

**IGNITION!**

*U.S. Navy photo* This is what a test firing should look like. Note the mach diamonds in the exhaust stream.

*U.S. Navy photo* And this is what it may look like if something goes wrong. The same test cell, or its remains, is shown.



# IGNITION!

## An Informal History of Liquid Rocket Propellants

by John D. Clark

*Those who cannot remember the past are condemned to repeat it.*

George Santayana

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This book is dedicated to my wife Inga, who heckled me into writing it with such wifely remarks as, “You talk a hell of a fine history. Now set yourself down in front of the typewriter—and write the damned thing!”



## In Re John D. Clark

*by Isaac Asimov*

I first met John in 1942 when I came to Philadelphia to live. Oh, I had known of him before. Back in 1937, he had published a pair of science fiction shorts, "Minus Planet" and "Space Blister," which had hit me right between the eyes. The first one, in particular, was the earliest science fiction story I know of which dealt with "anti-matter" in realistic fashion.

Apparently, John was satisfied with that pair and didn't write any more s.f., kindly leaving room for lesser lights like myself. In 1942, therefore, when I met him, I was ready to be awed. John, however, was not ready to awe. He was exactly what he has always been, completely friendly, completely self-unconscious, completely himself.

He was my friend when I needed friendship badly. America had just entered the war and I had come to Philadelphia to work for the Navy as a chemist. It was my first time away from home, ever, and I was barely twenty-two. I was utterly alone and his door was always open to me. I was frightened and he consoled me. I was sad and he cheered me.

For all his kindness, however, he could not always resist the impulse to take advantage of a greenhorn.

Every wall of his apartment was lined with books, floor to ceiling, and he loved displaying them to me. He explained that one wall was devoted to fiction, one to histories, one to books on military affairs and so on.

"Here," he said, "is the Bible." Then, with a solemn look on his face, he added, "I have it in the fiction section, you'll notice, under J."

"Why J?" I asked.

And John, delighted at the straight line, said, "J for Jehovah!"

But the years passed and our paths separated. The war ended and I returned to Columbia to go after my PhD (which John had already earned by the time I first met him) while he went into the happy business of designing rocket fuels. Now it is clear that anyone working with rocket fuels is outstandingly mad. I don't mean garden-variety crazy or a merely raving lunatic. I mean a record-shattering exponent of far-out insanity.

There are, after all, some chemicals that explode shatteringly, some that flame ravenously, some that corrode hellishly, some that poison sneakily, and some that stink stenchily. As far as I know, though, only liquid rocket fuels have all these delightful properties combined into one delectable whole.

Well, John Clark worked with these miserable concoctions and survived all in one piece. What's more he ran a laboratory for seventeen years that played footsie with these liquids from Hell and never had a time-lost accident.

My own theory is that he made a deal with the Almighty. In return for Divine protection, John agreed to take the Bible out of the fiction section.

So read this book. You'll find out plenty about John and all the other sky-high crackpots who were in the field with him and you may even get (as I did) a glimpse of the heroic excitement that seemed to make it reasonable to cuddle with death every waking moment—to say nothing of learning a heck of a lot about the way in which the business of science is really conducted.

It is a story only John can tell so caustically well from the depths within.

## Preface

Millions of words have been written about rocketry and space travel, and almost as many about the history and development of the rocket. But if anyone is curious about the parallel history and development of rocket propellants—the fuels and the oxidizers that make them go—he will find that there is no book which will tell him what he wants to know. There are a few texts which describe the propellants currently in use, but nowhere can he learn why these and not something else fuel Saturn V or Titan II, or SS-9. In this book I have tried to make that information available, and to tell the story of the development of liquid rocket propellants: the who, and when, and where and how and why of their development. The story of solid propellants will have to be told by somebody else.

This is, in many ways, an auspicious moment for such a book. Liquid propellant research, active during the late 40's, the 50's, and the first half of the 60's, has tapered off to a trickle, and the time seems ripe for a summing up, while the people who did the work are still around to answer questions. Everyone whom I have asked for information has been more than cooperative, practically climbing into my lap and licking my face. I have been given reams of unofficial and quite priceless information, which would otherwise have perished with the memories of the givers. As one of them wrote to me, "What an opportunity to bring out repressed hostilities!" I agree.

