but he could foster research into new military applications of supersonic transport, and eventually, rocketry.

In essence, the GALCIT Jet Propulsion Research Project, by 1944, the Jet Propulsion Laboratory, was cultivating a hybrid arrangement with three major military services. Aerojet was developing the JATO for the navy and researching new types of power plants for the Army Air Force, while the Jet Propulsion Laboratory developed new uses for rockets and athodyds (ramjets.) [601] Often contracts for research into rocket motors or jet aircraft engines had dual provenance. Both Army Ordnance and the air force's Air Technical Service Command authorized JPL to pursue its research. Contracts did not always result in the actual construction of production models, but testing and design were extensive. [602] After July 1, 1944, research on solid

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601 Von Kármán (1967), p. 264.

602 A good example of the complexity of the JPL and Aerojet contract structure can be found on the title page of JPL Memorandum No. 4-7, 'The Liquid Propellant Rocket Motor—Present Status and Direction of Development.' The Report's authors were Frank Malina and Howard Seifert, then working on liquid propellant motors with Summerfield. Clark Millikan and Malina both signed off on the report, Millikan as Acting Chairman of the JPL Executive Board, and Malina as Acting Director of JPL, GALCIT. The report, dated August 28, 1945, had two contract numbers on it: ORDCIT Project Contract No. W-04-200-ORD-455 (ASF Ordnance Department), and Air Laboratory Project MX121—otherwise known as Jet Propulsion Project No.1—Contract No. W 535-ac-20260. Funds for this study came from both Army Ordnance and

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propellant motors became the responsibility of the ORDCIT project—in other words, Army Ordnance. [603] This was to bear fruit within less than a year in the production of the Private A missile.

By 1944, JPL and Aerojet were conducting research into solid propellants, including *Aeroplex*, a smokeless substitute for GALCIT- 53, and later composite propellants which billowed out plumes of white smoke after ignition. As early as 1942, the Jet Propulsion Project had begun research into pumps for the delivery of fuel and oxidizer to rocket firing chambers. In the summer of 1943, GALCIT actually undertook research for Aerojet of the *Rotojet* motor. Martin Summerfield, who took a leave of absence from GALCIT at this time to work at Aerojet, directed this project. *Rotojet* introduced the radical principle of a rotating combustion chamber, but production problems proved insurmountable and the two motors never went into full-scale production. Aerojet engineers did construct models of the *Rotojet* and its successor, *Centrojet*, and eventually their research paid off: Aerojet produced a gas turbine drive that was used in future rocket motors. [604]

The Jet Propulsion Project also undertook experimentation in two other related areas: underwater rocket propulsion and a rocket-propelled towing

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the Air Technical Services Command, based at Wright Air Base in Ohio. A key to understanding the transition the GALCIT Project underwent in these years, 1942-1945, can be found in the designation of the report's sponsorship to the Jet Propulsion Laboratory/GALCIT/California Institute of Technology.

603 Malina (1969), p. 181.

604 Malina (1969), pp. 182-183. In 1937, the Germans had designed an A-4 motor with eighteen separate fuel injectors— centrifugal nozzles which would enhance mixing of fuel and oxidizer. However, the entire combustion chamber was not expected to rotate. See: Neufeld (1995), p. 76.

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car. The towing car was an ungainly device, with two fuel pods on either side of a liquid propellant motor. Its designers, Walter Powell and E. G. Crofut, intended it to pull experimental naval hulls through a long channel, testing hydrodynamic properties of design at high speed. [605] In the fall of 1942, GALCIT began testing solid propellant rocket units submerged up to six feet underwater at an artificial lake formed near Morris Dam, California. The Patent Office granted Malina and Summerfield patent no. 2,431,132 for their invention of the underwater motor. The two engineers even speculated that such motors could be used as underwater JATOs for flying boats or as propulsion systems for torpedoes. [606]

Soon after hearing of this patent, the Army Air Force Air Technical Command asked GALCIT to submit a proposal for a 'hydrobomb.' (Actually, this was to be an air-launched rocket-powered torpedo.) But inter-service rivalries were again at work, because the navy held a monopoly on torpedo production, and so the air force had to call the torpedo by another name. Benjamin Chidlaw's reaction to the idea was predictable. When Malina explained the proposal to him, he remarked. "The next time you come to see me you will want money to put rockets in my swivel chair." [607] But the army's Armament Laboratory picked up the project and allowed GALCIT to build both a towing channel for hydrobomb testing and the rocket-propelled towing car.

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605 Malina (1971), p. 192.

606 Malina (1969), pp. 191-192.

607 Malina (1969), pp. 191.

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The rocket car did not fare so well. In a 1943 static test of the towing carriage, Malina "almost immediately saw one of the combustion chambers of a motor fly past us and flame envelop the carriage. The carriage, in a matter of seconds, was a burned out wreck because a violently 'hard start' of the engine not only separated the parts of one of the motors but also broke the lines of the spontaneously ignitable propellant components." [608]

GALCIT technicians eventually rebuilt the carriage and used it during hydrobomb tests. GALCIT farmed out construction of two prototype hydrobombs, not to Aerojet, but remarkably, to the Westinghouse Company (which was also building the secret jet engine for the air force) and to the United Shoe Machinery Company. United Shoe Machinery, located in Boston, Massachusetts, became involved in the hydrobomb project as the designer and builder of small-scale models. The actual full-size test version measured 10 ft x 28 inches. Frank Malina had made a detailed study of the technical aspects of this underwater rocket-powered torpedo in February 1943, and sent it along through von Kármán to the Army Air Force. It was the AAF which chose the development sites. A fighter pilot could (hypothetically) launch the hydrobomb at flight speeds up to 350 mph. The torpedo was meant to run underwater at 70 mph, powered a by solid fuel rocket, and with an expected range of 1,000 yards. The bomb never went into production during the war. It nevertheless stood as a novel weapon, well ahead of its time. [609]

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608 Malina (1969), pp. 192-193.

609 Malina (1969), p. 194. As usual, the story of the hydrobomb's development was a convoluted one. The AAF (at Wright Field) wanted an underwater jet-powered projectile.

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The Jet Propulsion Project's most spectacular success, prior to its 1945 WAC Corporal sounding rocket tests, was the development of the Private A missile. Not quite a guided missile, but certainly no mere rocket, the Private A, built for Army Ordnance, was the pride of the ORDCIT Project. The first missile built by JPL for the United States Army, the Private A had a maximum range of 11.3 miles. [610] With the exception of the German A-4, this was the only missile successfully tested during the Second World War to have a range beyond ten miles. The Private A was not a tube-launched rocket like the Russian *Katyusha* or the American 'bazooka.' It was a missile with four tail fins. Private A was 92 inches (just over 7.6 feet) long, weighed approximately 550 lbs. and could carry a payload of 60 pounds. Its propellant plant was solid fuel, GALCIT- 61. This composite fuel had distinct advantages over its predecessor, GALCIT- 53, with a higher specific impulse (186 lb.-seconds) and a higher exhaust velocity (6000 ft./sec.).

JPL engineers hoped that Private A would inaugurate an entire series of missiles to be tested for Army Ordnance. The last in the series was, in fact, the Sergeant, a large solid propellant ballistic missile with an inertial guidance system, finally operational in the late 1950s. The Sergeant was the direct predecessor of such ballistic missiles as the Minuteman, but unlike Private A,

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Colonel R. E. Jarman of the AAF requested that GALCIT submit a design proposal and technical evaluation. Both the National Defense Research Council, through Dr. W. V. Houston of Columbia University, and GALCIT became involved in the research phase of the weapon's development. GALCIT and the NDRC were to provide solid propellants for the rocket. However, actual design and construction was left up to Westinghouse and United Shoe Machinery. Both companies built quarter and half-scale models, with Wright Field's Armament Laboratory building the full-scale model. United Shoe Machinery dropped out of the project by mid-September 1945, with the war's end. JPL Archives Document #'s, HC3-734, HC 3-539. Information provided, courtesy of Dr. John Bluth, JPL Archivist.

610 This range was the greatest attained in 24 separate Private A test firings.

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used a butyl-polysulfide (plasticized) propellant, rather than a GALCIT composite. These missiles, categorized by army rank, were meant to stretch from 'Private' on up. But the Sergeant was the highest rank reached. Development of the series halted with the Sergeant's introduction into regular army service in the 1960s. When General Gladion M. Barnes, chief of Army Ordnance during the war, asked von Kármán how high in rank the missile call-names would rise, the professor answered, "Colonel. That's the highest rank that works." [611]

The Private A marked a turning point in North American rocket technology. JPL's first memorandum, 'The Possibilities of Long-Range Rocket Projectiles,' propelled the rocket into testing and model production. [612] As von Kármán wrote in his short advisory to the military, "ranges in excess of 100 miles cannot be realized with propulsive equipment now available in this country. However, with the equipment already developed for super-performance of aircraft, rocket projectiles can be constructed which have a greater range and a much larger explosive load than rocket projectiles currently being used by the Armed Forces." [613]

Von Kármán went on to propose the construction of a solid propellant rocket (to be produced by Aerojet), with boosters similar to those built by the NDRC at Caltech. He envisioned the rocket weighing approximately 350 lbs., with a 50 lb. payload, and a range of about ten miles. In fact, the 1943 memorandum extended JPL's vision beyond the Private A. GALCIT's Director also called for

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611 Von Kármán (1967), p. 265.

612 Von Kármán (1943).

613 Von Kármán (1943), pp. 1-2.

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the development of a liquid propellant rocket weighing 2000 lbs., with a 200 lb. payload and a range of 12 miles. He advocated research into athodyd engines, and eventually proposed testing (under 'Phase 3' of the proposed schedule) a rocket weighing 10,000 lbs., with a range of 75 miles or more. This rocket would become the Corporal E, weighing 11,700 lbs fueled, designed to carry a minimum 200 lb. payload 62.5 miles. (The Corporal E was first tested in May 1947.) [614] The intermediate-weight liquid propellant rocket, transformed from research proposal into reality, would end up as the WAC Corporal.

Malina and Tsien, in the study accompanying von Kármán's memorandum, discussed the technical parameters of a hypothetical solid propellant rocket in detail. In their 'Review and Preliminary Analysis of Long-Range Rocket Projectiles,' the two engineers wrote that "Jet units have been tested that deliver 1000 lb.-thrust for as long as 45 seconds. Duration of this order of magnitude is near the maximum practically attainable. Thrusts of 2000 and 3000 lb. are believed feasible. " [615] In a 'specifications table' some few pages after these comments, Malina and Tsien listed the characteristics of the LRRP-I (Long-Range Rocket Projectile-I):

Solid -Propellant

Initial Weight	350 lb.
Thrust	1150 lb.
Propellant weight	130 lb.
Empty weight	220 lb.
Explosive load	50 lb.
Duration of Thrust	20 sec.

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614 Von Kármán(1943), p. 3. See also: Koppes (1982), p. 39.

615 Malina and Tsien. 'A Review and Preliminary Analysis of Long-Range Rocket Projectiles,' 1943, Pasadena, JPL Archives, Document No. 3-1203, p. 9.

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Maximum diameter	10 in.
Length	81 in. [616]

Private A, with a 10.25 inch maximum diameter, and its Aerojet motor with 1000 lbs. of thrust (the 14AS 1000), compared very favorably with its creators' specifications.

The development of Private A proceeded rapidly, and the first models reached the test stage by December 1944. The quick evolution of the missile, from theoretical plan to fruition, depended upon a relatively flexible working relationship between Army Ordnance and JPL. Letters of Intent substituted for contracts in many cases, with amendments, alterations, shifting of funds; in effect, a sharing of research and contractual responsibilities by Army Ordnance and the Army Air Force Air Technical Services Command became standard procedure for GALCIT in the early-mid 1940s (as it was for several other civilian research agencies and industries doing business with the armed forces.) Time was of the essence, and weapons development could not wait for drawn-out legal formalities.

Actually, this pattern best suited GALCIT's experimental work, and allowed the military to modify or back out of contracts, as it saw fit. In the early days of government/university research collaboration, nothing was hard-and-fast. The two sides groped through a scenario without guideposts, each tentatively committing itself as the need arose, and as vision dictated.

Thus was Private A born. The rocket, technically not a guided missile because it possessed no internal guidance system, nevertheless manifestly advanced

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616 Malina and Tsien (1943), p. 13.

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American rocket technology in the 1940s. Malina and the JPL team built a 36-ft-long launcher to aim Private A toward the sky. The missile (for so Malina called it) would be mounted on a "rectangular steel boom of the truss type, with four guide rails inside the truss....Both the lateral and vertical angles could be varied. The missile was boosted by four modified Ordnance Department aircraft armament rockets in a cluster....The four rockets delivered a thrust of 22,000 lb. for 0.18 sec. They completed their burning and disconnected from the missile before it left the launcher." [617] These Ordnance boosters were none other than the NDRC tactical rockets Lauritsen's group had built throughout the war.

Private A first flew in the Mojave desert, at Leach Spring, Camp Irwin (near Barstow, California) on December 1, 1944. The missile's twenty four test firings lasted for fifteen days, until December 16. Not a single Private A failed. The average down-range trajectory reached 18,000 yards. The longest shot hit 20,000 yards from its take-off point. Estimated trajectory height was 14,500 feet, with an estimated maximum velocity of 1,300 ft./sec. [618]

This speed, approximately 886 mph., breaks the sound barrier. The fact that the missile could become supersonic and keep to its course represented a triumph, not only of motor function, but of rocket design and of forethought. German V-2s often experienced wobbling as they crossed the sound barrier. The barrier is aerodynamically de-stabilizing, and engineers were not sure that a rocket could cross into supersonic speed without tumbling.

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617 Malina (1971), p. 353.

618 Malina (1971), p. 353.

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Apparently, both the bullet-like design and the fast acceleration of the Private A as it left its launcher helped ease the transition to supersonic speed.

Why did Malina call Private A a missile, rather than simply a rocket? Rockets can be both large and small. Generically speaking, even the Saturn V was a rocket. It carried its own internal combustion system, and boosted a payload at supersonic speeds beyond the atmosphere. But a missile is designed to strike a target or insert itself into a keyhole in space that its launch crew pre-determines. In other words, it is a guided projectile. Private A was only partially so; only its launching track provided the necessary guidance for accurate flight. For Malina, Private A was the graduation present the GALCIT Rocket Team gave itself after years of low-keyed, determined work. They had always demurred from aggressively publicizing their work with rockets, for fear of adverse reaction. Now, with JPL Contract No. 4, the team had created a rocket that actually was ballistic.

Private A had tail fins, was fairly long, flew faster than the speed of sound, and landed roughly near its target. It was guided to the extent that its launcher could be aimed, much as an artillery piece is aimed, at a distant point. But it was no mere shell fired out of a gun; nor was it an arrow with a booster attached. Qualitatively speaking, Private A marked the end of GALCIT's JATO phase, and the beginning of missile development for JPL. Once free of its launcher, Private A became a powered, targeted projectile, with an arching trajectory. It was truly on its own. With Private A, both JPL and the US Armed Forces entered the missile age.

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Even so, JPL's first foray into missile construction was not an unqualified success. The next logical experimental phase in the development of the Private missile series was to attach wings to the rocket casing. Both Tsien and Malina had proposed this idea in their 1943 memorandum (JPL Memorandum No.1), hoping to increase Private A's range by about 50%, while reducing its payload. In 1944, the Germans were experimenting along similar lines, with both their *Wasserfall* missile, and the A-9; Wernher von Braun's (1912-1977) group at Peenemünde hoped the latter would glide into an ionosphere-skimming trajectory capable of striking targets as far away from Europe as New York. Neither *Wasserfall* nor the A-9 was ever operationally successful, and the war's end put a stop to Peenemünde's experimentation. [619]

Malina and Tsien, in JPL Memorandum No. 1, had cautioned against early success with a winged missile, as "the problems of stability and control of such an unguided missile were very complicated." [620] Their prediction, ominously, proved highly accurate. The new rocket was called Private F. The appellation was appropriate, for Private F flunked all its tests.

A new test stand was built to accommodate Private F's wings, and the missile was transported to Fort Bliss, Texas for tests beginning on April 1, 1945. "The

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619 See Neufeld (1995), Chapters 3 and 7, for more information on the A-9 and the *Wasserfall* missile. Wernher von Braun led the team that developed the V-2 (A-4) war rocket in Nazi Germany. He came to the United States in 1946, along with other Peenemünde scientists, and went to work using the V-2 for high altitude research. (A WAC Corporal was attached to the V-2's nose in some cases, making it a two-stage missile.) Von Braun also worked on the first American satellite, Explorer, and headed the crew at Huntsville, Alabama, that designed and built Saturn V moon launch vehicles. Von Braun was undoubtedly one of the great pioneers in Twentieth Century rocket technology.

620 Malina (1971), p. 354.

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tests began on April Fool's Day," wrote Malina, "and turned out to be quite appropriate to the occasion. All of Private F's rounds, though successfully launched, went into a tail spin after a short flight. A striking corkscrew smoke trail was drawn in the sky by the rocket jet. Engineers concluded after a post-mortem that better performance might have been obtained if the missile had been constructed with better precision and if the lifting surfaces had been more readily adjustable." [621]

Malina later admitted that the Private F project had been too hastily developed, and that the missile was too complex for then-current technology. Interestingly, he called the Private F both a 'rocket jet' and a missile, illuminating his own ambivalence toward the technological evolution underway. Perhaps, if Private F had been successful, Malina would have felt comfortable calling it a missile. In any case, with this small syntax slip, he revealed his own Janus-like vision: straddling two ages of rocket technology, Malina was present at the birth of simple rocket motors, and at the start of the missile age.

Since late 1935, Frank Malina had focused his professional career on unfolding the rocket motor's dynamics. He had started small. Nine years later, he was on the threshold of fulfilling his goal. The technology of the Private A was simply the very design he, Parsons, and the Rocket Team had sweated over for nearly a decade. The mysteries still to be revealed were those of the Private A's aerodynamics. For this challenge, the wind tunnel and the feathered arrow's basic design could provide helpful answers. (For example,

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621 Malina (1971), p. 355.

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Private A had only three fins, like an arrow, rather than four, which most bombs carry ; this became an important consideration in making Private A aerodynamically stable.) Malina's easy transition between the terms 'rocket jet' and 'missile' in his description of the Private F emphasizes how rooted he had become in the image of his work: he may have dreamed of creating a missile, but he had labored long and hard to produce a small rocket. On some level, Malina had accepted the vision of his age. Rockets capable of leaving the earth behind were still the stuff of fantasy in 1944 America.

Even so, the Private A and its ill-fated successor, Private F, marked a new threshold for the US Armed Forces. Private A's unqualified success opened the door to a new vision. Private A was not the German A-4. The change had come first at Pennemünde, the birthplace of the guided missile. America's military and civilian engineering community had been slow to accept the new light pouring in. Despite this, men like Malina, Tsien, Parsons, and Summerfield had the courage to stand in the door and face the light full force.

Private A, a short, simply constructed missile, also paved the way for the acceptance of systems engineering in rocketry. Goddard had been building complex rockets since the 1930s. His technical marvels had pumps, vanes on tail fins, and an array of conduit piping. He had begun to amass a large number of patents on his sub-system components. Goddard had worked on his own until the 1940s. But a complex system requires development by a team of engineers. One inventor cannot turn the key alone.

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The Private A, even with its JATO vintage solid propellant motor and its external guidance track, raised a whole new series of issues awaiting solution. It would not be long until the liquid propellant motor would replace the composite solid Parsons had created. Liquid propellant motors tend to use fuels with a high specific impulse, and, when radio-controlled, their motors can be turned on and off on command. External guidance controls would, of necessity, yield to an internal system. Motor coolant systems would require perfecting. And the size of the rocket would likely increase. A new generation of rocket technology was beginning to transform aeronautical engineering.

Remarkably, Malina was not present for the inauguration of the Private A tests. He was then winding down his first overseas inspection visit for Army Ordnance, having flown from Pasadena, via Washington, to Prestwick, Scotland, where he caught a train to London. Malina had ostensibly gone to England in return for the visit of a British Ordnance officer, Colonel F. F. Reed, to Caltech in the summer of 1944. [622] In reality, the liberation of much of France by the fall of that year had presented the army with an unprecedented opportunity. Not only could Malina take stock of British innovations in jet and rocket motor technology, but the US Army could send a qualified engineer to inspect captured V-1 and V-2 sites in France.

Malina and Tsien had prepared the theoretical groundwork for the construction of the Private A in their 1943 memorandum. Along with Summerfield, the two men made further studies of missiles and transonic

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622 Malina (1971), p. 349.

