

Lox and Flox and Cryogenics in General

While all this was going on, liquid oxygen was still very much in the picture. The sounding rocket Viking burned it with ethyl alcohol, as had the A-4, and so did several experimental vehicles of the early 50's, as well as the Redstone missile. Most of these, too, used the auxiliary power source of the A-4, hydrogen peroxide, to drive the feed pumps, and so on. The X-1, the first supersonic plane, was driven by an RMI Lox-alcohol rocket motor.

Other alcohols were tried as fuels to be used with oxygen—methanol by JPL as early as 1946, and isopropanol by North American early in 1951—but they weren't any particular improvement over ethanol. Neither was methylal, $\text{CH}_3\text{OCH}_2\text{OCH}_3$, which Winternitz, at RMI, was pressured into trying, much against his will (he knew it was a lot of foolishness) early in 1951. It seems that his boss had a friend who had a lot of methylal on hand, and if only some use for it could be found—? And at NARTS we did some studies for Princeton, using LOX and pure USP type drinking alcohol—not the denatured stuff. The only difference we could find was that it evaporated a lot faster than denatured alcohol when a sailor opened a drum to take a density reading. We had some very happy sailors while that program was going on.

But something more potent than alcohol was needed for the X-15 rocket-driven supersonic research plane. Hydrazine was the first choice, but it sometimes exploded when used for regenerative cooling, and in 1949, when the program was conceived, there wasn't enough of it around anyway. Bob Truax of the Navy, along with

Winternitz of Reaction Motors, which was to develop the 50,000 pounds thrust motor, settled on ammonia as a reasonably satisfactory second best. The oxygen-ammonia combination had been fired by JPL, but RMI really worked it out in the early 50's. The great stability of the ammonia molecule made it a tough customer to burn and from the beginning they were plagued with rough running and combustion instability. All sorts of additives to the fuel were tried in the hope of alleviating the condition, among them methylamine and acetylene. Twenty-two percent of the latter gave smooth combustion, but was dangerously unstable, and the mixture wasn't used long. The combustion problems were eventually cured by improving the injector design, but it was a long and noisy process. At night, I could hear the motor being fired, ten miles away over two ranges of hills, and could tell how far the injector design had progressed, just by the way the thing sounded. Even when the motor, finally, was running the way it should, and the first of the series was ready to be shipped to the West Coast to be test-flown by Scott Crossfield, everybody had his fingers crossed. Lou Rapp, of RMI, flying across the continent, found himself with a knowledgeable seat mate, obviously in the aerospace business, who asked him his opinion of the motor. Lou blew up, and declared, with gestures, that it was a mechanical monster, an accident looking for a place to happen, and that he, personally, considered that flying with it was merely a somewhat expensive method of suicide. Then, remembering something he turned to his companion and asked. "By the way, I didn't get your name. What is it?"

The reply was simple. "Oh, I'm Scott Crossfield."

Our first real IRBM's were Thor and Jupiter, and these were designed to burn oxygen and JP-4. And the pumps would be driven by a gas generator burning the same propellants, but with a very rich mixture, to produce gases which wouldn't melt the turbine blades. JP had a better performance than alcohol, and getting rid of the peroxide simplified matters.

But there were troubles. The sloppy specifications for JP-4 arose to haunt the engineers. It burned all right, and gave the performance it should—but. In the cooling passages it had a tendency to polymerize (you will remember that the specifications allowed a high percentage of olefins) into tarry substances which slowed the fuel flow, whereupon the motor would cleverly burn itself up. And in the gas generator it produced soot, coke, and other assorted deposits that completely fouled up the works. And, of course, no two barrels of it were alike. (Also, believe it or not, it grows bacteria which produce sludge!)

But they needed the performance of a hydrocarbon; alcohol would not do. So then what?

Finally somebody in authority sat down and thought the problem through. The specifications of JP-4 were as sloppy as they were to insure a large supply of the stuff under all circumstances. But Jupiter and Thor were designed and intended to carry nuclear warheads, and it dawned upon the thinker that you don't need a large and continuing supply of fuel for an arsenal of such missiles. Each missile is fired, if at all, just once, and after a few dozen of them have been lobbed over by the contending parties, the problem of fuel for later salvos becomes academic, because everybody interested is dead. So the only consideration is that the missile works right the first time—and you can make your fuel specifications just as tight as you like. Your first load of fuel is the only one you'll ever need.

The result was the specification for RP-1, which was issued in January of 1957. The freezing point limit was -40° , the maximum olefin content was set at 1 percent, and of aromatics at 5 percent. As delivered, it's usually better than the specifications: a kerosene in the C_{12} region, with a H/C ratio between 1.95 and 2.00, containing about 41 percent normal and branched paraffins, 56 of naphthenes, three of aromatics, and no olefins at all.