My sources were many and various. Contractor and government agency progress (sometimes!) reports, published collections of papers presented at various meetings, the memories of participants in the story, intelligence reports; all have contributed. Since this is not a formal history, but an informal attempt by an active participant to tell the story as it happened, I haven't attempted formal documentation. Particularly as in many cases such documentation would be

embarrassing—not to say hazardous! It's not only newsmen who have to protect their sources.

And, of course, I have drawn on my own records and recollections. For something more than twenty years, from 1 November 1949, when I joined the U.S. Naval Air Rocket Test Station, until 2 January 1970, when I retired from its successor, the Liquid Rocket Propulsion Laboratory of Picatinny Arsenal, I was a member of the unofficial, but very real, liquid propellant community, and was acutely aware of what was going on in the field, in this country and in England. (It wasn't until the late 50's that it was possible to learn much about the work in the Soviet Union, and propellant work outside these three countries has been negligible.)

The book is written not only for the interested layman—and for him I have tried to make things as simple as possible—but also for the professional engineer in the rocket business. For I have discovered that he is frequently abysmally ignorant of the history of his own profession, and, unless forcibly restrained, is almost certain to do something which, as we learned fifteen years ago, is not only stupid but is likely to result in catastrophe. Santayana knew exactly what he was talking about.

So I have described not only the brilliantly conceived programs of research and development, but have given equal time to those which, to put it mildly, were not so well advised. And I have told the stories of the triumphs of propellant research; and I have described the numerous blind alleys up which, from time to time, the propellant community unanimously charged, yapping as they went.

This book is opinionated. I have not hesitated to give my own opinion of a program, or of the intelligence—or lack of it—of the proposals made by various individuals. I make no apology for this, and can assure the reader that such criticism was not made with the advantage of 20-20 hindsight. At one point, in writing this book, when I had subjected one particular person's proposals to some rather caustic criticism, I wondered whether or not I had felt that way at the time they were made. Delving into my (very private) logbook, I found that I had described them then, simply as "Brainstorms and bullbleep!" So my opinion had not changed—at least, not noticeably.

I make no claim to completeness, but I have tried to give an accurate account of the main lines of research. If anyone thinks that I have unreasonably neglected his work, or doesn't remember things as I do, let him write to me, and the matter will be set right in the next (d.v.) edition. And if I seem to have placed undue emphasis on what happened in my own laboratory, it is not because my labora-

tory was unusual (although more nutty things seem to have happened there than in most labs) but that it was not, so that an account of what happened there is a good sample of the sort of things which were happening, simultaneously, in a dozen other laboratories around the country.

The treatment of individuals' names is, I know, inconsistent. The fact that the family name of somebody mentioned in the text is preceded by his given name rather than by his initials signifies only that I know him very well. Titles and degrees are generally ignored. Advanced degrees were a dime a dozen in the business. And the fact that an individual is identified in one chapter with one organization, and with another in the next, should be no cause for confusion. People in the business were always changing jobs. I think I set some sort of a record by staying with the same organization for twenty years.

One thing that is worth mentioning here is that this book is about a very few people. The propellant community—comprising those directing or engaged in liquid propellant research and development—was never large. It included, at the most, perhaps two hundred people, three-quarters of whom were serving merely as hands, and doing what the other quarter told them to do. That one quarter was a remarkably interesting and amusing group of people, including a surprisingly small number (compared to most other groups of the same size) of dopes or phoneys. We all knew each other, of course, which made for the informal dissemination of information at a velocity approaching that of light. I benefited particularly from this, since, as I was working for Uncle, and not for a rival contractor, nobody hesitated to give me “proprietary” information. If I wanted the straight dope from somebody, I knew I could get it at the bar at the next propellant meeting. (Many of the big propellant meetings were held in hotels, whose management, intelligently, would always set up a bar just outside the meeting hall. If the meeting wasn't in a hotel, I'd just look around for the nearest cocktail lounge; my man would probably be there.) I would sit down beside him, and, when my drink had arrived, ask, “Joe, what did happen on that last test firing you made? Sure, I've read your report, but I've written reports myself. What really happened?” Instant and accurate communication, without pain.

Conformists were hard to find in the group. Almost to a man, they were howling individualists. Sometimes they got along together—sometimes they didn't, and management had to take that into account. When Charlie Tait left Wyandotte, and Lou Rapp left Reaction Motors, and they both came to Aerojet, the management of the latter,

with surprising intelligence, stationed one of them in Sacramento and one in Azusa, separated by most of the length of the state of California. Lou had been in the habit, when Charlie was giving a paper at a meeting, of slipping a nude or two into Charlie's collection of slides, and Charlie was no longer amused.