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aircraft in the spring of 1944. [623] Other engineers, including Tsien, Clark Millikan, and Homer J. Stewart, could fill in for Malina. Tsien led the research group. [624] Millikan had already conducted scale model tests of a winged ORDCIT missile early in the spring of 1944. [625] GALCIT engineers Chia Chiao Lin (1916-), who had just graduated from Caltech that year with his doctorate in aeronautics, and Wei-zhang Chien (1912-), a post-doctoral researcher there, began studies of trajectories and 'exterior ballistics' for the Private A in the fall. [626]

Evidently, Malina was not indispensable at GALCIT. In keeping with tradition, the former GALCIT Rocket Team, now JPL, worked as a collective unit. Certainly, a small nucleus of seasoned engineers led the research effort. JPL never excluded 'new blood.' Caltech turned out well-trained engineers. Rocketry was an exotic field, and there was a definite shortage of young professional scientists in wartime.

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623 Frank J. Malina, M. Summerfield, and H. S. Tsien. 'Comparative Study of Jet Propulsion Systems as Applied to Missiles and Transonic Aircraft.' JPL-GALCIT Memorandum No. 2, March 28, 1944. Pasadena, JPL Archives, Document # HC3-65. The entire memorandum is neatly summarized in von Kármán's *Collected Works, Vol. IV*, pp. 209-211, and appears, both complete and in summary form, in the Library of Congress, Malina Collection, Box 10.3 (complete) and Box 1, fiche 18.1 (1944).

624 Remarkably, in Malina's 1971 memoir, there is no mention of Tsien's leadership in the Private A project. See: Malina, 'The ORDCIT Project: a Memoir.' pp. 353-355. The only mention of Tsien's work on the Private series comes in Malina's discussion of the Private F, where he acknowledged that Tsien and he had proposed the addition of wings to the Private A to increase its range.

625 Clark Millikan. 'Scale Model Flight Tests of ORDCIT Winged Missiles.' JPL-GALCIT Memorandum No. 4-6, April 8, 1945. Pasadena, JPL Archives. Microfilm # 614-232, Item 34.

626 Chang (1995), p. 106. Chien was born October 9, 1912. He received a doctorate in mathematics from the University of Toronto in 1942, was at Caltech from 1942-1946, and returned to China in 1946. C. C. Lin was born July 7, 1916. He also received an advanced degree from Toronto (a Masters) and completed his doctorate at Caltech in 1944. He worked at MIT from 1953 until his retirement in 1996. Information courtesy of Professor Zheng Zemin, Institute of Mechanics, Beijing, China.

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Malina's trip was very productive, both from JPL's standpoint and personally, for the thirty-two year old engineer. He went over with the 'assimilated rank' of Army Colonel. [627] Malina wore an insignia on his shoulder emblazoned with an embroidered rocket, firing away. But his real reward lay in discussing jet and ramjet engine technology with the British, especially with Air Commodore Frank Whittle, and in inspecting the V-1 and V-2 sites on the continent.

Still, Malina was not completely open with the British, nor they with him. In his memoir, he admits that both he and the British engineers "held back on some of our plans and development." [628] Even among allies, national security concerns insinuated themselves during wartime. This was most especially the case when new experimental technologies were at issue. A certain element of professional reticence, or even rivalry, may also have been at work.

But Malina came away "particularly impressed by the extent of British theoretical analysis and the length of debate on pros and cons before a decision was made to build something." [629] The evolution of British aeronautical engineering seemed to mirror Malina's own conception of how the GALCIT project had evolved. Malina did not mention, however, how

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627 Malina was quickly given a high rank so that he could gain access to sensitive sites in Britain and France. He had been a First Lieutenant in the Army Reserves in 1942, before resigning his commission to continue his rocket work at GALCIT/JPL.

628 Malina (1971), p. 351.

629 Malina (1971), p. 351.

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Parsons and Forman had been so difficult to control in the project's early years; nor how essential Parsons' experimental work had been to the Rocket Team's success.

Malina ascribed the extensive pre-construction theoretical process to wartime exigencies: there were, he asserted, just no men or material to waste on uncertain experiments. The truth is that Malina—even in 1971—still could not remember Parsons' incessant experimentation without some degree of irritation, and this may have colored his judgment. He respected and admired Parsons the man, but he could not identify with his lack of commitment to 'theorizing' as a key to discovery.

While in Britain in October 1944, Malina experienced his first—and only—V-2 attack. V-2's were coming into London from the Dutch coast, and one strike actually landed near the conference hall where Malina and British officers were meeting. He admitted being "rather disturbed" by the explosion, but said, "my colleagues continued the meeting as though nothing had happened." [630] Malina inspected V-2 components at Farnborough Air Base, and was particularly intrigued by the German rocket's guidance system, which he said both he and the British only partially understood. (The Germans, at Kreiselgeräte, Gmbh, along with Siemens A. G. and Askania/Moller, had developed a complicated guidance and control servo-mechanism for the A-4 as early as 1939. Called the *Sg 52*, it was relatively reliable, working to control the missile's pitch during its short 45-second powered phase of flight.) [631]

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630 Malina (1971), p. 351.

631 Neufeld (1995), pp. 94-109. Malina may not have been able to view a whole guidance or

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Malina returned to the United States impressed by the level of German technology. He was loathe to admit it, but the Germans had built a machine he had always dreamed of designing: a true high-altitude ballistic missile. As he flew over the Atlantic toward Washington during the second week of December (the very time Private A launchings were taking place), Malina turned over in his mind all that he had seen, both in Britain and France. He was inspired to push ahead with the design for a sounding rocket. He knew that Private A was "far advanced" in its development. "I suddenly realized," he wrote in his memoirs, "that the first objective our rocket research group had set for itself in 1936 was within reach—a sounding rocket." [632]

The Jet Propulsion Laboratory, fundamentally an arm of the military, yet staffed with Caltech engineers, its administration neither independent of Caltech, nor fully a part of the Institute, had crossed the threshold from the infancy of the rocket age into its early adolescence. The Private A missile and the sounding rocket, dubbed the 'WAC Corporal,' epitomized this new stage of rocket production. The strange and somewhat tortuous evolution of the GALCIT Rocket Team into JPL was rife with ambiguities.

The creation of JPL represents one of the first links in the formation of a solid chain between university research, business, and the military. There were no clear guidelines for this new affiliation, and since JPL and Aerojet were in the

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motor system for the V-2, as he was viewing wreckage. Kreiselegeräte, Siemens, and Askania/Moller were German firms developing gyroscopic stabilizing platforms for the A-4 and A-5. The *Sg 52* was a rather expensive gyroscopic stabilizer, and the Germans tried other combinations, including hydraulically operated servomotors, rather than electric motors, in some models of their ballistic missiles (p.107).

632 Malina (1971), p. 352.

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business of creating and producing rockets, each step in the formation of JPL was an experiment in itself. The very change in the name of the aeronautical laboratory symbolizes this transition. From 1943 through 1944, reports emanating from JPL were invariably entitled GALCIT/JPL reports. The demise of one was slow. The birth of the other, no matter how quickly it had appeared in print after November 1943, brought both administrative conflict with Caltech and a tighter relationship to the army.

A clear harbinger of what was to come in the 1950s and 1960s, the formation of JPL as both an administrative arm of Caltech and a research facility whose land was actually bought (or leased) with army money, whose projects were devoted to military use, spelled a remarkable change for the American scientific community. This blossoming of a close relationship between research institutes and the US military was not only confined to JPL, of course. Often the civilian branch of the federal government acted as mediator in building the rapport. The NDRC had established the Radiation Laboratory at MIT for the study and design of radar, as well as a Radio Research Laboratory at Harvard; and the Manhattan Project relied upon scientific work done at Columbia University, Princeton, and the University of Chicago, among other institutions. [633]

The military contractual process, both in Aerojet's case, and for GALCIT, was fraught with ambiguity and tentativeness. During the war, and for some time afterwards (until the late 1950s or 1960s in some cases), these contracts were usually shrouded in secrecy. The armed forces only slowly bought into the

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633 Burns (1970), p. 346. See also: Kathleen B. Williams. *Secret Weapon, US High -Frequency Direction Finding in the Battle of the Atlantic.* Annapolis, Naval Institute Press, 1996, p. 158.

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concept of the rocket as more than an auxiliary weapon. Hence the complexity of GALCIT and Aerojet contracts with the army. The whole process had an *ad hoc* flavor to it because no policy had yet evolved to incorporate the rocket into long-range strategic planning.

And yet, from this experimental process of technological and administrative innovation came a powerful institutional base for the development of the modern rocket, and a new kind of warfare state. Men like Frank Malina, troubled by the moral and social implications of their lives' work, nevertheless plunged into the new world with enthusiasm. Their whole lives were wrapped up in this business, and try as they might, there was no way to escape the dénouement. The WAC Corporal was not merely the prototype of new generations of civilian space boosters. It was, in its own small way, grandfather to the ICBM.

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Chapter 13

The Culmination: Creation of the WAC Corporal

The WAC Corporal, a sounding rocket just under 16.2 feet high and a bit over a foot in diameter, evokes an image of the nested Russian *matyoshka,* (wooden dolls): smaller and smaller egg-like faces pop out as one opens one doll after another, each revealing a face smaller in size than the one before. Such is the story of the WAC Corporal, a rocket with a tiny brother, appropriately named "Baby WAC," and a much larger sibling, the Corporal ballistic missile. "Baby WAC" was only one-fifth the length of the WAC Corporal, or just over 3.2 feet long. GALCIT designers built "Baby WAC" strictly to test the flight characteristics of a full-size WAC Corporal. [634] The Corporal, largest of the three rockets, when finally constructed, would measure 39 feet in length.

After returning from his fact-finding tour of Europe for Army Ordnance in December 1945, Frank Malina picked up a thread of secret rocket development that had been underway since August 1944. The ORDCIT project had not merely developed and successfully test-fired the Private A rocket. The Jet Propulsion Laboratory had written a memorandum for the benefit of Army Ordnance, entitled 'Research Program for the Second Type of Long-Range Jet Propelled Missile (XF30L 20,000),' ORDCIT Memorandum No. 2. [635]

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634 Malina (1971), p. 361.

635 ORDCIT Memorandum No. 2 (Attributed by Malina to H. S. Tsien) 'Research Program for the Second Type of Long- Range Jet Propelled Missile (XF30L 20,000).' Jet Propulsion Laboratory/GALCIT, California Institute of Technology. August 20, 1944. JPL Archives, Document No. JPLH5, 3-142. The memorandum bears the signature of Theodore von Kármán, but the GALCIT Director merely signed off on the report as a formality. The Army

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The memorandum outlined JPL's plan to engineer and build a liquid propellant rocket with a 20,000 lb. thrust potential, capable of reaching a target 30-40 miles down range. This missile, designated tentatively XF30L 20,000, signifying both its power and its shell diameter (30 inches), was intended to be the fruit of coordinated research by both GALCIT engineers and Aerojet. The drawing-board prototype, designed with an internal guidance system and equipped, either with a cluster of 3,000 lb.-thrust Aerojet motors or one massive 20,000 lb.-thrust motor (not then developed), evolved into the Corporal rocket.

The Corporal was the first operational ballistic missile in the US Army's arsenal. ORDCIT/Aerojet teams designing the Corporal foresaw a test-firing date of March 1, 1945, but this proved far too ambitious. The first Corporal E did not roll off the assembly line until May 1947, with its first test-firing almost immediately afterward at White Sands Proving Ground on May 22. The Corporal weighed 11,700 lbs. when loaded and could carry a 300-400 lb. payload up to 62.5 miles. [636] Its original specifications had called for a length more than two-and-one half times its actual dimension (100 feet), a range half its true potential, and a gross weight of 10,000 lbs. [637]

Over various stages of its development XF30L 20,000 slowly emerged from the simultaneous effort of JPL engineers, who designed the missile's casing and

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Ordnance Contract sponsoring this 'feasibility study' was Contract No. 04-200-ORD-455-RAD, 2865-1.

636 Koppes (1982.), p. 39.

637 ORDCIT Memorandum No. 2 (1944), pp. 1-2.

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calculated its trajectory and guidance system; Aerojet's Liquid Propellant Rocket Section provided the missile's motor. The Aerojet/JPL link, now firmly cemented, was gliding into a new realm of rocket construction. No longer would JATOs be the delivered product. A true missile would be born, the fruit of collaboration between the first American corporation organized primarily to build rockets, and a hybrid unique in American academic life: a joint Caltech/US military research arm fashioned specifically to design rockets. 'Phase 3' of von Kármán's planned program of rocket construction at Caltech and Aerojet was becoming a reality. [638] The gamble GALCIT's director had taken in 1936, risking his academic reputation on a small group of visionary engineering students and unpredictable rocket builders, had produced a dividend not even he could have predicted.

The WAC Corporal was Frank Malina's cherished creation. He and fellow engineer Homer Joe Stewart had hoped to generate Army Ordnance support for its construction with the completion of a January 1945 ORDCIT memorandum, 'Considerations of the Feasibility of Developing a 100.000 Ft. Altitude Rocket (The "WAC Corporal")' [639] The bait worked, and according to Malina, Army Ordnance's Gervais Trichel then requested a JPL study of the proposal. Malina even called the WAC Corporal engineers "my WAC team." [640] Key among them were Stewart and Mark Mills, both of them GALCIT Ph.D.s, both excellent rocket engineers. Malina admitted in his 1971

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638 Von Kármán (1943), p. 3. Phase 3 called for the " design and construction of a projectile of 10,000 lbs. weight or larger with a range of the order of 75 miles."

639 Frank J. Malina and Homer J. Stewart. 'Considerations of the Feasibility of Developing a 100,000 Ft. Altitude Rocket ("The WAC Corporal").' ORDCIT Project, Letter Order Contract No. W-04-200-ORD-455, ASF Ordnance Department, Memorandum No. 4-4, JPL/GALCIT, January 16, 1945. JPL Archives, Document # 3-121, Microfilm # 614-232, Item 33.

640 Malina (1971), pp. 359, 364.

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memoir that, "in 1936, I had placed on my office wall at Caltech a chart showing the component parts required for a successful sounding rocket. The dream had now become a reality, with an engineer in charge of each of those components. "[641]

However, reality was a bit more complex than Frank Malina's vision. Malina had shelved his dream while engineering and testing JATOs, and while building a company whose sole client was the military. It was actually the von Kármán memorandum of November 1943, along with Tsien and Malina's attached collaboration, 'Review and Preliminary Analysis of Long-Range Rocket Projectiles,' which had spurred the WAC Corporal's development. The WAC Corporal, a smaller version of the proposed 10,000 lb. missile foreseen by von Kármán, also had a military call-name. Although most knew the WAC as an acronym for the 'Women's Army Corps', an auxiliary of the army, to those in-the-know at JPL/GALCIT, WAC also stood for 'Without Attitude Control,' since the rocket had no internal guidance system. [642] The problem of guidance was solved externally, by constructing a long launch tower, with initial propulsion out of the tower using booster rockets whose purpose would be to catapult the WAC Corporal to a speed of 400 fps., the minimum required for a nearly vertical trajectory.

Although Malina usually referred to the WAC Corporal as a rocket whose payload was to be purely meteorological, even meant to be shot off near

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641 Malina (1971), p. 364.

642 Malina and Stewart (1945), p.1. Malina wrote that the WAC Corporal "would serve as a scaled-down model of the 'CORPORAL,' which is now under development, and for this reason, the designation 'WAC CORPORAL' has been chosen."

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populated areas as a weather forecasting and scientific research tool (close to the original expectations of Robert Millikan, who wanted a sounding rocket capable of carrying out cosmic ray research), the truth is that he sold the idea for the rocket to the military by pointing out that the WAC Corporal "would represent a logical first step in the development of a guided anti-aircraft projectile." [643]

Malina had quite correctly criticized Goddard for failing to approach rocket research with a true 'team approach.' "The day of the isolated inventor of complex devices was over," he wrote in 1971, musing about the birth of the WAC Corporal. [644] In 1945, the only ready source of sufficient funds for the 'team approach' was the military, and it was the military which seized upon the weapon potential of the sounding rocket. Perhaps prophetically, in mid-1945, the US Army was also preparing to use the atom bomb on Japanese cities, in hopes of quickly ending the war in the Pacific.

As for the WAC Corporal and its future, Malina had little choice but to work with the military. The Jet Propulsion Laboratory, at least partially the brain child of his own efforts and research, had become a government-owned facility. His own university, Caltech, his home since the mid-1930s, was linked to the everyday management of JPL and the Institute exerted influence upon JPL's policy evolution. Malina's efforts, born of his love for pure science in 1936, had led him and his team to collaboration with the armed forces. He disliked intensely their penchant for secrecy; the implications of

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643 Malina and Stewart (1945), p. 1

644 Malina (1971), p. 358.

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rocket research for future international political stability had begun to dawn on him.

However, the only way for Malina to build the sounding rocket without military support would have been to resign from Caltech and begin work with an amateur rocket society, such as the ARS. Other technical institutes, like MIT, were themselves deeply involved in military-related research. If one wanted to build state-of-the-art rockets in 1945, apparently the only road to follow was the military road.

The engineering of the WAC Corporal was actually quite a remarkable achievement. Only a collaborative effort by several scientists and technicians could have produced it. The performance characteristics of both the Private A and the Corporal missiles had been worked out, at least in theory, by Malina and Tsien in their November 1943 report on Long-Range Rockets. The two engineers were able to compute range and thrust considerations for the Private A, creating a set of velocity equations that would hold for a variety of flight angles. [645] These equations were extended and modified to allow for measurement of thrust (specific impulse) for the Corporal.

Difficulties arose because there were no specific calculations available for a rocket intermediate between the Private A and the Corporal. This was odd, as the closest specifications described in the 1943 report were for a liquid propellant rocket with an initial weight of 2000 lbs. and a 4000 lb.-thrust capacity (LRRP-II). [646] The WAC Corporal fell far short of this, with a loaded

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[645] Malina and Tsien (1943), pp. 15-17. For the equations relevant to Corporal performance, see pp. 20-25 of the memorandum.

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weight of 665 lbs. and a thrust of 1,500 lbs. It seems that, prior to late 1944, no thought had been given to the actual detailed analysis and construction of a sounding rocket.

It was Malina's European tour in 1944 which spurred preparations for the WAC Corporal. Malina and Stewart's 1945 study on the '100,000 ft. Altitude Rocket' neatly filled the gap between the Private A and the much larger Corporal. Essentially, what the WAC Corporal team did was refer back to earlier, 1938 studies by Malina, 'Amo' Smith, and Tsien for the equations and data related to the WAC Corporal. [647] Malina and Tsien's formulations, in particular, were quite sophisticated. The Malina/Tsien 1943 collaboration had been a new platform built upon the earlier studies and was the fruit of more than six years' experience in sketching the theory of rocket performance. To some extent, Malina and Stewart's 1945 work relied on past achievements: even Goddard's 1919 monograph, 'A Method of Reaching Extreme Altitudes,' is listed as a reference at the back of the JPL/ORDCIT Report. [648]

There is no question that their previous theoretical investigations gave them the confidence to proceed with the construction of a rocket whose dimensions and full design they had not yet completed. Most of the trajectory equations for the Private A were differential equations, with rate changes in velocity measured as a factor of *d*t. The GALCIT team had not worked through the exact formulations before, and there were several variables to account for,

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[646] Malina and Tsien (1943), p. 13.

[647] These monographs included the work by Malina and Smith, 'Flight Analysis of the Sounding Rocket' (1938), and Tsien and Malina's 'Flight Analysis of the Sounding Rocket with Special Reference to Propulsion by Successive Impulses ' (1938).

[648] Malina and Stewart (1945), p. 12.

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although the equations were relatively straightforward. [649] Tsien and Malina made a good team. Given a specific set of launch conditions, their equations could, in Malina's own words, "completely determine the performance of the projectile." [650]

But would these equations, calculated for a smaller rocket, accurately reflect the performance of the WAC Corporal? Years of experimentation, supplemented by independent theoretical study, classroom teaching, and writing on the details of 'jet propulsion' provided sufficient grounding for equations which would approximate the WAC Corporal's flight dynamics. Malina's comment was a reasonably accurate one. 1945 was not 1938. There was a closer fit between the equations and actual performance in 1945, although some functions, such as thrust variations in flight, were still poorly understood.

Theory had evolved to the extent that mathematics could now define specific parameters of rocket performance—trajectory angles and coordinates at the summit of the flight—more precisely. The problems lay in gathering adequate data for the complex equations the team was employing. Without radio telemetry to report back a missile's exact coordinates and functions from instant to instant, there were large gaps in the information available for

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649 For a more detailed perusal of the equations, see Malina and Tsien (1943), particularly p.16. Most of these equations were non-linear, that is, they did not take the form $Ax+By+C=0$. W. Z Chien's 1944 on the Private A ("Rocket Missile XF10S1000"), JPL Document # Microfilm 614-241, Item 13, and C.C. Lin's 1945 report on the Private A's Exterior Ballistics, JPL Document #614-232, Item 34, also contain several of these trajectory equations.