But friends or not, or feuding or not, everything we did was done with one eye on the rest of the group. Not only were we all intellectual rivals—"anything you can do I can do better"—but each of us knew that the others were the only people around competent to judge his work. Management seldom had the technical expertise, and since most of our work was classified, we couldn't publish it to the larger scientific community. So praise from the in-group was valued accordingly. (When Irv Classman, presenting a paper, mentioned "Clark's classical work on explosive sensitivity," it put me on cloud nine for a week. Classical, yet!) The result was a sort of group Narcissism which was probably undesirable—but it made us work like Hell.

We did that anyway. We were in a new and exciting field, possibilities were unlimited, and the world was our oyster just waiting to be opened. We knew that we didn't have the answers to the problems in front of us, but we were sublimely confident of our ability to find them in a hurry, and set about the search with a "gusto"—the only word for it—that I have never seen before or since. I wouldn't have missed the experience for the world. So, to my dear friends and once deadly rivals, I say, "Gentlemen, I'm glad to have known you!"

John D. Clark

*Newfoundland, N.J.*  
*January 1971*

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**IGNITION!**



## How It Started

The dear Queen had finally gone to her reward, and King Edward VII was enjoying himself immensely as he reigned over the Empire upon which the sun never set. Kaiser Wilhelm II in Germany was building battleships and making indiscreet remarks, and in the United States President Theodore Roosevelt was making indiscreet remarks and building battleships. The year was 1903, and before its end the Wright brothers' first airplane was to stagger briefly into the air. And in his city of St. Petersburg, in the realm of the Czar of All the Russias, a journal whose name can be translated as "Scientific Review" published an article which attracted no attention whatsoever from anybody.

Its impressive but not very informative title was "Exploration of Space with Reactive Devices," and its author was one Konstantin Eduardovitch Tsiolkovsky, an obscure schoolteacher in the equally obscure town of Borovsk in Kaluga Province. The substance of the article can be summarized in five simple statements.

1. Space travel is possible.
2. This can be accomplished by means of, and only by means of, rocket propulsion, since a rocket is the only known propulsive device which will work in empty space.
3. Gunpowder rockets cannot be used, since gunpowder (or smokeless powder either, for that matter) simply does not have enough energy to do the job.
4. Certain liquids do possess the necessary energy.

5. Liquid hydrogen would be a good fuel and liquid oxygen a good oxidizer, and the pair would make a nearly ideal propellant combination.

The first four of these statements might have been expected to raise a few eyebrows if anybody had been listening, but nobody was, and they were received with a deafening silence. The fifth statement was of another sort entirely, and a few years earlier would have been not merely surprising, but utterly meaningless. For liquid hydrogen and liquid oxygen were new things in the world.

Starting with Michael Faraday in 1823, scientists all over Europe had been trying to convert the various common gases to liquids—cooling them, compressing them, and combining the two processes. Chlorine was the first to succumb, followed by ammonia, carbon dioxide, and many others, and by the seventies only a few recalcitrants still stubbornly resisted liquefaction. These included oxygen, hydrogen and nitrogen (fluorine had not yet been isolated and the rare gases hadn't even been discovered), and the holdouts were pessimistically called the "permanent gases."

Until 1883. In April of that year, Z.F. Wroblewski, of the University of Krakow, in Austrian Poland, announced to the French Academy that he and his colleague K.S. Olszewski had succeeded in their efforts to liquefy oxygen. Liquid nitrogen came a few days later, and liquid air within two years. By 1891 liquid oxygen was available in experimental quantities, and by 1895 Linde had developed a practical, large-scale process for making liquid air, from which liquid oxygen (and liquid nitrogen) could be obtained, simply by fractional distillation.

James Dewar (later Sir James, and the inventor of the Dewar flask and hence of the thermos bottle), of the Royal Institute in London, in 1897 liquefied fluorine, which had been isolated by Moisson only eleven years before, and reported that the density of the liquid was 1.108. This wildly (and inexplicably) erroneous value (the actual density is 1.50) was duly embalmed in the literature, and remained there, unquestioned, for almost sixty years, to the confusion of practically everybody.

The last major holdout—hydrogen—finally succumbed to his efforts, and was liquefied in May of 1898. And, as he triumphantly reported, "on the thirteenth of June, 1901, five liters of it (liquid hydrogen) were successfully conveyed through the streets of London from the laboratory of the Royal Institution to the chambers of the Royal Society!"

And only then could Tsiolkovsky write of space travel in a rocket propelled by liquid hydrogen and liquid oxygen. Without Wroblewski and Dewar, Tsiolkovsky would have had nothing to talk about.

In later articles, Tsiolkovsky discussed other possible rocket fuels—methane, ethylene, benzene, methyl and ethyl alcohols, turpentine, gasoline, kerosene—practically everything that would pour and burn, but he apparently never considered any oxidizer other than liquid oxygen. And although he wrote incessantly until the day of his death (1935) his rockets remained on paper. He never did anything about them. The man who did was Robert H. Goddard.

As early as 1909 Dr. Goddard was thinking of liquid rockets, and came to the same conclusions as had his Russian predecessor (of whom he had never heard); that liquid hydrogen and liquid oxygen would be a near-ideal combination. In 1922, when he was Professor of Physics at Clark University, he started actual experimental work on liquid rockets and their components. Liquid hydrogen at that time was practically impossible to come by, so he worked with gasoline and liquid oxygen, a combination which he used in all of his subsequent experimental work. By November 1923 he had fired a rocket motor on the test stand, and on March 16, 1926, he achieved the first flight of a liquid-propelled rocket. It flew 184 feet in 2.5 seconds. (Exactly forty years later, to the day, Armstrong and Scott were struggling desperately to bring the wildly rolling Gemini 8 under control.)

One odd aspect of Goddard's early work with gasoline and oxygen is the very low oxidizer-to-fuel ratio that he employed. For every pound of gasoline he burned, he burned about 1.3 or 1.4 pounds of oxygen, when three pounds of oxygen would have been closer to the optimum. As a result, his motors performed very poorly, and seldom achieved a specific impulse of more than 170 seconds. (The specific impulse is a measure of performance of a rocket and its propellants. It is obtained by dividing the thrust of the rocket in pounds, say, by the consumption of propellants in pounds per second. For instance, if the thrust is 200 pounds and the propellant consumption is one pound per second, the specific impulse is 200 seconds.) It seems probable that he worked off-ratio to reduce the combustion temperature and prolong the life of his hardware—that is, simply to keep his motor from burning up.

The impetus for the next generation of experimenters came in 1923, from a book by a completely unknown Transylvanian German, one Herman Oberth. The title was *Die Rakete zu den Planetenraumen*, or *The Rocket into Planetary Space*, and it became, surprisingly, something of a minor best seller. People started thinking about rockets—practically

nobody had heard of Goddard, who worked in exaggerated and unnecessary secrecy—and some of the people who thought about rockets decided to do something about them. First, they organized societies. The Verein für Raumschiffahrt, or Society for Space Travel, generally known as the VfR, was the first, in June 1927. The American Interplanetary Society was founded early in 1930, the British Interplanetary Society in 1933, and two Russian groups, one in Leningrad and one in Moscow, in 1929. Then, they lectured and wrote books about rockets and interplanetary travel. Probably the most important of these was Robert Esnault-Pelterie's immensely detailed *L'Astronautique*, in 1930. And Fritz Lang made a movie about space travel—*Frau in Mond*, or *The Woman on the Moon*, and hired Oberth as technical adviser. And it was agreed that Lang and the film company (UFA) would put up the money necessary for Oberth to design and build a liquid-fueled rocket which would be fired, as a publicity stunt, on the day of the premiere of the movie.

The adventures of Oberth with the movie industry—and vice versa—are a notable contribution to the theater of the absurd (they have been described elsewhere, in hilarious detail), but they led to one interesting, if abortive, contribution to propellant technology. Foiled in his efforts to get a gasoline-oxygen rocket flying in time for the premiere of the movie (the time available was ridiculously short) Oberth designed a rocket which, he hoped, could be developed in a hurry. It consisted of a long vertical aluminum tube with several rods of carbon in the center, surrounded by liquid oxygen. The idea was that the carbon rods were to burn down from the top at the same rate as the oxygen was to be consumed, while the combustion gases were ejected through a set of nozzles at the top (forward) end of the rocket. He was never able to get it going, which was probably just as well, as it would infallibly have exploded. But—it was the first recorded design of a hybrid rocket—one with a solid fuel and a liquid oxidizer. (A “reverse” hybrid uses a solid oxidizer and a liquid fuel.)