650 Malina and Tsien (1943), p. 16.

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technical analysis. Such radio communications with an American missile did not come into use until the Corporal was tested in the late 1940s. [651]

Much of the groundwork for the successful completion of the WAC Corporal rested upon the mating of successful, proven rocket motors to a new casing and tail fin design. The closer fit between theory and performance came because theory evolved through a process of insight and feedback from actual experimental results. Experimental and theoretical work in the late 1930s were simply inadequate to fly a rocket as sophisticated as the WAC Corporal.

Since the WAC Corporal was an engineering system, and not a simple solid-pack rocket motor with a steel casing, constructing it necessitated a *systems* approach. The WAC Corporal design and assembly team was large, with 37 members, and component work was delegated to various engineers, radar experts, and technicians. The success of the WAC Corporal program depended upon delegation of tasks to specialist engineers.

Mark Mills, working alongside an Army Ordnance engineer, Captain E. W. Bradshaw, Jr., developed the booster rocket that would accelerate the WAC Corporal to a threshold velocity. Homer Stewart and Wei-zhang Chien calculated the projected trajectory of the missile and its ballistics (its speed and stability at various moments in its flight.) The army supplied officers and enlisted men to develop and install radar tracking of the missile. The entire project was under the command of an Army Ordnance officer, Colonel Benjamin S. Mesick. Logistics, transport, radar, meteorology, and

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651 Koppes (1982), p. 40.

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photographic recording were all within the army's purview. Malina was the only civilian in a position of high administrative authority, and even he was subordinate to Colonel Mesick.

As Tsien was in Europe with von Kármán's Scientific Advisory Group from April to June 20, 1945, touring German V-2 factories and interviewing engineers who had designed and built the Me-262 jet, he was not directly involved with the WAC Corporal project. However, the strategic placement of scientists such as Mills, Stewart, and Chien in key roles meant that civilian mathematicians and aeronautics experts would actually design the WAC Corporal, finessing their theoretical calculations with the actual reality of the constructed missile. [652] The army relied on JPL engineers to produce data on the estimated dimensions and performance of the WAC Corporal that would justify military involvement and expense.

Chien, Stewart, and Mills satisfied the army's demands. The estimated overall length of the WAC Corporal was 168 inches. The actual rocket measured 194 inches. Stewart and Chien rated the liquid propellant motor's thrust at 1,500 lbs., and this was the delivered thrust. [653] The estimated gross weight of the rocket was 700 lbs., its actual weight, 665 lbs. However, the engineering team estimated the WAC Corporal would carry 330 lbs. of fuel and oxidizer. In reality, the missile hoisted 400 lbs. of combustibles. This meant that the missile would have to be lighter than projected, or use more initial thrust to attain the velocity, calculated to be 400 fps., which would

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652 Malina (1971), p. 366.

653 It should be noted, however, that the estimated and the rated thrust of the WAC Corporal matched so neatly because the engineers were using an already proven motor in the missile, the 38 ALDW-1500, built by Aerojet.

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allow it to fly straight up. After all, the WAC Corporal was a sounding rocket: it had to reach a maximum vertical altitude in order to accomplish its mission.

Wei-zhang Chien calculated the performance characteristics of the rocket, based upon "approximate integration of the equations of motion on the assumption that the trajectory was purely vertical." [654] The challenge was to construct a missile that would conform to these specifications. Although the WAC Corporal did weigh less than its theoretical maximum, it carried 60 lbs. more fuel/oxidizer than its designers had projected. The trajectory of the rocket *had* to be vertical, or it would not reach altitude. But in order to create a flight path "purely vertical," as Malina and Chien had assumed, the WAC Corporal would have to jump off the 'starting block' at over 272 mph. (400 ft./second.) And it would have to reach this velocity in just over 0.25 second.

So the engineering hurdles were formidable. Once again, the GALCIT rocket engineers were in uncharted territory. And once more, the team had to use engineering savvy, mathematical approximation, and the pluck or self-assurance that their calculations were accurate in order to build a working rocket. Weight considerations were paramount. Limiting the rocket's weight might spell success for the WAC Corporal. Excessive weight or inadequate thrust spelled disaster. It was decided that the rocket would use a modified 1,500 lb. thrust liquid propellant motor already designed by Aerojet, the regeneratively-cooled JATO 38 ALDW-1500. [655] Malina and Stewart had

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654 Malina and Stewart (1945), p. 8.

655 See: Aerojet Marketing Brochure (1948) 'Experience Aerojet: The Accomplishment,' p. 50. The 38 ALDW-1500 was originally designed as a JATO unit for the Navy Bureau of Aeronautics, and was meant to be used on flying boats. The motor delivered its rated thrust for

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determined that only a liquid propellant motor, using a nitric acid-aniline combination fuel, could provide the thrust necessary to power the WAC Corporal, without adding too much weight.

The team evaluated solid propellants, including ballistite (the high energy propellant used in NDRC rockets built by Caltech during the war) and GALCIT 61-C, Parsons' improved composite propellant. Ballistite would not suffice, however, because it produced acceleration and g-forces that were initially too great, thus making the installation of sensitive meteorological instruments impossible. GALCIT-61 would not do, either. It would produce an impulse of 77 lb.-secs./sec., but for the WAC to reach altitudes beyond 100,000 feet, an impulse of at least 95 lb.-secs/sec. was necessary, and this meant a motor that weighed too much. [656] Sounding balloons had already reached 100,000 ft., and there would be little point in constructing a high-altitude sounding rocket if it could not exceed that height.

The nitric acid-aniline combination suited the WAC Corporal well. As it was hypergolic, the propellant required no ignition assembly. The WAC Corporal engineers even added a new ingredient to the acid-aniline: furfuryl alcohol. Furfuryl alcohol, $C_4H_3OCH_2OH$, has a slightly higher enthalpy (ΔH_v 107.5) than aniline (ΔH_v 103.7) and vaporizes at a lower temperature. [657] So the

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38 seconds, and Aerojet delivered 100 of the units to the navy by May 1945.

656 Malina (1971), p. 359. The impulse/weight ratio of lbs.-seconds/second indicates the rocket's thrust potential by stating it in terms of the number of lbs. of thrust generated per second, for every second the motor is in operation. Thus, a GALCIT 61-C motor producing 77 lb. secs/sec. of thrust would produce 77 lbs. of thrust per pound of propellant burned for every second in operation. In effect, thrust potential is a good measure of the rocket's acceleration, which is the change in velocity/unit time.

657 *Lange's Handbook of Chemistry, 11th Edition.* (1973). pp. 7-84-85, 10-200. For vaporization temperatures, see pp. 9-97, 9-101.

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fuel's energy output would be slightly greater, and its volatility somewhat higher. But even this higher energy fuel, combined with a proven liquid propellant motor, would not suffice to lift the WAC Corporal quickly off the launch pad.

The problem was a very practical one. There could be no internal guidance system aboard the WAC, because any gyroscopically controlled guidance unit would add immensely to the rocket's weight. So the missile had to be externally guided. The launching tower had to be just high enough to shoot the WAC Corporal out on a straight and true trajectory; the instant the missile left its launcher, both the earth's eastward motion, friction, and gravitation would begin to pull the rocket into curved flight. These forces, although physically independent of each other, would combine to lower the WAC's maximum altitude.

Homer Joe Stewart and Mark Mills devised two ingenious solutions. First, Stewart reasoned that he could reduce the rocket's weight even more by reducing the number of tail fins from four to three. The army balked at this idea. All bombs had four fins. Three fins might not provide enough in-flight stability. But Stewart adroitly emphasized that arrows had three fins and flew quite well. [658] When the 'Baby WAC' flew on July 4, 1945 at Camp Irwin, California, and peaked out at 3,000 ft., the army softened its position. The WAC Corporal would carry three fins.

Mills' task was harder, and his solution even more unique. Mills knew that he had to devise a way to get the WAC Corporal off the ground fast. The

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658 Malina (1971), p. 361.

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liquid propellant motor alone would not accomplish this; the rocket, under power of the 38 ALDW-1500 motor alone, would rise slowly. Even with a high launch tower, it would soon veer away from the vertical, as many of Goddard's rockets had done in the late 1930s. The solution was a booster rocket. Clusters of NDRC rockets, attached to the Private A, had launched that rocket quickly and dropped off at the end of the launch ramp. The same type of unit could be mated to the WAC Corporal. But there was one problem: the WAC Corporal was meant to be a vertically launched rocket; the Private A launch ramp had been inclined off the horizontal. The WAC Corporal booster rockets had to be designed so that the rocket itself would leave both the launch tower and its booster rockets behind.

Since the WAC Corporal would stand upright under its launch tower, Mills, Bradshaw, and the 'Booster Group' faced a dilemma. If they placed the booster rockets on top of the WAC Corporal, they would have to install explosive bolts to remove the booster assembly before the missile left its launch track. The blast from the booster rockets might very well sear through the WAC Corporal's casing and ignite the fuel. To install the booster rockets below the WAC Corporal meant designing an escape mechanism, using bolts that would align both the booster and the WAC piggy-back fashion. Mills planned his design so that the booster would drop off at the top of the tower.

This was possible, of course. Mills and his crew designed a set of rods with a circular ring at the top, placing the entire apparatus at the bottom of the WAC Corporal (below its exhaust nozzle); they then secured the booster rocket assembly to this ring. The ring fitted snugly below the rim of the WAC Corporal's fuselage. But the problem was not so quickly resolved. The WAC

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and the booster rockets would be firing simultaneously. The WAC's exhaust just might burn through the booster rockets' casing and ignite them, if there were no heat ablative shield above them.

So the Booster Group designed one. The booster rocket, dubbed 'Tiny Tim,' was short, black, and had three fins, aligned with those of the WAC Corporal just above it. Clustered in its tail were up to 24 modified NDRC ballistite rockets. Technicians could regulate the burning time of 'Tiny Tim' by plugging the exhaust nozzles of some of the NDRC rockets. If the atmospheric pressure and temperature on a particular launch day were too low, selected booster rockets could be plugged, thereby reducing the overall thrust. [659] Mills wanted to ensure that the WAC Corporal would not leave its tower too fast. Mills and Bradshaw had mated two rockets together. They had created an early version of the 'step rocket.'

Now the WAC Corporal team had the outline of a sounding rocket, one designed quite ingeniously. In mid-1945, the WAC Corporal rested at the top of an engineering pyramid, the base of which the GALCIT team had laboriously built up over nearly ten years. The GALCIT/JPL team had an established network of technicians and engineers. Given a ready source of research funds, JPL engineers were able to convince Army Ordnance that their ideas would work. Theory was encapsulated in the process of

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659 In static tests of the booster rockets, Mills' crew determined that the lower the atmospheric temperature, the longer the burning time of the rockets. For example, at 110° F., the burning time was 0.756 second. At 30° F., the burning time was (on average) 0.800 seconds. By plugging selected booster rockets within the cluster, burning time could be reduced, so that the Tiny Tim booster did not produce excessive thrust. See: Capt. E. W. Bradshaw, Jr., and Mark Mills. 'Development and Characteristics of the WAC Corporal Booster Rocket.' Project Note No. 4-30. Jet Propulsion Laboratory, California Institute of Technology, February 26, 1948. JPL Archives, Document # PN 4-30, p. 9.

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developing and writing 'Letter Order Contracts.' These short contracts were essentially research proposals, and military officers served as the review board evaluating them. The army expected sound theoretical work which would establish clear performance guidelines.

But these 'Letter Order Contracts', of necessity, had to include some experimental data. As Malina wrote in 'Considerations of the Feasibility of Developing a 100,000 Ft. Altitude Rocket,'

> The calculations of the performance of the rocket are based on earlier analyses of the sounding rocket carried out by various investigators. The parameters used in the present calculations are based on present day experience with propellants and rocket unit components and recent data on the drag coefficient of projectiles. [660]

Such "earlier analyses" had been purely theoretical and included Robert Goddard's 'A Method of Reaching Extreme Altitudes,' Willy Ley's '*Les Fusées Volantés Météorologiques*,' as well as Malina and 'Amo' Smith's own 1938 work, 'Flight Analysis of the Sounding Rocket,' and Tsien and Malina's study on propulsion by 'successive impulses.' [661] Malina and Stewart made sure that the literature consulted, while not voluminous, reflected well-established work and their own prior research.

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[660] Malina and Stewart (1945), p. 2. The Army Ordnance 'Letter Order Contract' No. was # W-04-200-ORD-455, the same contract funding the Private A.

[661] Malina and Stewart (1945), p. 12. Goddard's work appeared originally in the *Smithsonian Miscellaneous Collections*, Vol. 71, No. 2 (1919); Ley and H. Schaefer's ' Les Fusées Volantés....' appeared in *L'Aerophile*, Vol. 44, No. 10 (1936). p. 228; Tsien and Malina's study, more technical than Malina's work with A. M. O. Smith, was published in the *Journal of Aeronautical Science*, Vol. 6, No. 2 (1938). p. 50. All these works, including Smith's 'Flight Analysis of the Sounding Rocket,' *Journal of Aeronautical Science*, Vol. 5 (1938) have been cited above.

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True to form, Malina's' Considerations of the Feasibility of Developing a 100,000 Ft. Altitude Rocket' appeared at first to be largely theoretical. Although the choice of propellants had been made on the basis of experimental evidence, in January 1945, when Malina's memorandum was completed, Malina and Stewart had not yet built a single WAC Corporal. The project's main goal shaped the missile's specifications: construction of a relatively small rocket with a narrowly-defined mission— exploration of the fringes of space. The point was to calculate the trajectory of a missile expected to reach 'extreme altitudes,' as Goddard had so tersely put it in 1919. With a motor chosen from the working inventory of Malina's own company, Aerojet, a motor with a known thrust rating, Homer Joe Stewart concerned himself with launch and flight velocities.

Stewart went to work, calculating the stability of a missile in 'vertical flight.' He and Malina believed that, by discovering the minimum deviation from the vertical in flight, they could calculate the optimal initial lift-off velocity of the rocket. This initial velocity, U_0, could not be too high, for, as previously noted, too severe a take off speed would spell disaster for any scientific instruments on board. To determine the optimal value of U_0, Stewart calculated a series of ratios, θ / θ_0, where θ represented the angle of deviation from the vertical during flight and θ_0 = the rocket's angle of deviation from the vertical just prior to launch. $\theta_0 = 1$ when the rocket stood on the ground, for, as Stewart and Malina wrote, "it is probably not practical to expect the deviation angle at launching to be less than 1°." [662] For any simple multiple of U_0—100, 200, 300 ft./sec., there had to be a ratio, θ / θ_0,

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662 Malina and Stewart (1945), p. 7.

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which theoretically expressed the degree of divergence from the vertical the rocket might assume in flight.

Stewart's use of this ratio of trajectory angles, θ / θ_0, meant that two key variables would determine optimum initial launch speed: velocity in flight and the angle of the rocket's path upward. The smaller the ratio, the more vertical the flight path. Stewart wrote out an equation which appeared to be simple and straightforward:

$$d\theta/dt - \frac{g}{U}(\theta) = 0$$

where U= the flight velocity
g= gravitational force, and
t= time.

In other words, a change in the rocket's trajectory equaled its velocity—increasing steadily as gravitational force decreased—multiplied by its angle of deviation from the vertical.

$$d\theta/dt = \frac{g}{U}(\theta)$$

Of course, Stewart knew that, to maximize the rocket's altitude (given constant thrust and acceleration), it was important to maintain the trajectory as close to the vertical as possible. In other words, the rocket had to fly nearly straight up during its powered phase, to reach as great a height as possible. The objective was not to have the WAC Corporal fly in an arching trajectory like an artillery shell, but for its parabolic path to peak above 200,000 feet. (This, of course, meant that the rocket would land fairly close to its launch point.)

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Now, velocity increases linearly with time, so

$$U = U_0 + at$$

(a= constant acceleration)

This is the case because $a = \frac{U - U_0}{t - t_i}$

if, $t_i = 0$ and t = any arbitrary later time. [663]

(Here, U_0 = some arbitrary initial velocity.)

Therefore, $a = \frac{U - U_0}{t}$

and consequently, $at = U - U_0$.

Finally, $U = U_0 + at$

Taking the derivative of θ as a function, not of time, but of velocity, Stewart resolved the equation $d\theta/dt = \frac{g(\theta)}{U}$ into—

$$Ud\theta/dU - [g/a]\ \theta = 0$$

He could do this because $d\theta/dU$ is a function of both trajectory angle and velocity. Both of these will determine the rocket's maximum altitude.

Velocity equals dx/dt, $(v = dx/dt)$

with x = a vector in any direction.

Stewart knew that a rocket's trajectory in the x direction was a vector equal to 90° minus its deviation from the vertical (90° - θ), and as,

$$v = v_0 \cos(90° - \theta), \text{ and } v = U$$

therefore, $U = U_0 \cos(90° - \theta)$ or $U = U_0 \sin\theta$

and $U = dx/dt$. [664]

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[663] Young (1992), p. 39.

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This equation, which represents the merging of two different forms of vector analysis, indicates that the rocket's velocity would be a function *both* of time and the angle of deviation from the vertical during flight.

Now, from the original equation, $d\theta/dt = \frac{g}{U}\theta$, as gravitational force diminished every instant the rocket was accelerating upward, g/U became smaller and smaller—in fact, it became a constant acceleration. (This also implies that $d\theta/dt$ was approximately equal to some multiple of θ.) Since $d\theta/dt$, then, is a function of time, and velocity and acceleration are both functions of time, $d\theta/dt$ and dU/dt must be related to each other. They are connected by the Chain Rule, and so,

$$(d\theta/dt) \times (dt/dU) = d\theta/dU \text{ [665]}$$

Then if, $$U = U_0 + at$$

when this equation is differentiated,

$$dU = a\,dt$$

Both sides of this differentiation can be divided by a:

$$dU/a = a\,dt/a.$$

The result, $dU/a = dt$, of course, can be substituted into the equation

$$d\theta/dt = \frac{g}{U}\theta \text{ to give—}$$

$$\frac{d\theta}{dU/a} = d\theta\,\frac{(a),}{dU}$$

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664 Young (1992), p. 64.

665 The Chain Rule relates two derivatives with different variables in a composite function by relating the first derivative to the second derivative through a middle variable which 'connects' the two. In Leibniz notation, $dy/dx = (dy/du)\,(du/dx)$. See: Varburg and Purcell, *Calculus, with Analytic Geometry, 6th Edition*. Englewood Cliffs, NJ, 1992, pp. 123-130.

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which simplifies to $a\frac{d\theta}{dU}$.

The entire equation then becomes:

$$a\frac{d\theta}{dU} - \frac{g\theta}{U} = 0.$$

Multiplying this equation by (U/a) gives

$$U\frac{d\theta}{a\,dU} - \frac{g\theta}{U(a)} = 0.$$

and, by cancellation,

$$U\frac{d\theta}{dU} - \frac{g\theta}{a} = 0.$$

In this equation, Stewart assumed that velocity was a constant: (U = a).

Stewart achieved the final steps by re-iterating that flight trajectory was fundamentally a function of velocity, the force of gravity (g)—which was becoming more and more negligible as the rocket ascended—and θ.

By integration,

$$\theta/\theta_0 = [U/U_0]^{g/a}$$

or

$$\theta/\theta_0 = [1 + at/U_0]^{g/a}$$

The steps leading to the above results begin with the equation:

$$U\frac{d\theta}{dU} = \frac{g\theta}{a}$$

Dividing both sides of this equation by U, the next step is—

$$\frac{d\theta}{dU} = \frac{g\theta}{aU}$$

Then, dividing this quantity by θ, the resolution of the consequent step is:

$$\frac{d\theta}{\theta dU} = \frac{g}{aU}$$

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Lastly, before integration is performed, multiplication of both sides by dU brings:

$$\frac{d\theta}{\theta} = \frac{g}{a}\frac{dU}{U}$$

The next step in Stewart's derivation of the above ratio, θ / θ_0, involved indefinite integration, using the natural logarithm, *ln* :

$$\ln\theta \int = \ln U(g/a)$$

which unfolds to $$\ln\theta = \ln U(g/a)$$

However, this is none other than a form of the natural exponential function,

$$e^{\ln\theta} = e^{\ln U(g/a)}$$

Because, as a general rule, $e^{xy} = (e^x)^y$ [666]

Therefore $$e^{\ln\theta} = (e^{\ln U})^{(g/a)}$$

(here $y = g/a$ and $x = \ln U$)

and because $$(e^{\ln U}) = U$$

(any $e^{\ln x} = x$)

thus $$(e^{\ln U})^{(g/a)} = U^{(g/a)}.$$

But since $e^{\ln\theta} = (e^{\ln U})^{(g/a)}$ (see above)

$$e^{\ln\theta} = \theta,$$

it is clear that $$U^{(g/a)} = \theta$$

What followed for Stewart was the creation of the ratio

$$[U/U_0]^{(g/a)} = \theta / \theta_0{}^{(g/a)}$$

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[666] The natural exponential function, e, is defined as $f(x) = e^x$, and represents an irrational number approached as one derives the quantity $(1 + 1/n)^n$ [as n increases to infinity]. Leonard Euler (1707-1783) first calculated e to several decimal places: 2.71828183. See: Aufmann, Barker, and Nation. *College Algebra and Trigonometry, 2nd Edition*. Boston, Houghton Mifflin, 1993, p. 255.