At any rate, the premiere came off on October 15, 1929 (without rocket ascent), and the VfR (after paying a few bills) fell heir to Oberth's equipment, and could start work on their own in early 1930. But here the story starts to get complicated. Unknown to the VfR or to anybody else—at least three other groups were hard at work. F.A. Tsander, in Moscow, headed one of these. He was an aeronautical engineer who had written extensively—and imaginatively—on rockets and space travel, and in one of his publications had suggested that an astronaut might stretch his fuel supply by imitating Phileas Fogg.

When a fuel tank was emptied, the astronaut could simply grind it up and add the powdered aluminum thus obtaining to the remaining fuel, whose heating value would be correspondingly enhanced! This updated emulation of the hero of *Around the World in Eighty Days*, who, when he ran out of coal, burned up part of his ship in order to keep the rest of it moving, not unnaturally remained on paper, and Tsander's experimental work was in a less imaginative vein. He started work in 1929, first with gasoline and gaseous air, and then, in 1931, with gasoline and liquid oxygen.

Another group was in Italy, headed by Luigi Crocco, and financed, reluctantly, by the Italian General Staff.\*

Crocco started to work on liquid rockets in 1929, and by the early part of 1930 was ready for test firings. His work is notable not only for the surprising sophistication of his motor design, but above all for his propellants. He used gasoline for his fuel, which is not surprising, but for his oxidizer he broke away from oxygen, and used nitrogen tetroxide,  $N_2O_4$ . This was a big step—nitrogen tetroxide, unlike oxygen, can be stored indefinitely at room temperature—but nobody outside of his own small group heard of the work for twenty-four years!†

V.P. Glushko, another aeronautical engineer, headed the rocket group in Leningrad. He had suggested suspensions of powdered beryllium in oil or gasoline as fuels, but in his first firings in 1930, he used straight toluene. And he took the same step—independently—as had Crocco. He used nitrogen tetroxide for his oxidizer. The VfR was completely unaware of all of this when they started work. Oberth had originally

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\*The fact that the whole project was headed by a General G.A. Crocco is no coincidence. He was Luigi's father, and an Italian father is comparable to a Jewish mother.

†In a letter to *El Comercio*, of Lima, Peru, 7 October, 1927, one Pedro A. Paulet, a Peruvian chemical engineer, claimed to have experimented—in 1895-97 (!)—with a rocket motor burning gasoline and nitrogen tetroxide. If this claim has any foundation in fact, Paulet anticipated not only Goddard but even Tsiolkovsky.

However, consider these facts. Paulet claimed that his motor produced a thrust of 200 pounds, and that it fired intermittently, 300 times a minute, instead of continuously as conventional rocket motors do.

He also claimed that he did his experimental work in Paris.

Now, I know how much noise a 200-pound motor makes. And I know that if one were fired three hundred times a minute—the rate at which a watch ticks—it would sound like a whole battery of fully automatic 75 millimeter anti-aircraft guns. Such a racket would have convinced the Parisians that the Commune had returned to take its vengeance on the Republic, and would certainly be remembered by somebody beside Paulet! But only Paulet remembered.

In my book, Paulet's claims are completely false, and his alleged firings never took place.

wanted to use methane as fuel, but as it was hard to come by in Berlin, their first work was with gasoline and oxygen. Johannes Winkler, however, picked up the idea, and working independently of the VfR, was able to fire a liquid oxygen-liquid methane motor before the end of 1930. This work led nowhere in particular, since, as methane has a performance only slightly superior to that of gasoline, and is much harder to handle, nobody could see any point to following it up.

Much more important were the experiments of Friedrich Wilhelm Sander, a pyrotechnician by trade (he made commercial gunpowder rockets) who fired a motor early in March 1931. He was somewhat coy about his fuel, calling it merely a “carbon carrier,” but Willy Ley has suggested that it may well have been a light fuel oil, or benzene, into which had been stirred considerable quantities of powdered carbon or lampblack. As a pyrotechnician, Sander would naturally think of carbon as the fuel, and one Hermann Noordung (the pseudonym of Captain Potocnik of the old Imperial Austrian army), the year before, had suggested a suspension of carbon in benzene as a fuel. (The idea was to increase the density of the fuel, so that smaller tanks might be used.) The important thing about Sander’s work is that he introduced another oxidizer, red fuming nitric acid. (This is nitric acid containing considerable quantities—5 to 20 or so percent—of dissolved nitrogen tetroxide.) His experiments were the start of one of the main lines of propellant development.

Esnault-Pelterie, an aviation pioneer and aeronautical engineer, during 1931, worked first with gasoline and oxygen, and then with benzene and nitrogen tetroxide, being the third experimenter to come up, independently, with this oxidizer. But that was to be a repeating pattern in propellant research—half a dozen experimenters generally surface simultaneously with identical bones in their teeth! His use of benzene (as Glushko’s of toluene) as a fuel is rather odd. Neither of them is any improvement on gasoline as far as performance goes, and they are both much more expensive. And then Esnault-Pelterie tried to use tetranitromethane,  $C(NO_2)_4$  for his oxidizer, and promptly blew off four fingers. (This event was to prove typical of TNM work.)

Glushko in Leningrad took up where Sander had left off, and from 1932 to 1937 worked with nitric acid and kerosene, with great success. The combination is still used in the USSR. And in 1937, in spite of Esnault-Pelterie’s experience, which was widely known, he successfully fired kerosene and tetranitromethane. This work, however, was not followed up.

Late in 1931 Klaus Riedel of the VfR designed a motor for a new combination, and it was fired early in 1932. It used liquid oxygen, as usual, but the fuel, conceived by Riedel and Willy Ley, was a 60-40 mixture of ethyl alcohol and water. The performance was somewhat below that of gasoline, but the flame temperature was much lower, cooling was simpler, and the hardware lasted longer. This was the VfR's major contribution to propellant technology, leading in a straight line to the A-4 (or V-2) and it was its last. Wernher von Braun started work on his PhD thesis on rocket combustion phenomena at Kummersdorf-West in November 1932 under Army sponsorship, the Gestapo moved in on the rest of the VfR, and the society was dead by the end of 1933.

Dr. Eugen Sanger, at the University of Vienna, made a long series of firings during 1931 and 1932. His propellants were conventional enough—liquid (or sometimes gaseous) oxygen and a light fuel oil—but he introduced an ingenious chemical wrinkle to get his motor firing. He filled the part of his fuel line next to the motor with diethyl zinc, to act as what we now call a “hypergolic starting slug.” When this was injected into the motor and hit the oxygen it ignited spontaneously, so that when the fuel oil arrived the fire was already burning nicely. He also compiled a long list, the first of many, of possible fuels, ranging from hydrogen to pure carbon, and calculated the performance of each with oxygen and with  $\text{N}_2\text{O}_5$ . (The latter, being not only unstable, but a solid to boot, has naturally never been used.) Unfortunately, in his calculations he somewhat naively assumed 100 percent thermal efficiency, which would involve either (a) an infinite chamber pressure, or (b) a zero exhaust pressure firing into a perfect vacuum, and in either case would require an infinitely long nozzle, which might involve some difficulties in fabrication. (Thermal efficiencies in a rocket usually run around 50 or 60 percent.) He also suggested that ozone might be used as an oxidizer, and as had Tsander, that powdered aluminum might be added to the fuel.

Then Luigi Crocco, in Italy, had another idea, and was able to talk the Ministry of Aviation into putting up a bit of money to try it out. The idea was that of a monopropellant. A monopropellant is a liquid which contains in itself both the fuel and the oxidizer, either as a single molecule such as methyl nitrate,  $\text{CH}_3\text{NO}_3$  in which the oxygens can burn the carbon and the hydrogens, or as a mixture of a fuel and an oxidizer, such as a solution of benzene in  $\text{N}_2\text{O}_4$ . On paper, the idea looks attractive. You have only one fluid to inject into the chamber, which simplifies your plumbing, your mixture ratio is built in and stays where you want it, you don't have to worry about building an injector

which will mix the fuel and the oxidizer properly, and things are simpler all around. *But!* Any intimate mixture of a fuel and an oxidizer is a potential explosive, and a molecule with one reducing (fuel) end and one oxidizing end, separated by a pair of firmly crossed fingers, is an invitation to disaster.