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And, since, from the equation for velocity,

$$[U/U_0] = \frac{U_0 + at}{U_0} \quad \text{and,}$$

$$\frac{U_0 + at}{U_0} = \left(1 + at/U_0\right)$$

The final result for Stewart was:

$$\theta / \theta_0 = [1 + at/U_0]^{g/a}$$ [667]

Returning to the equation, $d\theta/dt = \frac{g(\theta)}{U}$,

Stewart reasoned that if g/U constantly decreased as the rocket gained in altitude, then the deviation of flight path from the vertical $d\theta/dt$ would also decrease—that is, the flight path would not diverge too much from the vertical, as gravitational force weakened. Of course, there would always be some deviation, the result of air resistance, wind gusts, vibration, etc. So, Stewart had to find a value for θ/θ_0 that was as close to the ideal as possible.

Now, the degree of divergence from the vertical at the apogee of the rocket's flight was actually proportional to the initial lift-off velocity, for as Stewart calculated $\theta / \theta_0 = [1 + at/U_0]^{g/a}$

setting the exponent $g/a = 1$, then

$$\theta / \theta_0 = [1 + at/U_0]$$

Since "the height obtained during coasting [at the end of powered flight] is proportional to $\cos^2 \theta$ $\{\cos^2 \theta = [\cos \theta]^2\}$, where θ is the angle from the

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667 I am indebted to Mr. Yibao Xu of the Dept. of the History of Science, City University of New York Graduate Center, for his gracious help in clarifying certain key points in Stewart's derivation of the optimal flight path for a sounding rocket.

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vertical at the end of burning," if $\theta = 20°$, then $(\cos 20°)^2 = 0.883$, equivalent to a 12% loss in height. [668] Because Stewart and Malina "wanted to limit the inclination at the end of burning to about 15°,"—about a 10% loss in maximum altitude—they had to find a corresponding initial velocity, U_0, which would conform to this expected figure. [669]

For a set of initial velocities, U_0, with $g/a = 1$, Stewart derived the following data:

Deviation Ratios for t= 44 seconds

θ / θ_0

U_0 (ft/sec.)	100	200	300	400	500	600
g/a=1	15.2	8.1	5.7	4.5	3.8	3.4

These data show that the higher the initial velocity, the less the deviation from the vertical in flight. Since an initial velocity of 500-600 ft/sec. would be too high, i.e. would create g-forces beyond the stress limits of the meteorological payload, Stewart chose the value $\theta / \theta_0 = 15.2$ as the outside limit for trajectory deviation. This would have meant an initial liftoff velocity of approximately 100 fps., too low a figure. [670] The actual initial lift-off speed of the WAC Corporal was close to 400 fps., with $\theta = 4.5$. [671]

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668 Malina and Stewart (1945), p. 7.

669 Malina and Stewart (1945), p. 7.

670 For Stewart's own derivation of these equations and data, see: Malina and Stewart (1945), pp. 5-7. Stewart chose a flight time of t = 44 seconds because he knew the specifications of the Aerojet motor and could calculate that at 1500 lbs. of thrust, the rocket would require 40 50 seconds to reach an altitude of 100,000 ft. The deviation ratios listed above may be found on the same page.

671 Malina and Stewart (1945), p. 8

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There was no way to guarantee that a meteorological accident, such as a sudden wind burst at altitude, or mechanical vibration, would not alter the missile's pitch in flight. Stewart had to assume ideal conditions for his calculations to have any validity. He had to design the WAC Corporal for stability and low weight. These two variables were at odds, for certainly, the heavier the rocket, the less it would be buffeted by winds during flight. Added weight did not necessarily correlate with increased stability, as the Germans had discovered in their A-4 tests. Design and the behavior of the rocket as it approached and passed transonic speeds would also have determined the failure or success of a WAC Corporal flight.

Given advantageous meteorological conditions in the New Mexico desert—generally dry and cloudless—and Stewart's economical design of the WAC Corporal fuselage and tail assembly—his calculations were remarkably accurate. His values for the initial velocity, U_0, were arbitrarily chosen, in simple whole number multiples of 100 ft/sec., and he assumed no deviation from the vertical in flight; in point of fact, the WAC Corporal flew straight and true. Of course, the story was a bit more complex. Lacking an internal guidance system, the WAC Corporal team had to construct a launch tower approximately 100 feet high to ensure that the rocket would reach launch velocity without toppling over soon after it flew in open air.

Stewart's calculations provided more than theoretical reassurance that the rocket might fly correctly. His determination of launch velocity and optimal deviation from the vertical in a wind-less sky gave Malina and the WAC Corporal team guidelines to help determine the amount of fuel to be carried,

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the tower height, and a reasonable calculation of the altitude the rocket would reach. Not inconsequentially, the mathematical data also convinced Army Ordnance that the WAC Corporal was worth funding.

'Considerations of the Feasibility of Developing a 100,000 Ft. Altitude Rocket,' short as it was (12 pages, excluding charts and diagrams), represented a landmark in the evolution of Frank Malina's (and Homer Joe Stewart's) thinking. Malina truly had left behind von Kármán's *mantra* : theoretical, mathematical proof first before experiment (although he would still have been loathe to admit it.) The paper is heavily theoretical, but laced through it, Malina and Stewart gracefully admit their dependence upon prior experimentation as the basis for key decisions about the WAC's design and propellants. Just after Stewart's calculation of optimal initial velocity ends, Malina added the following qualification:

> For a constant thrust rocket unit, the simplest kind to construct, the net acceleration is not constant, but increases as the propellant is consumed, so the results of the preceding calculation...are not strictly applicable to this case, but the results can be applied by using a mean acceleration in which the initial acceleration is weighted more heavily than that during the later part of the burning period.[672]

The dynamics of rocket flight were too complicated for the first-order differential equations Stewart had devised. The WAC's acceleration was not constant, but varied throughout the flight. If Malina and Stewart had not approximated the projected velocity, they would have been left writing data that did not conform to true flight specifications. Actual flight performance might differ markedly from their predictions. From instant to instant, the WAC Corporal would have been increasing its velocity as it became lighter

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672 Malina and Stewart (1945), p. 7.

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and lighter. Therefore, only higher order equations would have approximated the true state of the rocket's motion from second to second.

Malina and Stewart could have used higher order derivatives, such as second derivatives, to find the mean acceleration of the WAC Corporal. Yet they did not, leaving such calculations to other engineers in the future. They gave no explanation as to why they were not more precise and detailed in their calculations; undoubtedly, they were pressed for time, as their report appeared in print less than one month after Malina's return to the United States.

Malina wrote in his memoir of the ORDCIT project: "The Army Ordnance Department requested that JPL study my proposal for a sounding rocket. It was quickly completed and submitted on January 16, 1945...." [673] Malina had returned to the United States from his European tour in mid-December 1944. When Malina contacted Colonel Trichel in Washington (before traveling back to Pasadena) and told him of his new idea, Trichel's response was immediately favorable. Malina had, in fact, sold him on the concept of a scaled-down version of the Corporal as "a first step in the development of a guided anti-aircraft missile." [674] In early January 1945, priorities for Malina and Stewart involved convincing the army to fund their rocket. One did not need complex mathematics to do so; one only had to prove to the army's satisfaction that the rocket could fly.

Precise technical description of the WAC Corporal's flight, however, would have involved more detailed mathematical structure in their paper.

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673 Malina (1971), p. 359.

674 Malina (1971), p. 353.

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Essentially, the type of motion Malina was measuring was not strictly linear. The rate of fuel consumption would have been changing from moment to moment, and there was no way to measure the actual conditions inside the fuel chamber. Even assuming a constant feed rate of propellant into the chamber, Malina could not have predicted that the burn rate would remain constant as the rocket accelerated. If the burn rate did remain constant, then it would be safe to say that the WAC's velocity would still have been impossible to predict accurately, as the missile would have been accelerating every single moment until its motor cut off. Stewart and Malina had to assume a minimum initial velocity, and predicate their trajectory calculations upon that value.

To have described the WAC Corporal's ballistics as precisely as possible would have necessitated a computer. In 1945 America, computers were not enlisted in the effort to calculate rocket guidance. The Mark I, a digital computer developed at Harvard for the navy and the NDRC, was operating as early as 1944, carrying out complicated differential (ballistics) calculations. The Office of Naval Research, created in 1946 by the War Department, soon began funding digital computer research at MIT, the Raytheon Corporation and other venues. MIT's *Whirlwind* (on line by 1951) and Raytheon's *Hurricane* were the fruits of this work. But Caltech engineers had no access to such machines. In fact, Homer Stewart used an adding machine to help him with his trajectory calculations. Nothing more sophisticated was available to him. [675]

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675 Paul N. Edwards. 'Why Build Computers ?' In, *Major Problems in the History of American Technology*, Merritt Roe Smith and Gregory Clancey, eds. Boston, Houghton Mifflin, 1998, pp. 456-457. The article is excerpted from Edwards' *The Closed World: Computers and the Politics of Discourse in Cold War America*. Cambridge, Mass. MIT Press, 1996. In an August 1998 interview with this researcher, Dr. Stewart claimed that, even with the help of an

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Computer guidance of missiles during wartime had made strides under the Nazis. Helmut Hoelzer, a civilian enlistee who worked at Peenemünde after October 1939, developed a prototype analog computer used to test guidance systems' accuracy before they were installed in A-4 missiles. Hoelzer wrote a two-part dissertation on his analog computer in 1945—he divided the thesis into two parts because US Army occupation authorities would not let him publish a work on computer application to rocketry, very much considered a military application in post-war Germany. Hoelzer then re-submitted the thesis, with all references to rockets in the second part only. He showed only the latter half to his professors at the University of Darmstadt.

After receiving his doctorate in 1946, Hoelzer emigrated to the United States, where he went to work under Wernher von Braun at the army's Redstone Arsenal in Huntsville, Alabama. In the early 1950s, an American electronics expert who had worked with Hoelzer at Huntsville, James Farrior, developed an analog computer which could correct for sideways drift and range control problems on the Redstone rocket. This machine was the first computing device installed on any American missile. [676] Further research on digital computers in the United States would have to await work done at the Defense Advanced Research Projects Agency (DARPA), founded in 1958, and

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adding machine, the WAC Corporal calculations took days to complete.

[676] Paul Cerruzzi. *Beyond the Limits, Flight Enters the Computer Age.* Cambridge, MIT Press, 1989, p. 86. See also: *Understanding Computers: Space.* Time-Life Books, eds. Alexandria, Virginia, Time-Life Books, 1987, pp. 8-13. Information on Hoelzer's work at Peenemünde may also be found in Neufeld (1995), pp. 104-107. An analog computer uses the movement of gears or voltage fluctuations, that is, continuous variables, to measure data. However, a digital computer records data in discrete on-off bits, thus basing its technology more fundamentally on quantum mechanical principles.

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at NASA. Both of these organizations went into overdrive in their research work, following the launching of *Sputnik* in 1957. [677]

In 1945, what saved Malina and Stewart the necessity of hair-splitting calculations was one simple fact: to attain a minimum altitude, the engineer need only know a minimum threshold velocity. The longer a rocket is in flight, the greater the possibility that some anomaly, either mechanical or dynamic, will push it to diverge from a predicted flight path or anticipated performance. There are too many variables at work simultaneously for the projectile to function exactly as predicted. Burn rates can fluctuate; the rocket can roll on its vertical axis, thus changing its path as it turns; pumps can vary the flow rate of fuel to the chamber as the rocket lightens; the very chemistry of combustion can change during flight. Rocket engineers in 1945 could only predict the gross behavior of the machines they were constructing.

Certainly, Malina would not have confidently predicted the theoretical success of the WAC Corporal, had he not felt reasonably sure that his and Stewart's mathematics were on the mark. Notably, the WAC Corporal flew on a nearly vertical path for every one of its ten test rounds. However, the powered segment of its flight was short, on the order of 45 seconds. Nevertheless, in 45 seconds a great deal can happen inside a rocket motor. There was no guarantee that any flight of the WAC Corporal would succeed, even though a proven rocket motor had been installed in the missile.

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677 Jeffrey Bingamam and Bobby Inman, 'Dual-Use Technology, 1991.' In, Smith and Clancey, eds., (1998), p. 446.

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The WAC flew well, not merely because it had no defined target, but because Malina's crew had systematically constructed a machine that would perform *within* estimated performance parameters. The rocket motor's design had been tested over a period of more than two years. The WAC's engineers had control over several flight variables: overall weight, type and amount of propellant used, design of the motor, pumps, and aerodynamics. Even so, each new flight was an adventure.

However, the reliability of the WAC power plant and auxiliary systems was not a matter of luck. These systems, mechanically speaking, would have proven quite durable, even if the WAC Corporal had flown for several minutes. It was still possible for some type of anomaly to develop within the fuel combustion process, but at what exact instant chaotic fluid behavior might have set in depended upon the threshold for instability within each particular motor.

The WAC Corporal began on paper in mid-January 1945. Eight months later, almost to the week, the first fully operational WAC flew at the White Sands Proving Ground. From vision to reality in less than a year—the pattern of quick construction, with theoretical support sometimes tagging along—this had been a hallmark of GALCIT work ever since the late 1930s. But there was a hint of an added incentive to rapid testing of the WAC Corporal, for Malina knew of the coincidence between the detonation of atomic bombs over Japan in August 1945 and his own missile work. [678] Clayton Koppes, in *JPL and the*

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[678] Malina (1971), p. 364. Malina wrote, after viewing the test site of a bomb blast sometime after September 26, "It was a very disturbing sight, especially for us who were involved in the development of long-range rocket missiles."

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American Space Program, noted that "[the army] ranked guided missiles second only to nuclear weapons as a military priority" at the end of the war. [679]

October 11, 1945 was a momentous day for Frank Malina and the entire crew. Four rounds had been fired previously, but they had been 'dummy rounds,' the nose cone filled with concrete, or the missile only partially filled with propellant, to check the reliability of the motor. On that day, a Thursday, from a control house 465 feet away from the launch tower, a member of the 'Missile Firing Group' of the WAC test program sent an electrical signal to the base of the WAC (through a circuit attached to the ballistite rockets), and the missile was on its way. [680] "We craned our necks to watch the WAC's smoke trail until the engine stopped at around 80,000 ft.," Malina wrote exultantly.

> On the basis of radar tracking data, it was estimated that the maximum altitude reached was between 230,000 and 240,000 ft. The total time of flight was about 450 seconds, or 7.5 minutes. The velocity of the WAC at the end of burning was about 3100 ft. per sec. The impact point of the first round was about 3,500 feet from the launcher, which meant that the WAC had maintained a very satisfactory vertical path. Success!" [681]

Malina had launched his sounding rocket. Despite his intensely controlled demeanor, he could not hide his joy. Even his restrained, matter-of-fact description revealed a wealth of information. The first implication of his

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[679] Koppes (1982), p. 24.

[680] Malina (1971), p. 361. For a description of the WAC Corporal booster ignition method, see: Bradshaw and Mills (1948), p. 13.

[681] Malina (1971), p. 365.

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short triumphant note was that data on the flight was quite imprecise. The zenith was "between 230,000 and 240,000 feet....Velocity was about 3100 ft./sec...; the impact point was about 3,500 feet from the launcher." Translation: there were limits on the accuracy of data the WAC crew could collect on the flight.

Some of the numbers are startling. If the WAC Corporal peaked out at over 230,000 ft., it reached an altitude of over 43.5 miles. It was nearly in outer space, as the threshold with space lies at approximately 50 miles above the earth's surface. At 3100 ft./sec, the rocket was traveling at 2113.63 mph., or Mach 2.8, nearly three times the speed of sound. What kind of buffeting had the rocket endured as it passed through the sound barrier? There was no way for Malina to tell. The WAC's shell, minus its nose cone, landed 3,500 feet from the launch tower—two-thirds of a mile away.

The nose cone, of course, presented a dilemma. The engineers decided to equip the WAC with a parachute, so that its meteorological instrumentation could be salvaged. As the WAC passed through its flight apogee, a small gyroscope in the nose cone detected deviation from the vertical and closed an electrical circuit to a set of explosive bolts. The cone then was supposed to separate from the missile's fuselage. There was also a mechanical clock in the payload bay, as well as a radio link to the Army Signal Corps Group on the WAC team; theoretically at least, there were three systems for retrieving the nose cone.

Six fully operational WAC Corporal flights lifted off in 1945, and not a single one of them properly deployed the nose cone parachute. No matter, reasoned

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Stewart. The nose cone wouldn't land near the ground crew, anyway. He was sure the "probability of being struck was extremely small." [682] So, many of the crew just milled about, hoping to see the nose cone float down at a relatively leisurely pace. Instead, on one shot, the cone came crashing in at over 2000 mph., two hundred yards from where the men were standing. [683] The parachute had failed to deploy, and true to form, the WAC Corporal had landed close to its lift-off point. Ten years of bizarre mishaps had inured the GALCIT rocketeers to 'a touch of drama.'

Statistically speaking, the WAC Corporal shots were completely successful. Out of six operational tests, there was not a single failure. The tenth lift-off was the most dramatic, as it was a night flight, Thursday, October 25, 1945. But again, the parachute refused to work properly. "This did not surprise us," Malina blithely commented. "There was very little experience on the behavior of chutes at high altitudes." [684]

Malina and the WAC team soon made plans to remedy this deficiency. Several recommendations to Army Ordnance on redesign shaped a new agenda: construction and test firing another round of WAC Corporals later in 1945 or early in 1946; included on the list was a call to redesign "a reliable nose cone release mechanism and...a stronger attachment of the parachute to the vehicle."[685] Following a strategy session among JPL engineers, Army Ordnance, and Army Signal Corps personnel in Aberdeen, Maryland,

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682 Malina (1971), p. 366. The quote is Malina's.

683 Malina (1971), p. 366.

684 Malina (1971), p. 366.

685 Malina (1971), p. 367.

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November 9, 1945, a new round of WAC Corporal tests was scheduled for the next year. The White Sands Proving Grounds was the scene of these tests in December 1946 and February-March 1947.

The WAC Corporal had proved its mettle. The next generation sounding rocket, the Aerobee, designed by Aerojet and assembled at Douglas Aircraft's Santa Monica, California plant (where the WAC had seen the light of day), was its direct descendant. The Aerobee broke ranks with the WAC Corporal in one significant respect: it became a vehicle devoted solely to the scientific exploration of the atmosphere, and was not the progenitor of a new class of military rockets. The Aerobee, first launched on November 24, 1947, was a Navy Bureau of Ordnance-funded project.

The director of the Aerobee program, however, was James A. Van Allen, of the High Altitude Research Group of the Applied Physics Laboratory at Johns Hopkins University. Van Allen had visited Aerojet in 1946, "to survey its rocket capabilities." When he returned East, he enlisted navy support for the construction of the Aerobee. A 45AL-2600 Aerojet liquid propellant motor powered Aerobee. This motor used the same RFNA-aniline-furfuryl alcohol fuel that had filled the WAC Corporal's tanks. The Aerobee even had the same type of booster rocket (the 'Tiny Tim' variety, but lighter, with a higher thrust), mounted below the Aerobee's rocket motor, as was characteristic of the WAC. [686]

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686 *Aerojet: The Creative Company* (1997), p. III-7; III-36. A photograph of an Aerobee launch appears on p. III-39. See also: Koppes (1982), p. 24.

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The WAC-Aerobee connection generated a secondary benefit: for the first time, West Coast rocket technology was migrating to the East, rather than the other way around. The effort to build a rocket for peaceful scientific uses had become nationwide. Aerobee would hoist instrumentation into the upper atmosphere well into the 1950s. Malina could feel justifiably proud. His work had spurred the development of a rocket whose design was not meant to be mated to an explosive.

When Malina watched the first fully-charged WAC Corporal soar into the sky at White Sands on October 11, 1945, he may have felt himself present at the birth of a new era in rocketry. "In the astronautical fraternity," he wrote in 1971, thinking back on that cloudless day in the New Mexico desert, "the tradition of lighting up a cigar had not yet begun. We contented ourselves by letting out whoops of joy, shaking hands, and patting each other on the back. Our celebration was of short duration. We quickly returned to work to prepare for the launching of the next round of the WAC the next day!" [687]

Malina's use of the term' astronautical,' rather than 'rocket,' is noteworthy. There is a distinct difference between rocketry and astronautics. Astronautics concerns itself with exploration of extraterrestrial space. Rocketry is the science of design and construction of vehicles with a self-contained power and oxygen source, operating on the reaction principle. In 1971, when Malina wrote these comments, astronautics was a well-entrenched field of science and engineering. But it is a vastly broader field than rocketry. Astronautics as

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687 Malina (1971), pp. 365-366.

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a scientific endeavor had not come to fruition in 1945. Malina's "slip-of-the-pen" reveals, at the least, an expanded vision of the fruits of his work. In truth, Malina and the WAC Corporal team had turned a page in the history of engineering.