All of which Crocco knew. But with a species of courage which can be distinguished only with difficulty from certifiable lunacy, he started in 1932 on a long series of test firings with nitroglycerine (no less!) only slightly tranquilized by the addition of 30 percent of methyl alcohol. By some miracle he managed to avoid killing himself, and he extended the work to the somewhat less sensitive nitromethane,  $\text{CH}_3\text{NO}_2$ . His results were promising, but the money ran out in 1935, and nothing much came of the investigation.

Another early monopropellant investigator was Harry W. Bull, who worked on his own at the University of Syracuse. By the middle of 1932 he had used gaseous oxygen to burn gasoline, ether, kerosene, fuel oil, and alcohol. Later he tried, without success, to burn alcohol with 30 percent hydrogen peroxide (the highest strength available in the U.S. at the time), and to burn turpentine with (probably 70 percent) nitric acid. Then, in 1934 he tried a monopropellant of his own invention, which he called "Atalene," but did not otherwise identify. It exploded and put him in the hospital. Dead end.

And Helmuth Walter, at the Chemical State Institute in Berlin, in 1934 and 1935 developed a monopropellant motor which fired 80 percent hydrogen peroxide, which had only lately become available. When suitably catalyzed, or when heated, hydrogen peroxide decomposes into oxygen and superheated steam, and thus can be used as a monopropellant. This work was not made public—the Luftwaffe could see uses for it—but it was continued and led to many things in the next few years.

The last strictly prewar work that should be considered is that of Frank Malina's group at GALCIT. (Guggenheim Aeronautical Laboratories, California Institute of Technology.) In February of 1936 he planned his PhD thesis project, which was to be the development of a liquid-fueled sounding rocket. The group that was to do the job was gradually assembled, and was complete by the summer of 1937: six people, included Malina himself, John W. Parsons, the chemist of the group, Weld Arnold, who put up a little money, and Hsu Shen Tsien, who, thirty years later, was to win fame as the creator of Communist China's ballistic missiles. The benign eye of Theodore von Kármán watched over the whole.

The first thing to do was to learn how to run a liquid rocket motor, and experimental firings, with that object in view, started in October 1936. Methanol and gaseous oxygen were the propellants. But other propellants were considered, and by June 1937, Parsons had compiled lists, and calculated the performances (assuming, as had Sanger, 100 percent efficiency) of dozens of propellant combinations. In addition to Sanger's fuels, he listed various alcohols and saturated and unsaturated hydrocarbons, and such exotic items as lithium methoxide, dekaborane, lithium hydride, and aluminum triemethyl. He listed oxygen, red fuming nitric acid, and nitrogen tetroxide as oxidizers.

The next combination that the group tried then, was nitrogen tetroxide and methanol. Tests began in August 1937. But Malina, instead of working outdoors, as any sane man would have done, was so ill advised as to conduct his tests in the Mechanical Engineering building, which, on the occasion of a misfire, was filled with a mixture of methanol and  $N_2O_4$  fumes. The latter, reacting with the oxygen and the moisture in the air, cleverly converted itself to nitric acid, which settled corrosively on all the expensive machinery in the building. Malina's popularity with the establishment suffered a vertiginous drop, he and his apparatus and his accomplices were summarily thrown out of the building, and he was thereafter known as the head of the "suicide squad." Pioneers are seldom appreciated.

But the group continued work, until July 1, 1939, when, at the instigation of General Hap Arnold, the Army Air Corps sponsored a project to develop a JATO—a rocket unit to help heavily laden planes take off from short runways.

From now on, rocket research was to be paid for by the military, and was to be classified. GALCIT had lost her virginity with Malina's first explosion. Now she had lost her amateur standing.



## ABOUT THE AUTHOR

John D. Clark joined the Naval Air Rocket Test Station at Dover, N.J., in 1949. (It became the Liquid Rocket Propulsion Laboratory of Picatinny Arsenal in 1960.) He retired in 1970, after having been in charge of liquid propellant development for most of the twenty-one years.

A native of Alaska, Dr. Clark attended the University of Alaska and received the B.S. from the California Institute of Technology, the M.S. from the University of Wisconsin, and, in 1934, the Ph.D. from Stanford University. He worked as a chemist in several industries prior to his association with NARTS.

A great deal of Dr. Clark's writing has been in the form of government reports which are classified, but he has also contributed scientific articles and papers in the open literature and, as an avocation, written stories and articles for science fiction magazines.