Ironically, none of the other original members of the GALCIT Rocket Team was present when the WAC Corporal lifted off in 1945. John Parsons, the co-inventor of RFNA-aniline fuel, along with Malina, had left Aerojet that very year to return to independent work in the explosives industry. Parsons died tragically only seven years later, in 1952, at age 38. Hsueh-shen Tsien was working with von Kármán in Washington, and preparing to move to MIT; Not even Martin Summerfield, who had worked on the ORDCIT Project, was at White Sands that first September day. Four years later, in 1949, he would leave JPL for Princeton University, where he became a professor of Rocket Propulsion Technology.

Of the original GALCIT team, Malina alone stood and watched the successful tests of the WAC Corporal in 1945. His presence may be taken as a symbol, not only of the continuity of hard, determined work that had begun long before, in October 1936, in the deserted canyon known as the *Arroyo Seco*, but also of the Rocket Team's metamorphosis. As in 1937, the GALCIT Rocket Team drifted apart; but this time it was forever.

The WAC Corporal tests represent the maturation of American rocketry in the waning years of World War II. From an engineering standpoint, the WAC remains a remarkable achievement. Although it rested squarely on the effort of university-trained engineers, a research team organized by private

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industry and the financial and organizational contributions of the US military, developing the WAC was an effort without any input from an international community of scientists.

Like the V-2, the WAC Corporal was the product of a state-funded project, one which fused the collaborative work of industry, the army, and academia. Malina's 'WAC team' conducted all its research and directed construction completely in secret. As there was no chance of input from German engineers, the creation of the WAC Corporal was strictly an independent accomplishment. Malina's crew had come to its design decisions without the benefit of the only other intensive rocket-building project in the world: that of Nazi Germany. Malina never got a complete look at German technology during his trip abroad in 1944. He compared notes with British rocketeers, but they, too, were leery of divulging too much. Not even the amateur rocket societies in the United States were involved. So, the WAC Corporal was born entirely of Caltech and army know-how.

Perhaps, if Tsien had been a member of the WAC team, theoretical calculations describing the WAC's behavior in flight would have been even more detailed and accurate than they were. Nevertheless, Homer Joe Stewart's contributions gave Malina all the underpinning he needed to convince himself and the army that the WAC could fly. GALCIT and Aerojet engineers had confronted and mastered many theoretical and design problems in building their early rocket motors. Others remained to be solved (the problem of motor throbbing due to unstable burning, and the use of newer more sophisticated, more refractory materials in rocket motor casings, such as ceramics and titanium.)

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Unquestionably, the calculation of the WAC's performance could have been more precise. Because tracking capabilities at White Sands were rudimentary in October 1945, it was impossible to do more than estimate results. Had the WAC Corporal's motor been in powered flight for more than a few minutes, other mechanical and dynamic problems might well have surfaced. Yet, the WAC Corporal used proven technology, and within the limits of its mission, it performed quite well. In the WAC Corporal, GALCIT had created the first of a new generation of rockets. Its design mated theory to construction. By 1945, both GALCIT and American rocket technology had entered a new and challenging phase.

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Chapter 14

Conclusion

By January 1947 the GALCIT Rocket Team was only a memory in the minds of its founders. Its shadow lay over the still nascent Jet Propulsion Laboratory. The *Arroyo Seco* became the sprawling laboratory's permanent home. Until 1946, when he resigned as Acting Director of JPL, Frank Malina stayed on, along with GALCIT veterans Martin Summerfield, Walter Powell, and Homer J. Stewart. JPL grew and consolidated into a research arm of the US Army. On January 1, 1959, the National Aeronautics and Space Administration absorbed the Pasadena laboratory into its own organization.

The change from close affiliation with the military to incorporation as a branch of a civilian space agency proved no easy transition for JPL staff. Many grumbled about civilian inefficiencies and felt that their long commitment to the army had given them an edge—a knack for keeping to timetables, for innovation, for resourceful solutions to knotty technical problems—which only the pressure of military expectations could extract from researchers. NASA was born as a reconstructed National Advisory Committee on Aeronautics in July 1958, and JPL veterans looked down at NACA's bureaucracy with disdain. [688] Admittedly, NACA had been slow to realize the coming of jet propulsion technology in the late 1930s; perhaps in 1958, the civilian agency would again fail to seize the technological 'moment' and thus force JPL into a position of irrelevance. [689]

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688 Koppes (1982), p. 97.

689 Daso (1997), pp. 70-72.

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The Laboratory's third director, William Pickering (1910-), who had taken over when Louis Dunn (1908-1979) unexpectedly resigned in August 1954 to head the Atlas missile development project, was unafraid of the change to civilian administration. Pickering wanted JPL disengaged form pure military research assignments, and pushed to create a niche for the laboratory at NASA which suited its special qualities as a jet and rocket propulsion think tank. The Jet Propulsion Laboratory maintained its administrative link to Caltech, from which many of its engineering staff came, and the Institute continued to receive a management fee from NASA, even though administrative policy and research agendas really emanated from NASA headquarters in Washington.

Whatever JPL's new priorities, the laboratory still remained a hybrid of civilian academics and government administrators. Eventually, William Pickering's hope that JPL would become a "national space laboratory" bore fruit; JPL collaborated with the Army Ballistic Missile Agency in tracking the Explorer and Pioneer series of satellites in 1958 and 1959. Explorer was America's first orbiting satellite; Pioneer would be next, with its target, the moon. Pioneer 4 approached within 37,000 miles of the moon in March 1959. [690] Despite its sliding under NASA's umbrella in 1958, JPL's tie to the US Army could not be so easily severed. As World War II had instigated research into short-range missiles like the Private A, so the Cold War had intensified the pace of efforts to build larger and more powerful missiles, encouraged continued collaboration with the armed forces in satellite

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690 Koppes (1982), pp. 92-93. See also: Launius (1994), p. 36.

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deployment, and had even given JPL a central role in the establishment of a world-wide satellite tracking network.

That JPL and the army collaborated on space projects of value to US national security and prestige well after the end of World War II seemed a confirmation of Frank Malina's anxiety about the mating of the rocket and the security concerns of the national state. It was certainly true that JPL engineers had contributed to the development, not only of ballistic missiles like the Atlas, but plasticized solid propellants which could be stored indefinitely inside Polaris and Trident missiles whose launch pads were submarines gliding stealthily underneath the oceans.

But Frank Malina would have no part of this evolution of rocketry into a key arm of US national security during the Cold War. When he flew to Europe on another of his fact-finding trips for Army Ordnance in June 1946, along with stops in London and Paris, Malina took a side trip to visit his father's relatives in Czechoslovakia (October 1946.) His visit to the continent this time was an extended one, lasting until December. A full five months in Europe had put the stamp of reality upon his recent separation from his wife, Liljan. In July, he wrote his parents that he "had made contact with UNESCO and I am keeping my eyes open for a possible connection." [691] In September, the "slight possibility" of working for the United Nations had begun to gel in Malina's mind. He wrote home,

> If an appointment is available, it would be advisable I would have to request Caltech for an additional grant of leave, and I don't know how the Institute would like that. I am thinking over the whole situation very carefully. What do you think? Since this whole picture is very

691 Malina, Letters Home, 27 July 1946, p. 72.

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> uncertain, don't tell anyone about it. The job would pay about what I get at Caltech, and I think would offer a wonderful experience. [692]

Within two weeks of writing this letter, on September 17, UNESCO had offered Malina a job, working as a counselor in the Natural Sciences Division in Paris, under the renowned biochemist and historian of science, Joseph Needham. [693] He accepted by September 30. Malina's job, in his own words, was to study "ways to break down the barriers to the free movement of scientists and engineers between nations." [694] Malina remained with UNESCO until 1953, becoming Deputy Director of the Natural Sciences Division in 1948, and Head of the Division of Scientific Research in 1951. [695]

All in all, the move to Paris was surely bittersweet for Malina. On June 14, 1946, he had admitted to his parents that he and Liljan Darcourt were separating. Both Liljan and he had written previously, hinting at trouble between them and trying to cushion the blow. She wrote a personal note:

> Frank will leave for Pasadena Friday, but I am sure he'll be back on another trip before long. He has so much to think about in the way of future plans for his work that I don't see how he's going to make a choice of all the

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692 Malina, Letters Home, 2 September 1946, p. 73. Malina notified the Caltech administration of his intention to take a leave of absence from GALCIT in the fall of 1945. He expected to begin his leave the following June.

693 Malina, Letters Home, 30 September 1946, p. 75. Malina actually returned home to the United States in December, not only to prepare for his move to Paris, but to ask the advice of family and colleagues about the wisdom of making the move. He related in his memoirs that he discussed working for UNESCO with Albert Einstein and Vannevar Bush, even with the Senator from Texas, Lyndon Johnson. Only Johnson's reaction was lukewarm, saying he "was not acquainted with UNESCO intentions and that the United Nations was just a 'baby.'" Malina (1971), p. 341.

694 Malina (1971), p. 341.

695 Biographical Information Sheet, Frank Malina, provided by Mrs. Marjorie Malina. Personal Effects of Marjorie Malina, Boulogne, France.

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opportunities that have been offered him. [696]

Malina's acceptance of a position at UNESCO finalized their separation. The next year, in June 1947, he met Marjorie Duckworth through the UNESCO personnel office where she worked. They were married on March 12, 1949. Two children and several years later, Frank Malina had become, not only an artist, but member and later President of the International Academy of Astronautics (1963), and founder of his own journal, *Leonardo*, which he published from 1967 until his death in 1981, aged 69.

Malina's departure from the United States was difficult in more than one respect. Not only had his first marriage ended, but after nearly eleven years, he had to bid farewell to Caltech. This was not an easy break for him, despite his rancorous relationship with Clark Millikan and the stress of serving as JPL's first Director. "What troubled me most about leaving JPL," Malina confided in his 1971 memoirs, "was the separation from the members of the staff, many of whom were my closest friends and with whom I had shared many good and trying times. I have never again worked with a group that was as cooperative and enthusiastic." [697] Malina's years at GALCIT, Aerojet, and the Jet Propulsion Laboratory were, in a sense, his halcyon years. Peaceful, they were not, but golden and bright with promise, they most certainly were.

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696 Letter of Liljan Malina to Frank Malina's parents, 28 November 1945. Library of Congress, Malina Collection, Box 22.1.

697 Malina (1971), p. 340.

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Tragically, Malina's political associations as a youth in the late 1930s and early 1940s, his flirtation with communism, cost him dearly in the 1950s. A manifestation of his youthful idealism, and never a significant aspect of his career, his membership in the Communist Party caught up with him in the mid-1950s when the United States revoked his passport and forced him to seek refugee status in England, which then denied him protection. [698] Malina became a victim of the very world he had helped to create—a new era in which the guided missile, tipped with a nuclear warhead, had not only fostered a military standoff between his homeland and Russia, but had also contributed to an intense, yet elusive malaise permeating the very heart of American politics.

Malina was almost certainly not disloyal; he had worked long and hard for the United States government, from 1939 until 1946. Although he disliked the labyrinthine bureaucracy of the US military and the increasing administrative and travel demands put upon him from 1943 until 1946, Malina believed wholeheartedly in the cause of Allied victory over fascism, and contributed his expertise to the war effort unstintingly. After the war, Frank Malina, like many others, had to make painful readjustments to accommodate himself to the fearful time in which he lived.

The wide net of McCarthyism caught Martin Summerfield and Hsueh-shen Tsien, as it did Malina. In 1951, two years after leaving Caltech for a position as Professor of Applied Science at Princeton, Summerfield found himself the subject of a government investigation and lost his security clearance,

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[698] Email letter from Roger Malina, August 30, 1996.

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although he personally denied any involvement in politics. [699] Before resigning from Caltech and JPL in 1949, Summerfield made important contributions to the theory of rocket propulsion and improved upon the experimental work he had done on regeneratively cooled motors in the early 1940s. [700] In 1946, Summerfield suggested to Malina the idea of using the WAC Corporal as a second stage atop the nose cone of a captured V-2. Malina quickly adopted the idea and passed it along to Army Ordnance; the result was the 'Bumper WAC,' an early two-stage rocket that reached an altitude of 244 miles above the earth on February 24, 1949. [701]

In 1947, Malina and Summerfield collaborated on a study of how rockets could effectively escape from the earth's gravitational pull—this is known as reaching escape velocity. [702] As the editor-in-chief of the *Journal of the American Rocket Society* in the early 1950s, Summerfield published, not only his own work on rocket propulsion, but that of Tsien and others. [703] Summerfield taught at Princeton in the Department of Mechanical and Aerospace Engineering until 1977, when he retired to devote his energies to a small research company he had founded in Princeton, Combustion

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699 Hunley Interview with Summerfield (1994), p. 25, 28.

700 Hunley Interview with Summerfield (1994), p.11.

701 George S, James and Frank H. Winter. 'Early Educator-Supervised Student Rocketry; The GALCIT Rocket Research Project, 1936-39.' *Acta Astronautica, Vol. 10, No. 5-6*, p. 243.

702 Frank J. Malina and Martin Summerfield. 'The Problem of Escape from the Earth by Rocket,' *Journal of the Institute of Astronautical Sciences, Vol. 14, No. 8*, p. 471. The two engineers considered the use of 'step' or multistage rockets and nuclear energy as a source of heating hydrogen gas to the combustion point—two possible means of reaching escape velocity, or approximately 25,000 mph.

703 Martin Summerfield. 'A Theory of Unstable Combustion in Liquid Propellant Rocket Systems,' *Journal of the American Rocket Society, Vol. 21, No.5*, pp. 108-114.

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Dynamics. The Martin Marietta Corporation later purchased Combustion Dynamics. Summerfield died in 1997.

Of all the winding, convoluted paths that GALCIT Rocket Team members followed in the years after the group's dissolution in the late 1940s, none had more strange loops in it than Hsueh-shen Tsien's. In late April 1945 (along with Clark Millikan, Fritz Zwicky, Hugh Dryden, and other hand-picked members of a scientific team led by von Kármán), Tsien flew to Europe to investigate and report back to Hap Arnold on the current state of German aeronautics.

Tsien is alleged to have met with Wernher von Braun in the village of Kochel, Germany on May 5 and may even have requested that the German rocket engineer write a report on the prospects for German liquid propellant rocket development in post-war years. [704] However, there is no record of their meeting. In an April 21, 1951 *New Yorker* article on von Braun, the Nazi rocket scientist mentioned that he was questioned by a General Electric scientist, Richard Porter, as well as by Fritz Zwicky and Clark Millikan of Caltech, in the town of Garmisch-Partenkirschen. [705] Zwicky, still a Swiss citizen, would have been able to interview von Braun in his native tongue. Von Braun began to tell the American engineers about Peenemünde, and about his dreams for the future of rocketry. In September, von Braun flew to Fort Strong (near Boston, Massachusetts), and eventually on to the Aberdeen Proving Grounds in Maryland, and White Sands, New Mexico, where he

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704 Chang (1995), pp. 112-113.

705 Daniel Lang. 'A Reporter at Large: A Romantic Urge,' *New Yorker*, April 21, 1951, pp. 79-80. Library of Congress, Malina Collection, Box 20.11.

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continued to work alongside American engineers to reconstruct and launch crated V-2s shipped to the US by the army.

In 1945, Tsien was a trusted member of von Kármán's Scientific Advisory Group, assembled at Arnold's request to study European progress in air power—particularly jet aviation, supersonic flight testing, and rocketry. *Where We Stand*, the group's collaborative report, first appeared on August 22, 1945, and proved to be an influential study of German, British, and American developments in air power. Hap Arnold utilized this report to accelerate the pace of American aviation research and define future objectives for the Army Air Force. [706]

Tsien was in good company as part of the Scientific Advisory Group; many of his Caltech colleagues, Hugh Dryden, William Pickering, and Lee DuBridge (1901-1993) among them, served at his side. Even though Tsien was, in 1945, living and working on a student visa, the Immigration and Naturalization Service granted him a special visa extension so he could return to the US without incident after his European tour. [707] He seemed to be riding on a wave of trust, approbation and prestige, some of it reflected from his chief, von Kármán—that year. In 1945, Tsien also worked on a key blueprint for the Army Air Force's future, along with von Kármán and the SAG; *Toward New Horizons* (December 15, 1945) became the model for the development of the new independent air force which Arnold hoped would come out of the end

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706 Von Kármán. *Where We Stand. A Report Prepared for the AAF Scientific Advisory Group, August 1945*. Wright Field, Dayton Ohio, published by Headquarters, Air Material Command, Publications Branch, Intelligence T-2, May 1946. Washington, NASA History Office, Lek # 8/4/8, item # 2483. The entire text appears in Daso (1997), pp. 212-301.

707 Chang (1995), pp. 111-112.

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of the Second World War. [708] For the next two years, Tsien studied and taught at MIT, where he had originally gone after arriving from China in 1935. In 1949, he returned to Caltech with a new wife and a new position, as Robert Goddard Professor of Jet Propulsion, a tenured post. It looked as if his career would only rise.

Then, in 1950, Tsien came under suspicion of having been a member of Pasadena's Communist Party cell—Unit 122—during the late 1930s; on August 25 he was arrested. Tsien had considered returning to China that year, one year after the Communist victory over Chiang Kai-shek's Nationalist forces; but the INS, fearing that he would sail for China with the fruits of his research in his baggage, locked Tsien in a detention cell on Terminal Island, San Pedro, California, for fifteen days. [709]

Released to his home in Pasadena with the help of Caltech officials, awaiting imminent deportation, Tsien spent the next five years in an agonizing limbo. He consistently denied ever having been a communist and fought, with the help of Caltech administrators like Lee DuBridge, the Institute's first president after the retirement of Robert Millikan, against the INS deportation order. [710] Finally, after years under virtual house arrest in Pasadena, allowed

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[708] Daso (1997), pp. 313-429. The core of *Toward New Horizons, the Key to Air Supremacy,* appears in its entirety in Daso's work. Tsien made several contributions to this report, writing supplements on high speed flight, ramjet engines, atomic energy as a power source for airplanes, and "the launching of a winged missile for supersonic flight." See pp. 305-306. Another supplement, "Future Trends in the Design and Development of Solid and Liquid Fuel Rockets,". Vol. IV-4 of *Toward New Horizons,* is available through the NASA History Office, Washington, Item No. 400.426 (26 pages). The US Air Force was created by Congress on September 18, 1947.

[709] Chang (1995), pp. 161-164.

[710] Millikan had accepted the title of Chief Executive of the Caltech Executive Board, but

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only to travel back and forth between his home and the campus, the US government relented and allowed Tsien to leave for China on September 17, 1955. [711]

Within a year, Tsien was busy organizing the Institute of Mechanics in Beijing and the Fifth Academy of National Defense, where China's medium and long-range missiles began to take shape. The *Dongfeng* (East Wind) series, which began modestly enough as a model patterned after a Soviet prototype, developed into the DF-5 and DF-6, both intercontinental ballistic missiles. Tsien also led the construction of the Silkworm anti-ship missile. This missile, sold in quantity to Iraq, was trained on American ships during the 1991 Persian Gulf War. The life of Hsueh-shen Tsien had come full circle. He had returned to his homeland from his adopted nation, America. His departure from the United States was a study of quiet agony; his arrival in China, one of triumph. More than fifty years after coming to the US, he could look back at a lifetime of accomplishments—and at the fact that he had designed missiles capable of attacking, not only the American military, with whom he had worked so assiduously in the 1940s, but American cities, as well.

The same, of course, could not be said of Edward Forman and John Parsons. Forman died of a heart ailment in 1973. Both he and Parsons cut their ties with JPL in 1943. Not long afterward, Forman sold his stock in Aerojet to Frank Malina and to the General Tire and Rubber Company. Malina

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was never the Institute's 'president.'

711 Chang (1995), p. 199.

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purchased 111 shares from Forman in 1944 for $11,000. [712] At the time, General Tire and Rubber was negotiating to purchase fifty percent of Aerojet's outstanding shares from the company's founders. Only Malina and Clark Millikan held out and refused to sell. This proved a wise move, as the value of the new stock, Aerojet-General, soared in the 1950s. [713]

Forman invested his money in a variety of business ventures, but did not fare well. He later (1953) worked for Hughes Aircraft and in 1966, Lockheed hired him as an engineering specialist. Despite many difficulties in his later life, Forman achieved a measure of renown. He had come far from the days in the late 1930s and early 1940s when he gripped a welder's torch to piece together rocket casings and drove a bulldozer to clear land in the *Arroyo Seco* for the Rocket Team's experiments. On October 31, 1968, thirty two years after the first liquid propellant motor tests in the dusty *Arroyo*, Forman was honored, along with Frank Malina, 'Amo' Smith, and others, for their work in establishing JPL. [714] Without so much as a baccalaureate, Edward Forman had been a rocket builder, explosives expert, and missile designer for a major corporation (Lockheed.) While not a mathematician with the talent for calculating thrust potential or rocket trajectories like Frank Malina or Homer J. Stewart, Forman played a key, even invaluable, role in the birth of modern American rocketry.

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712 Letter, Edward Forman to Frank Malina, September 20, 1944. Library of Congress, Malina Collection, Box 16.13.

713 Von Kármán (1967), pp. 316-319.

714 James and Winter (1983), pp. 244-245. Forman actually brought the original liquid propellant motor he had helped construct in 1936 to the 1968 JPL ceremony.

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John Parsons, mercurial inventor, chemist without a degree, the man who would not live to see the impact of his key creation, the composite propellant, on rocketry and military history, left the scene of his triumphs by 1946. In that year, he quit Aerojet to work at the Vulcan Powder Company in Pasadena; he later served as an expert in rocket dynamics at North American Aviation, and at Hughes Aircraft. Parsons lost his position at Hughes during the FBI security investigation he endured there in 1950. Despite his impetuous and ill-considered actions in removing secret documents from the Hughes factory, Parsons came away from the FBI probe unscathed. After toying with the idea of exporting his talent as an explosives expert to Israel, a venture which never materialized, Parsons built 'special effects' pyrotechnic devices for the movie industry in Los Angeles one year before his death.

From his adolescence on, John Parsons was rarely far from explosives. Following the spirits he liked to "commune with," as Malina once quipped, Parsons was packing to ship his chemicals down to Mexico, where he anticipated working in a explosives plant, when he dropped the fulminate of mercury cap that killed him on June 17, 1952. Although Malina, Summerfield, indeed, the entire rocket engineering community acknowledged that Parsons had changed the face of solid propellant rocketry with the invention of GALCIT- 53, it seems that Parsons himself never looked back. Surely, he knew the significance of his creation. Parsons must have occasionally admitted to himself that GALCIT- 53 was an important discovery, even after Charles Bartley eclipsed his asphalt-based mixture with

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the plasticized propellant (1945) which became the prototype for all modern solid propellants. [715]

Outside the relatively small worlds of rocket propulsion engineering and black magic, John Parsons remained little known. He lived an adventurous life, rarely respected conventional mores, proved difficult to control at times; yet few people who knew him actually rejected him. Malina and von Kármán, for all the exasperation Parsons brought them, still regarded him with a kind of wry tolerance and even reverence. They sensed his brilliance and let him work. Had Parsons survived a decade beyond 1952, he probably would have wanly smiled at the reality of the world he had helped create, a jittery world armed with missiles powered by composite propellants. But he would have had few misgivings about his accomplishments. That was John Parsons, the man who thrived in a dangerous environment, questioned the 'rules of the game,' and yet, through his creative insight, brought the world closer to space and to nuclear endgame.

The quiet man of the GALCIT Rocket Team, Apollo Milton Olin Smith, rarely asked that the spotlight be focused on him, although his 1937 and 1938 collaboration with Malina on sounding rocket flight dynamics helped establish rocketry at Caltech on firm ground. After leaving Aerojet in 1943, Smith worked at Douglas Aircraft for the remainder of his professional life. There, he designed both military and civilian aircraft, including jet fighters, from the 1940s until 1975. Smith continued to live in southern California, where he had begun his aviation career as an amateur glider builder in the

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715 Parsons held several patents on his inventions, some of them jointly with Frank Malina. These patents include: US patents #, 2,573,471, 2,693,077, and 2,774,214 (originally filed May 8, 1943.) James and Winter (1983), p. 249.

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1920s. Only reluctantly would Smith have admitted that his work contributed to the emergence of modern rocketry and jet aviation; he was too humble a man to publicize his role. Yet his scrapbooks, loosely bound and yellowed with age, filled with photographs and drawings of the several aircraft he designed, stand as mute testament to his contributions. [716] 'Amo' Smith died in the summer of 1997.

That the GALCIT Rocket Team was destined to fade and merge with the current of history during the tumult of World War II was presaged by the *de facto* withdrawal from the Caltech scene of the Guggenheim Laboratory's director, Theodore von Kármán. By 1944, von Kármán was far more present in Washington, or overseas, in liberated Europe, than he was in Pasadena. This doer and thinker, one of the twentieth century's most influential scientists, had committed himself to the growth of aviation, and had chosen the most effective route to that end in 1938—collaboration with the military.

Von Kármán, whose friendship with Hap Arnold was destined to shape the direction, not only of GALCIT rocket research, but of American air power after 1945, saw himself as the champion of innovation in aviation, and astutely realized that he could best accelerate technological breakthroughs by allying aeronautical research with the armed forces. Of course, there was resistance to change among the military, just as in civilian ranks. during the Second World War. It was not only Vannevar Bush and NACA's George Lewis who failed to appreciate the potential of rocket technology; it was also

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716 Smith was gracious enough to spend several hours at his home with this researcher in July 1996, detailing his life and work, and displaying his scrapbooks, filled with his airplane designs, as well as honors he had received from Douglas Aircraft.

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military officers, such as Benjamin Chidlaw, who could not envision rocketry as anything but fantastic.

In *Endless Frontier: Vannevar Bush, Engineer of the American Century*, G. Pascal Zachary noted that von Kármán saw Bush as something of a rival, and intended to upstage his efforts at civilian control of aeronautical research. [717] This was quite true, for the Hungarian disagreed profoundly with the essence of Bush's strategy and with his shortsighted attitude toward rockets. Von Kármán did not have unshakable faith in the progressive influence of technology upon society, as did Bush, for he had seen its devastating consequences in German hands. (In all fairness to Bush, he, too worried about the impact of nuclear bombs on science and society.)

Instead, von Kármán believed that it was necessary to finesse the growth of applied science, while pushing ever so gently, usually through personal contact, applying his prestige as a lever against the resistance inbred among 'organization men.' Von Kármán knew that politicians and average men bore within them latent suspicions about the power of technology. He worked around this natural resistance through men like himself (Hap Arnold, in particular), who had the vision to transform their chosen profession and the energy to forge ahead as soon as they possibly could.

In the realm of aeronautical engineering, von Kármán's decision to ally himself with Arnold and work hand-in-hand with the military was perspicacious. He could have new technologies adopted quickly, and, in this

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[717] G. Pascal Zachary. *Endless Frontier: Vannevar Bush, Engineer of the American Century*. New York, Free Press, 1997, p. 228.

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sense, he did outflank Vannevar Bush. Among military officers, Bush was often seen as 'that civilian intruder;' von Kármán, on the other hand, was one scientist who was on the inside. [718] Of course, Bush also, at least nominally, oversaw the Manhattan Project, and with the merging of civilian and US War Department research in the post-war years, the issue of whether military or civilian direction of technology would win out became a moot point.

In *Toward New Horizons* (December 1945), von Kármán was able to make a very strenuous and detailed case for close collaboration between the Army Air Force and scientific research. Beyond stressing the well-understood importance of radar, and new technologies such as "rocket-driven airplanes", and "counter-measures against missiles," von Kármán wrote:

> Scientific planning must be years ahead of the actual research and development work. Long-range planning should be the responsibility of the Commanding General of the Air Forces....It is advisable that a permanent Scientific Advisory Group, consisting of qualified officers and eminent civilian scientists, should be available to the Commanding General....This group should contain experts with broad experience in the various branches of science involved. [719]

Von Kármán was only practicing what he had lived: for years, he had closely allied his GALCIT Rocket Team to military objectives.

In 1949, von Kármán officially resigned as GALCIT's director and devoted himself to expanding the horizons of aeronautical technology, all within a

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[718] Zachary (1997), pp. 181-182.

[719] Von Kármán, *Toward New Horizons*, Section 14.1, 'Long-Range Planning,' in Daso (1997), pp. 420-421.

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military framework, by organizing AGARD, the NATO Advisory Group for Aeronautical Research and Development. [720] Out of AGARD came collaboration on the design and deployment of fighter aircraft for NATO, and even a flickering of a new sense of unity in what had been a divided Europe—the first low-flying night fighter was built, not by British or French aeronautical firms, but by Italy's Fiat. [721]

Theodore von Kármán continued to support and conduct research into aeronautics and astronautics throughout his life, which ended in May 1963. On February 18, 1963, he received the first National Medal of Science from President John F. Kennedy. In the years between 1946 and 1965, von Kármán published well over 65 scholarly articles on aeronautics, fluid dynamics, nuclear energy, and space propulsion systems, among other topics. [722] One of his many singular achievements, of course, was his foresight in envisioning a role for rocketry in the world of flight.

Von Kármán's efforts at altering the original intent of the GALCIT Rocket Team's work, from pure research on rocket propulsion to the creation of military rockets, was a largely conscious effort to improve the professional, and later, the financial prospects of his rocket crew at Caltech. There was no sinister motive involved; von Kármán truly foresaw that rocketry would become an independent field, a new tree born of an aeronautical seed, and that it represented a profoundly exciting development in engineering. He,

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720 Von Kármán (1967), pp. 325-334.

721 Von Kármán (1967), pp. 330-331.

722 A complete bibliography of von Kármán's works may be found on pp. 355-361 of *the Wind and Beyond.*

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Frank Malina, Hsueh-shen Tsien, Mark Mills, Homer Stewart, all these men grasped the inherent beauty of their theoretical work, and yet hoped, beyond all, that they could successfully build rockets.

Malina's WAC Corporal sounding rocket flew, with the direct help of US Army funds, manpower, and support services, and this fact symbolizes an inevitable, if not disturbing transformation which took place between 1936 and 1945. The technology of rocketry could not be de-coupled from the political and military forces dominating the era of the Second World War.

Initially, Malina's training and perspective on scientific inquiry grew, not only from his undergraduate experience at Texas A&M, but from his years at Caltech, under von Kármán and other engineers who taught him the primacy of theory in the construction of new machines. Over eleven years of hard work and trial, what had begun as his teacher's expectations merged with the complex demands of a society which, step-by inexorable step, imperceptibly changed the idealistic goal of pure research into work for the war machine.

Malina, and for that matter the entire GALCIT rocket crew, may have occasionally doubted the wisdom of building military rockets; still, they knew viscerally that their country was in a fight to the death with an enemy intent on subjugating the world. The men usually submerged their longing to free themselves from organizational pressures or from the grip of individuals with little sympathy for the rigors of pure research. It was experiment, succeed, or fade away.

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The theory of rocketry was a narrow realm, inhabited by a relatively few men whose equations the military had little concern for. On the other hand, the technology of rocketry was the stuff of tactics, of gaining the edge over the enemy and of arsenals. The implications are clear: the technology of rocketry at Caltech in 1946 retained the essential features of its 1936 origins, but had taken on layer upon layer of organizational complexity, had fastened the sinews of governmental oversight to its skeleton, and had fundamentally transformed itself. Scientific ideas may obey an innate logic that is independent of social expectation, but technology is the child of society. Since science is a human endeavor, and mathematics a human language, the utterance of new ideas must be conditioned by the social institutions which humans fashion. Ideas are a product of symbolic logic and imagination, while technology is an expression of the human will to power.

From inception to fruition, the GALCIT Rocket Team evolved through several metamorphoses. Starting as an ostracized clan of graduate students and tinkers, graduating into recipients of National Academy of Sciences grants, and finally as founders of a large complex organization whose very sinews were composed of military men and academics tightly bound together, Frank Malina's crew etched out a remarkable story. Beyond making seminal contributions to the science and technology of rocketry, the GALCIT Rocket Team unwittingly strengthened the military hand of post-war American governments in a disturbingly unstable world. In truth, the mating of rocketry to the massive organization of the nation-state, whether civilian or military in nature, was inevitable. No individual could have effectively

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produced so complex a machine on the scale its very invention demanded. The rocket is a machine destined to shatter boundaries.

The liberating dynamic inherent in rocketry derives from just this very truth: rockets have been designed, whether for good or ill, to leave the world behind. A dream motivated the rocketeers who worked at Caltech's Guggenheim Aeronautical Laboratory, the very dream of breaking terrestrial bonds. The dream, however, manifested itself in a process, the growth of a new science which young engineers like Malina, Tsien and Summerfield believed depended upon the construction of a pre-eminently theoretical base. For these men, theory had to form the underlying definitive structure of the new science, or rocketry would remain, as it had for too long, the province of amateurs.

However, Theodore von Kármán's fervent belief that theory must precede successful experimentation proved incorrect. Theory did provide guidelines and insights into rocket performance, but was, in the final analysis, the servant of experiment. For all its limitations, not the least of which is the distinct possibility that an experiment may actually distort the very process it is designed to illuminate, experiment still lies at the very core of rocket science. Rocketry is far more an experimental than a theoretical endeavor.

The shortcomings of theoretical investigation of rocket motor function lie in theory's reliance on mathematics, which, to date, can only approximate actual conditions inside the combustion chamber. To a significant extent, what the study of combustion turbulence indicates is that there can never be full synchronicity between the mathematical description of physical events and

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their sensible manifestation. This is true because the language of mathematics, like any language, has varying levels of precision. So far, there remains a disjunction between mathematical statements and complex physical phenomena. It has been the belief of some physicists that the essential truths of physics may be expressed in the simplest, most beautiful mathematical statements, and that, somehow, the human mind will eventually penetrate to the deepest levels of creation and reveal its penultimate beauty. This is not the case. For, in unfolding the dynamics of rocket combustion, it becomes clear that each new layer of complex physical events serves as a transition to another, deeper layer.

There is potentially an infinite number of combinations for independent mathematical symbols, as there is a seemingly infinite number of ways in which any class of discrete symbols may aggregate to express new meaning. However, the grouping of symbols into statements limits the ways in which any language may express ideas or events. Mathematical statements, as sentences, form a series of finite sets. To enter a new set, one must re-arrange or invent symbols in order to create the transition to a new reality. Only by continually re-creating the language of mathematics can scientists hope to approach more closely the deep essence of the physical universe.

This is why, with all their faith in the power of mathematics to change the dream of rocketry into reality, the engineers of the GALCIT Rocket Team had to accept the truth John Parsons so eloquently expressed through his nerve-wracking, yet disciplined experimentation. To reach the stars, one must first begin with some very unpleasant explosions.

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Appendix I
Charts, Diagrams and Photographs

Diagrams of the design of GALCIT-built rocket test stands, motors, and one actual rocket—the WAC Corporal—will help visualize the Rocket Team's accomplishments. Three such schematics appear in the pages to follow. Certain diagrams included in Frank Malina's Ph.D. Thesis, *Characteristics of the Rocket Motor and Flight Analyses of the Sounding Rocket* (1940) also may be found here. The reader is encouraged to refer to specific page numbers in the text for further information. The "Four Differential Equations That Describe the Operation of an Ideal Solid Propellant Rocket Motor," given by Theodore von Kármán to Frank Malina for solution, in 1939, come directly after. Finally, two sketches of the WAC Corporal, showing the rocket's internal components, appear on pp. 517-518.

Schematic diagram of test setup similar to one used by GALCIT rocket research group on 11 November, 1936. (labeled as figure 3.)
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Schematic of the Early GALCIT Solid Propellant Motor Test Stand (1939-1940)
page 509

Drawing of the 25 lb. Thrust, 12 Second Solid-Propellant JATO Unit Used in the Ercoupe Flight Tests, August 1941. (labeled as Fig.11a)
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Figure 3 from Frank Malina's Ph.D. Thesis, *Characteristics of the Rocket Motor and Flight Analysis of the Sounding Rocket* (page 27 of his thesis.) η_{th} is a measure of the work done by the burning gas, as a function of its available heat capacity. Exhaust velocity, *c*, is measured on the abscissa (x-coordinate) of this graph. Note that when *c* peaks at more than 10,000 ft/sec., η_{th} approaches unity, or maximum thermal efficiency. At a gas exhaust velocity of 5,000 ft./sec, the gas' thermal efficiency is rated at only 20% of its theoretical maximum. *Refer to p.276, above.*

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Figure 6 from Frank Malina's Ph.D. Thesis, *Characteristics of the Rocket Motor and Flight Analysis of the Sounding Rocket* (page 30 of his thesis.) Angle θ equals the angle indicating the part of the nozzle through which exhaust gases flow fastest. Angle α is the angle of expansion of the nozzle, roughly approximating the radius. $d\theta$ approximates the expansion increment of the gas, as it flows toward the exit rim of the nozzle. Malina envisioned this increment as if it were a series of concentric circles, radiating out from the center. R is the length of the nozzle, as measured from the throat, and R sin θ is the trigonometric equivalent of the radius of the of the nozzle, measured as a function of angle θ, at the nozzle's rim. f_e and $f_{e'}$ signify the characteristic flow of the exhaust gas at the nozzle rim, and at a hypothetical spherical cross-section of the nozzle, also at the rim. Malina used these terms to measure the mass and velocity of the exhaust gas in the nozzle. *Refer to pp. 288-290 above.*

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Figure 8, Universal Ideal Thrust Diagram from Frank Malina's Ph.D. Thesis, *Characteristics of the Rocket Motor and Flight Analysis of the Sounding Rocket.*In this graph, Malina has measured thrust as a ratio of pressures, p_o/p_c, corresponding to the relation between atmospheric pressure, p_o, and combustion chamber pressure. γ equals the ratio of heat capacities of the propellant and oxidizer, and λ equals the angle of divergence of the nozzle, measured from the throat to the rim. f_x/f_t is the expansion ratio of the gas, that is, the amount of gas flowing by the nozzle exit, as a function of its mass

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at the throat. Note that the larger the ratio, f_x/f_t, the greater the amount of gas flowing out the nozzle, and the smaller the ratio, p_o/p_c. When the pressure ratio is near 0.001, the atmospheric operating pressure is far lower than the chamber pressure. At this point, the rocket is essentially operating in a vacuum, at maximum efficiency. *Refer to pp. 298 -299 above.*

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Figure 2. Variation of the Ideal Cycle Efficiency, η_i, with the Pressure ratio, p_c/p_e. from Frank Malina's Ph.D. Thesis, *Characteristics of the Rocket Motor and Flight Analysis of the Sounding Rocket* (page 26 of his thesis). Again, γ, the ratio of heat capacities of the propellant and oxidizer, is near unity (1.2). As the chamber pressure, p_c, increases markedly over the exhaust pressure, p_e, in the rocket flight's initial moments, the fuel burns hottest, does the most work, and consequently, the rocket has the maximum thrust. *Refer to p.p 297 -298 above.*

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Figure 4. Nozzle Cross Section, from Frank Malina's Ph.D. Thesis, *Characteristics of the Rocket Motor and Flight Analysis of the Sounding Rocket* (page 28 of his thesis). p_t, p_x, p_e, are all gas pressures at various cross-sections of the nozzle. p_o is the atmospheric pressure at the altitude at which the rocket is operating. d_t, d_x, and d_e are diameters of the nozzle at corresponding pressures, (as well as the approximate masses of gas flowing through the nozzle at those specific points); x is an arbitrary distance measuring the area at the entrance to the nozzle throat. Angle α, again, is the angle of expansion of the nozzle. *Refer to pp. 286-288 above.*

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Figure 10. Four Differential Equations That Describe the Operation of an Ideal Solid Propellant Rocket Motor., from *Frank Malina, The US Army Air Corps Jet Propulsion Research Project No. 1, 1939-1946: A Memoir, p. 166. Refer to pp. 13-14, p. 177 and 340-342 above.*

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***Sketch,* WAC Corporal.** (Courtesy of Jet Propulsion Laboratory).

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WAC B sketch, 12-2-47. (Courtesy of Jet Propulsion Laboratory).
page 518

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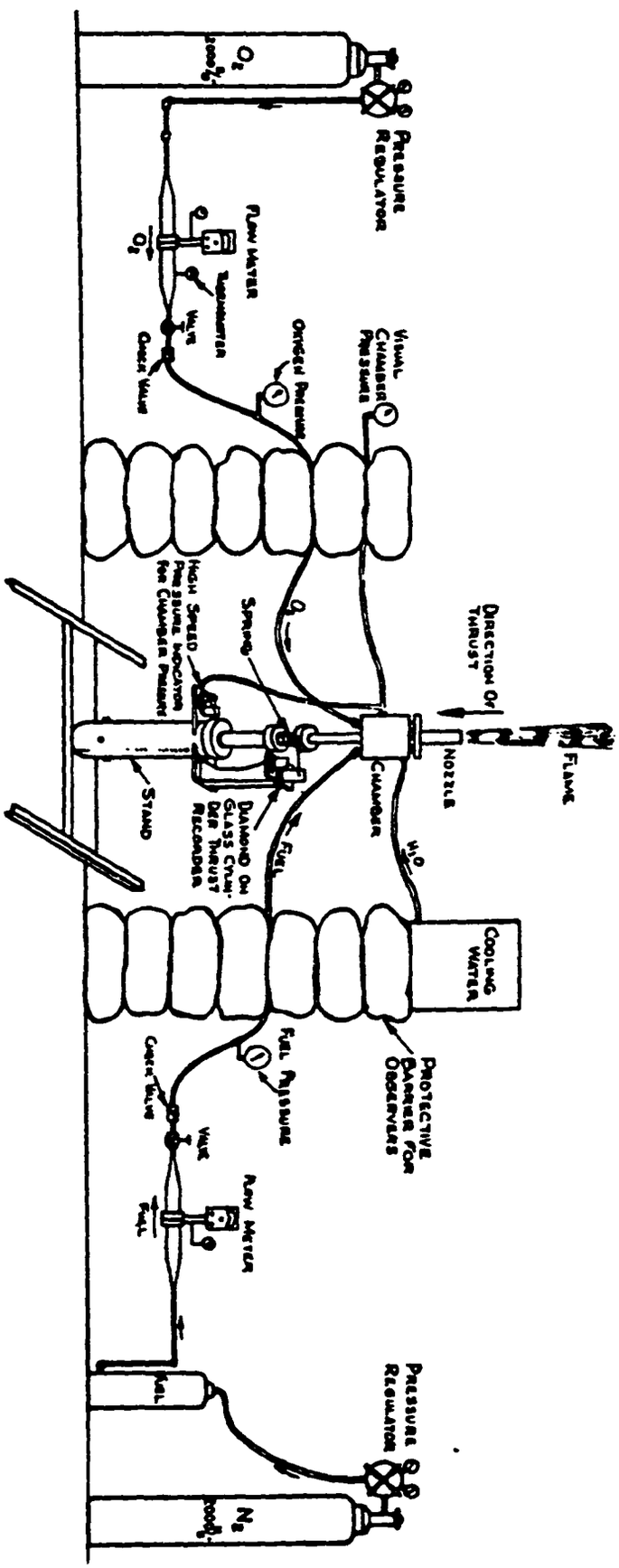
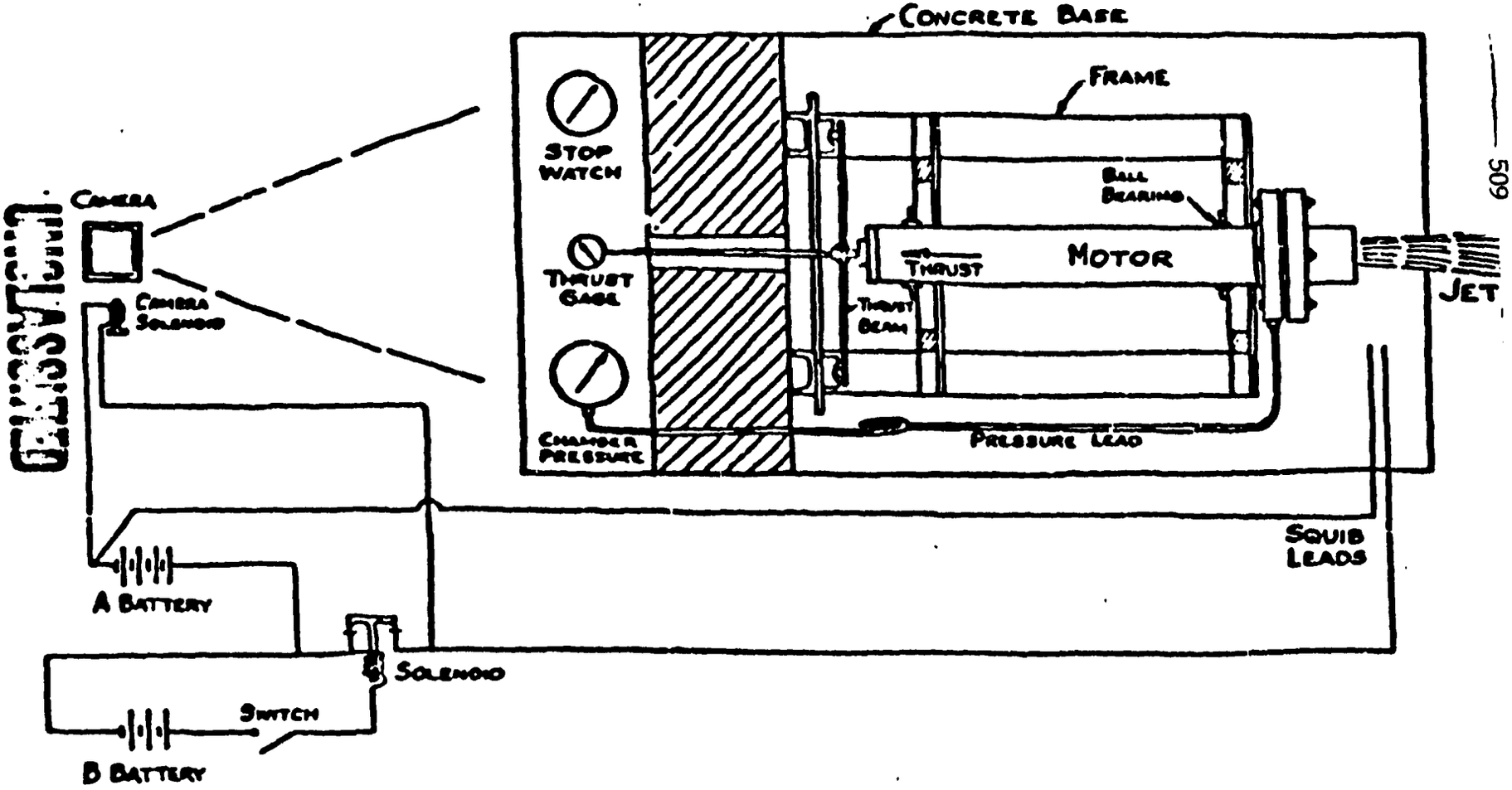


FIGURE 3.—Schematic diagram of test setup similar to one used by GALCIT rocket research group on 11 November 1936. Propellant was gaseous oxygen/methyl alcohol.

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Schematic of the Early GALCIT Solid Propellant Motor Test Stand (1939-1940)

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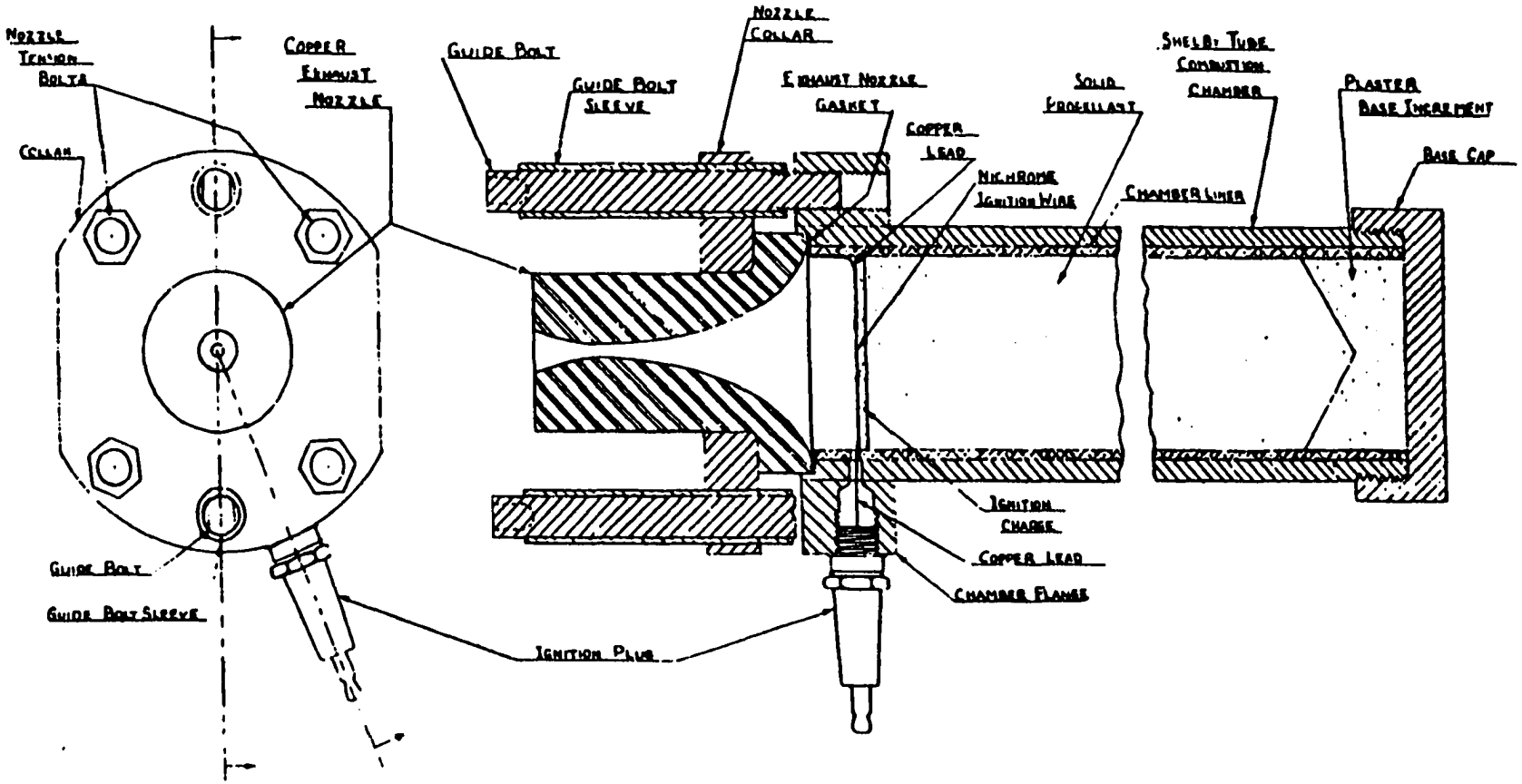


Fig. 11 (a)
Drawing of the 25 Lb. Thrust, 12 Second
Solid-Propellant JATO Unit Used In
The Ercoupe Flight Tests,
August 1941

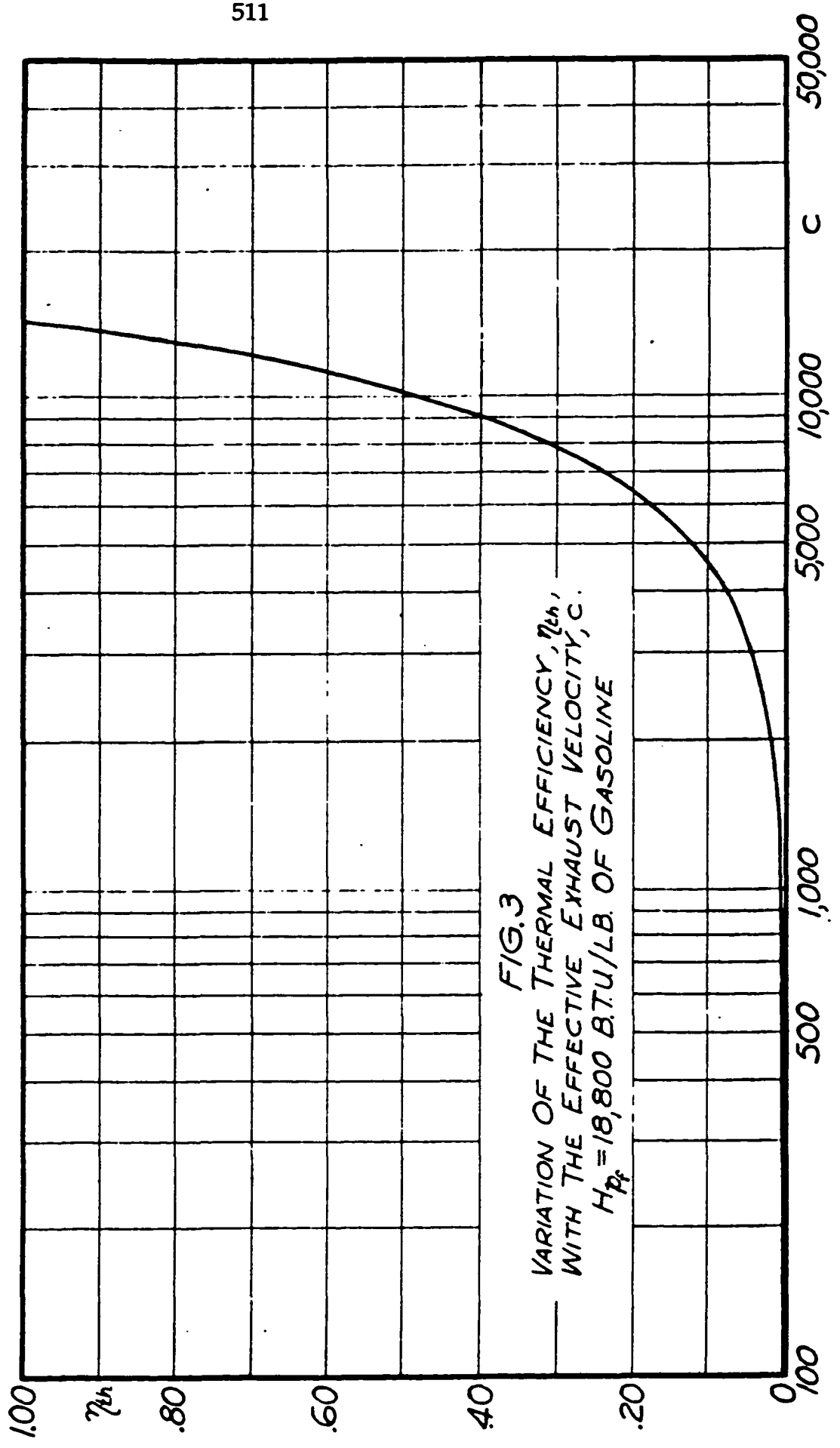


FIG. 3
VARIATION OF THE THERMAL EFFICIENCY, η_{th}, WITH THE EFFECTIVE EXHAUST VELOCITY, C.
H_{P_f} = 18,800 B.T.U/LB. OF GASOLINE

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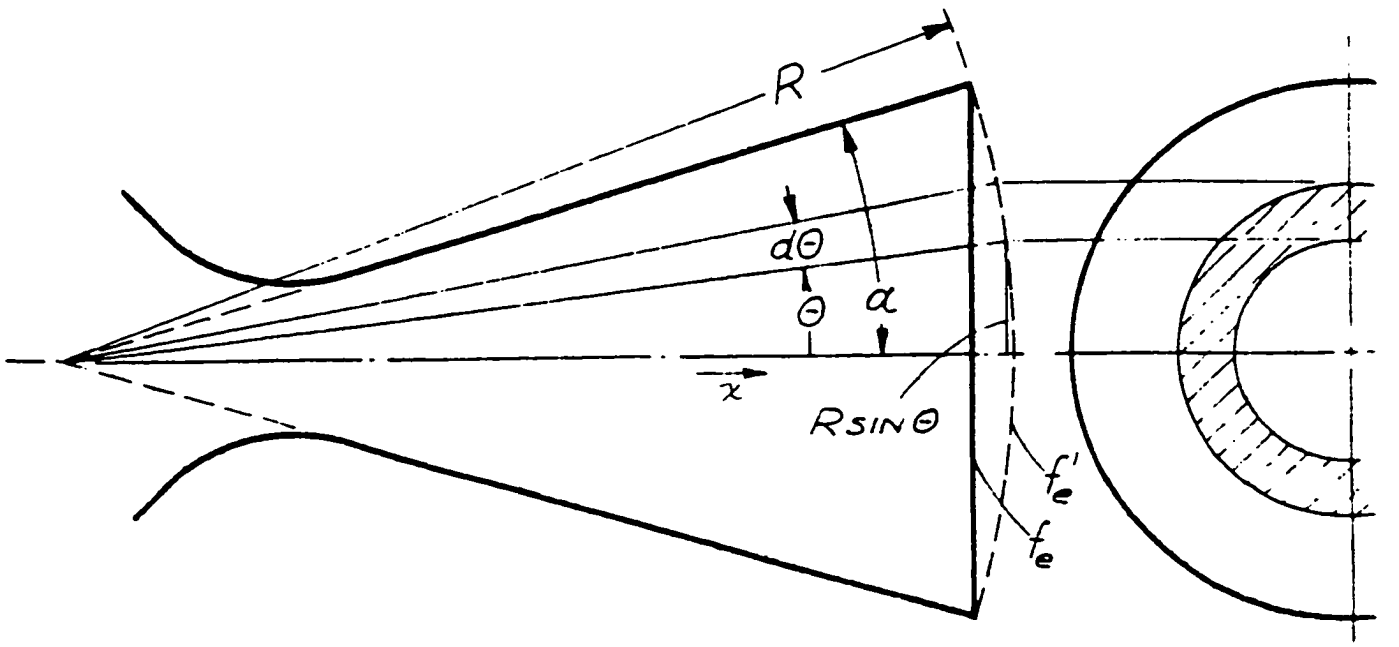


FIG. 6

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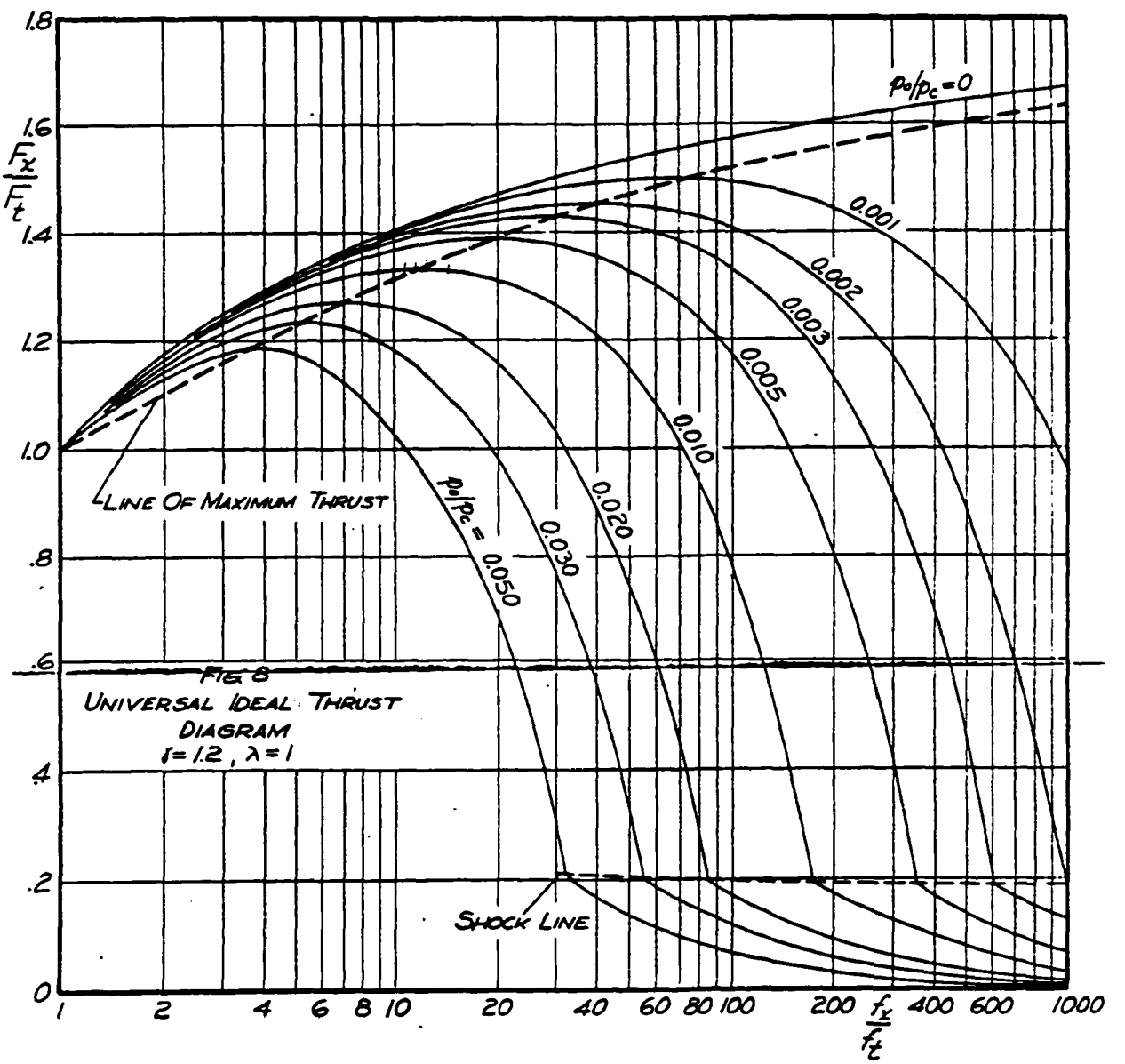
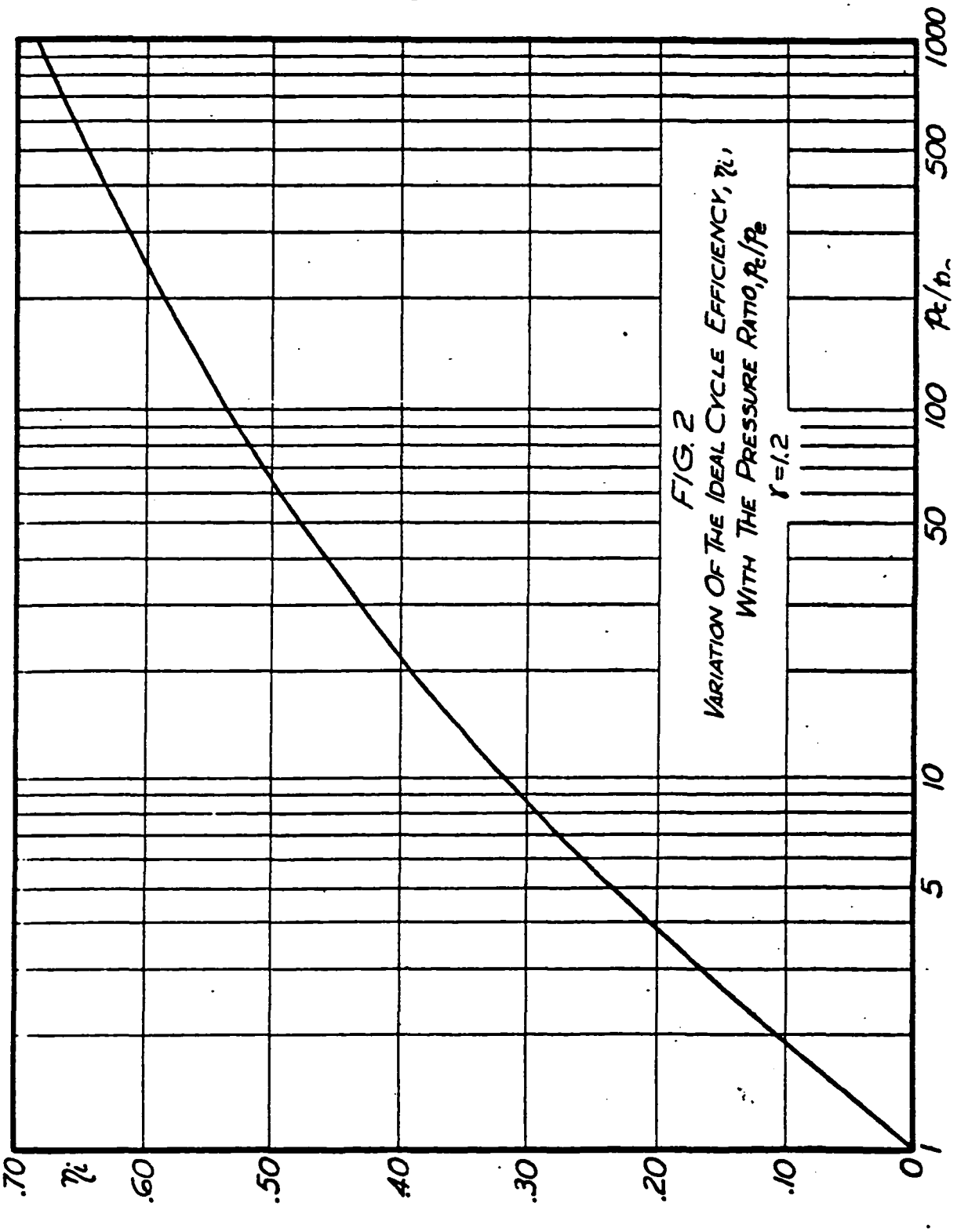


FIG 8
UNIVERSAL IDEAL THRUST DIAGRAM
$\gamma = 1.2$, $\lambda = 1$

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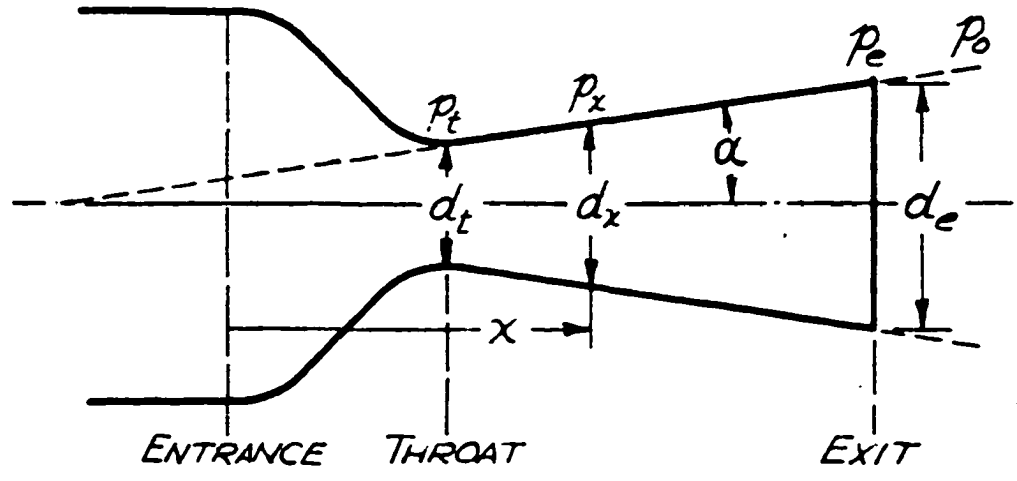


FIG. 4

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$$\text{I.}\quad m_{\dot{p}} = \Gamma f_t \frac{p_c}{\sqrt{RT_c}} + \frac{dm_c}{dt}$$

$$\text{II.}\quad m_{\dot{p}} = \frac{\rho_p R T_c}{p_c}\left(\frac{dm_c}{dt} - \frac{m_c}{p_c}\frac{dp_c}{dt} + \frac{m_c}{T_c}\frac{dT_c}{dt}\right)$$

$$\text{III.}\quad m_{\dot{p}} = \frac{m_c}{T_c - T_a}\frac{dT_c}{dt}$$

$$\text{IV.}\quad m_{\dot{p}} = (k + k_1 p_c) f_c \rho_p$$

where

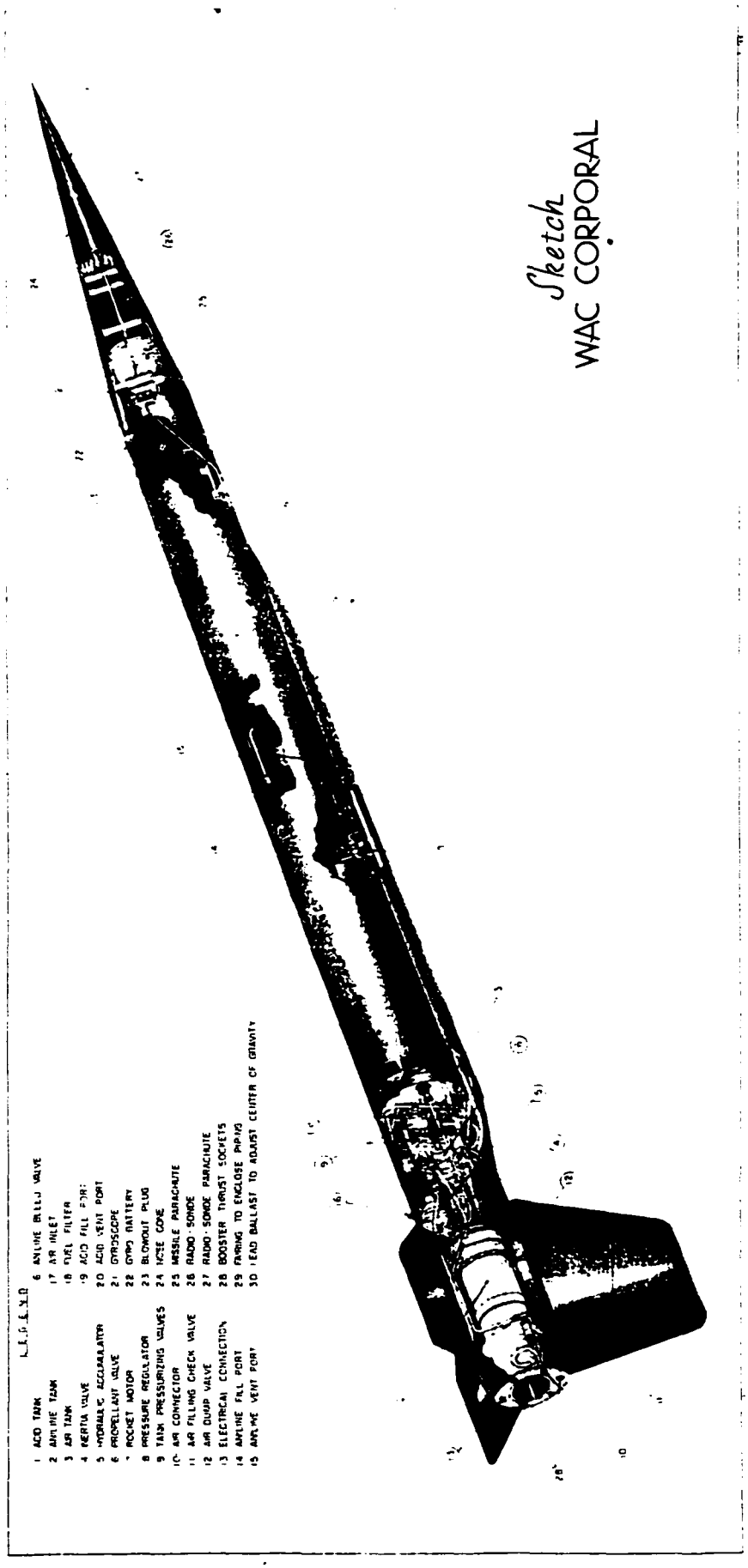
f_c = burning area of propellant
f_t = area of exhaust nozzle throat
k = propellant rate of burning coefficient
k_1 = propellant rate of burning coefficient
$m_{\dot{p}}$ = mass of propellant burned per second
m_c = mass of gas in combustion chamber
p_c = combustion chamber pressure
R = universal gas constant
t = time, seconds
T_a = maximum possible combustion temperature
T_c = temperature of gases in combustion chamber
γ = ratio of specific heats of gas

$$\Gamma = \left(\frac{2}{\gamma+1}\right)^{(\gamma+1)/2(\gamma-1)}\sqrt{\gamma}$$

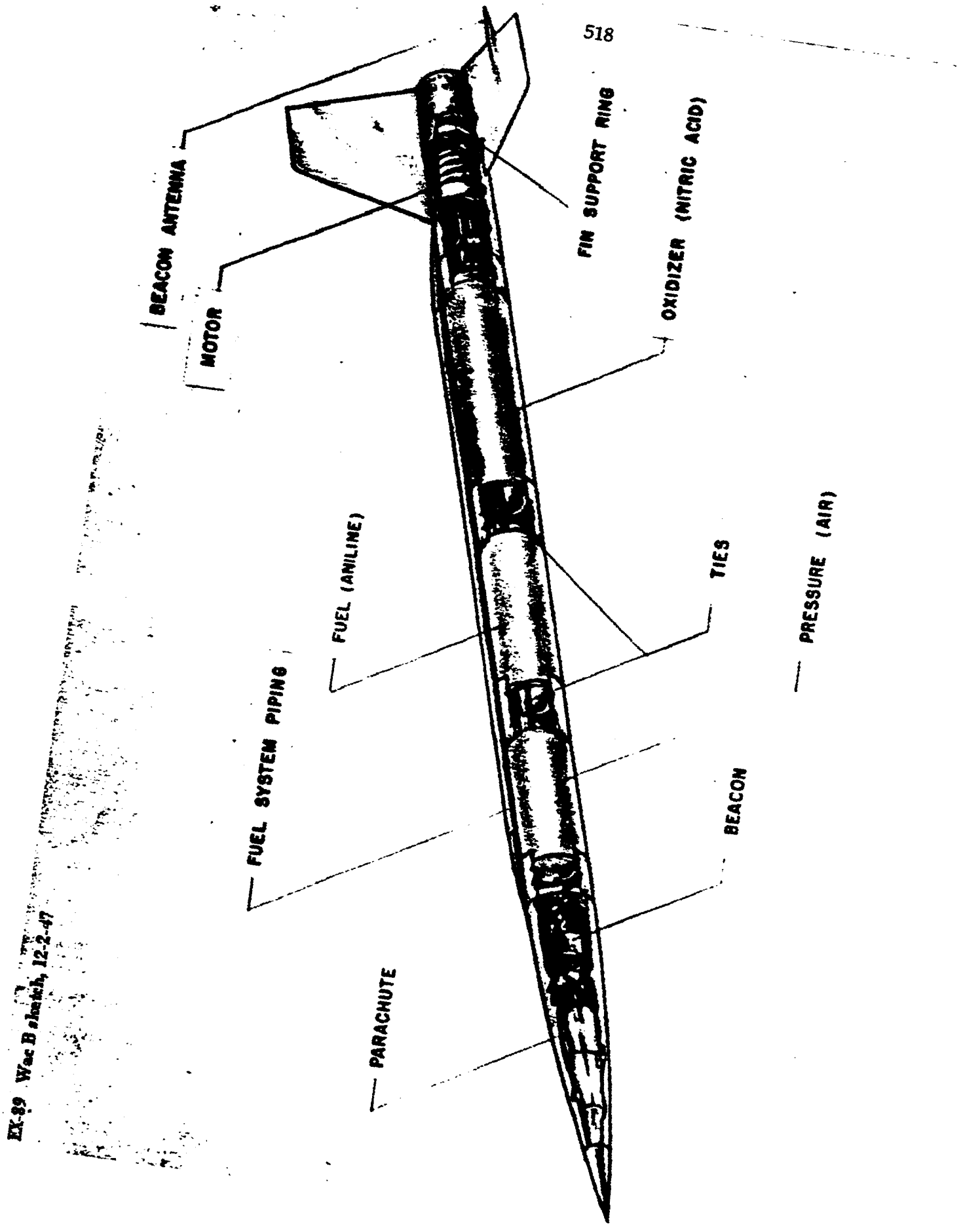
ρ_p = mass density of propellant

Fig. 10
Four Differential Equations That Describe the Operation of an Ideal Solid Propellant Rocket Motor

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Appendix II

A Short Glossary of Rocket Terms

athodyd (ramjet engine): a jet engine which mixes fuel with compressed intake air, ignites the mixture and forces it out the exhaust with a velocity greater than intake velocity.

ballistic missile: a guided rocket with a two-phase trajectory. The first phase is self-powered, the second involves a free-fall descent to a target.

characteristic velocity: a property of specific propellants, and independent of chamber and nozzle pressure, characteristic velocity is the equivalent of mass flow of a propellant through the nozzle, dependent only on properties of the combustion products and chamber temperature.

composite propellant: a solid propellant which combines oxidizer and fuel in one mixture. Not to be confused with double-base propellant, a composite propellant often begins as a liquefied mass, which then cools or gels. Grain structure is not distinct, as is the case in a powdered double-base propellant.

double-base propellant: a solid propellant whose composition contains two explosive elements. Either or both of these chemicals may contain an oxidizer in the same molecular chain as the fuel. The fundamental components are usually nitroglycerin and nitrocellulose.

effective exhaust velocity (c): a term describing actual thrust, increasing as gravitational force on a rocket decreases. Effective exhaust velocity is dependent on atmospheric pressure. The lower the air pressure, the higher the thrust. $(I_{sp})\ g_o = c$

hypergolic: the type of propellant/oxidizer mixture which ignites instantaneously and spontaneously upon combining.

Laval nozzle: a type of nozzle which first converges to a narrow dimension, then diverges out to its exit point. The narrowest part of the nozzle is called the throat.

rocket: a projectile propelled forward by the reaction principle, with its own internally ignited and self-contained fuel. Rockets may be guided or not. (Guided rockets are generally known as missiles.) Their chief characteristic is that they do not depend upon ingesting air from the atmosphere to operate.

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specific impulse (I_{sp}) : the ratio of the thrust delivered per unit weight of a propellant (liquid or solid); or the ratio of thrust delivered during an infinitesimal time interval and the propellant weight consumed during that time interval.

thrust coefficient: the ratio of a rocket's actual thrust and its theoretical thrust in a vacuum, with no propellant gases flowing through the combustion chamber.

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Oral Interviews

Transcripts of this researcher's Oral Interviews with Liljan Wunderman (Frank Malina's first wife), and with Apollo Milton Olin Smith are on file at the Caltech Archives, JPL Archives, (Pasadena) and the NASA History Office, (Washington.) Other extant Oral Interviews [utilized in this thesis], located either in the JPL Archives or NASA History Office, are also listed below.

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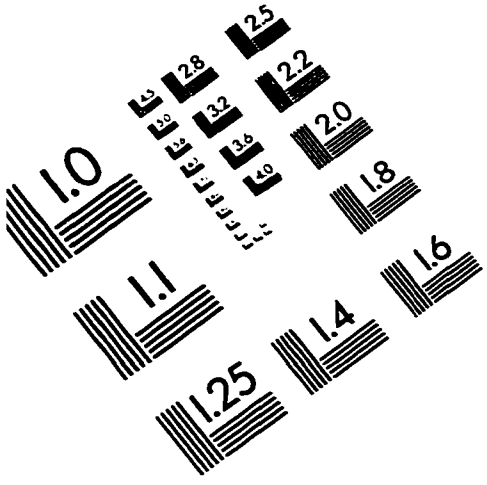
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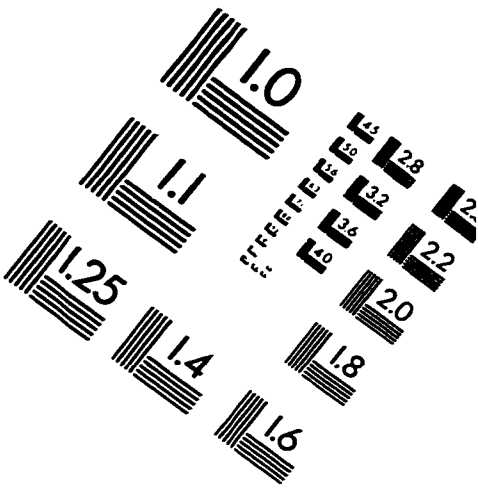
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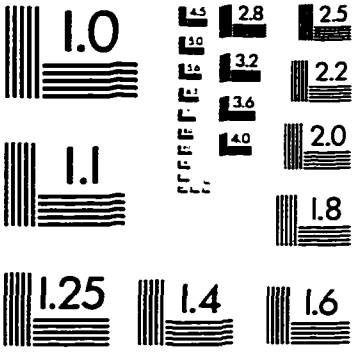
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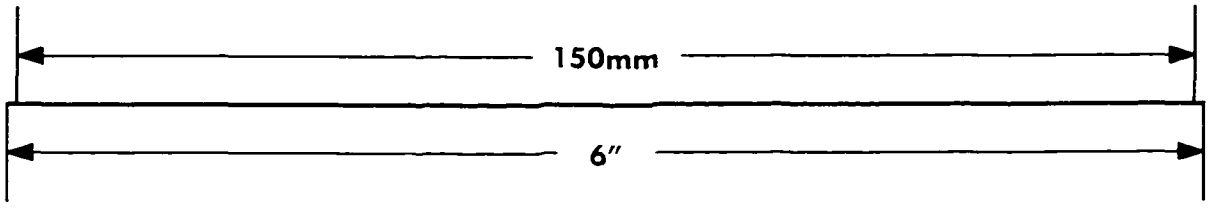
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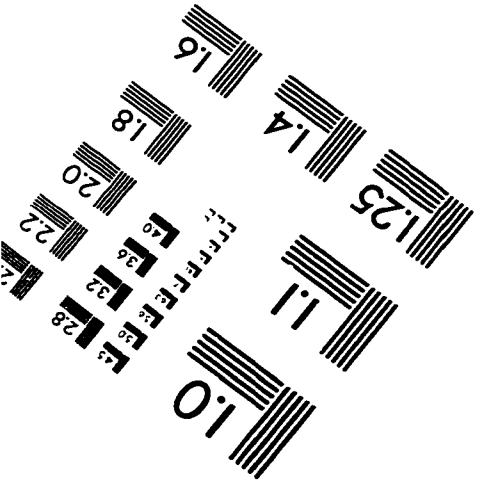
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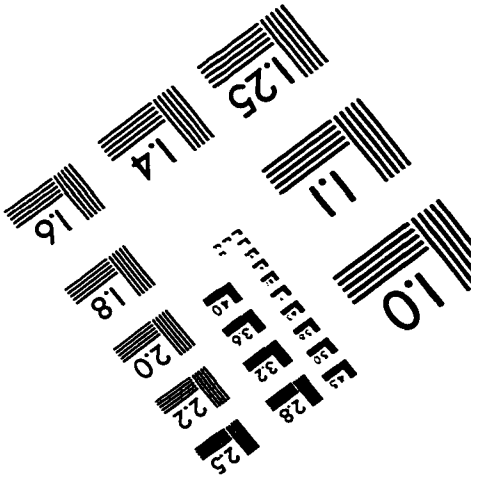












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