The polymerization and coking problems were solved, but Madoff and Silverman, at Rocketdyne (which was the autonomous division formed at North American to do all their rocket work) weren't entirely happy with the solution, and did extensive experimentation with diethylcyclohexane which, while not a pure compound, was a highly reproducible mixture of isomers, and was easy to come by. The results of their experiments were excellent, the fuel being appreciably superior to RP-1, but it never got into an operational missile. Atlas and Titan I, our first ICBM's were designed around RP-1 before Madoff and Silverman did their work, and Titan II used storable propellants. The F-1 motors of Saturn V burn LOX and RP-1.*

Oxygen motors generally run hot, and heat transfer to the walls is at a fantastic rate. This had been a problem from the beginning, even with regenerative cooling, but in the spring of 1948 experimenters

* LOX and RP-1 never burn absolutely clean, and there is always a bit of free carbon in the exhaust, which produces a luminous flame. So when you're looking at TV and see a liftoff from Cape Kennedy—or from Baikonur for that matter—and the exhaust flame is very bright, you can be sure that the propellants are Lox and RP-1 or the equivalent. If the flame is nearly invisible, and you can see the shock diamonds in the exhaust, you're probably watching a Titan II booster burning N_2O_4 and 50-50.

at General Electric came up with an ingenious fix. They put 10 percent of ethyl silicate in their fuel, which was, in this case, methanol. The silicate had the happy faculty of decomposing at the hot spots and depositing a layer of silicon dioxide, which acted as insulation and cut down the heat flux. And, although it was continuously ablated and swept away, it was continuously redeposited. Three years later, also at GE, Mullaney put 1 percent of GE silicone oil in isopropanol, and reduced the heat flux by 45 percent. The GE first stage motor of Vanguard used such a heat barrier. Winternitz at RMI had similar good results in 1950 and 1951 with ethyl silicate in ethanol and in methylal, and in 1951, with 5 percent of it in ammonia, he cut the heat flux by 60 percent.

Another tricky problem with an oxygen motor is that of getting it started. From the A-4 to Thor and Jupiter, a pyrotechnic start was the usual thing, but the complications were considerable and the reliability was poor. Sanger had used a starting slug of diethyl zinc, and Bell Aerosystems, in 1957, went him one better by using one of triethyl aluminum to start an oxygen-JP-4 motor. This technique was used in the later Atlas and all subsequent oxygen-RP motors. A sealed ampoule containing a mixture of 15 percent triethyl aluminum and 85 percent of triethyl boron is ruptured by the pressure in the fuel lines at start-up, reacts hypergolically with the liquid oxygen, and you're in business. Simple, and very reliable.

Alcohol, ammonia, and JP-4 or RP-1 were the fuels usually burned with LOX, but practically every other inflammable liquid available has been tried experimentally at one time or another. RMI tried, for instance, cyclopropane, ethylene, methyl acetylene, and methyl amine. None of these was any particular improvement on the usual fuels. Hydrazine was tried as early as 1947 (by the Bureau of Aeronautics at EES, Annapolis) and UDMH was tried by Aerojet in 1954. But in this country, in contrast to Russia, the combination of a hydrazine fuel and liquid oxygen is unusual. The only large-scale use of it was in the Jupiter-C, and the Juno-1 which were propelled by uprated Redstone motors, redesigned to burn Hydyne rather than alcohol. (Hydyne is a Rocketdyne developed 60-40 mixture of UDMH and diethylene triamine.)

Tsiolkovsky's ideal fuel was, of course, liquid hydrogen. It is useless, naturally, in a missile (its density is so low that it takes an inordinate tankage volume to hold any great amount of it) and the engineering problems stemming from its low boiling point are formidable, so it was pretty well left alone until after World War II.

Even then, it wasn't exactly easy to come by. There were just three organizations equipped to produce liquid hydrogen in 1947: the Uni-

versity of Chicago, the University of California, and Ohio State, and their combined productive capacity was 85 liters, or 13 pounds, per hour. (Assuming that the equipment could be run continuously, which it could not.) But in 1948 H. L. Johnson, of the Ohio State Research Foundation, burned it with oxygen in a small motor of about 100 pounds thrust. The next year Aerojet installed a 90-liter per hour continuous unit, and raised the U.S. capacity to 27 pounds an hour. Aerojet fired it at the 3000-pound thrust level, and used it as a regenerative coolant. (Each of the six 200,000 pound hydrogen motors in Saturn V, five in the second stage, one in the third, burns 80 pounds of hydrogen per *second*.)

Hydrogen is a super-cryogenic. Its boiling point of 21 K is lower than that of any other substance in the universe except helium. (That of oxygen is 90 K.) Which means that problems of thermal insulation are infinitely more difficult than with oxygen. And there is another difficulty, which is unique to hydrogen.

Quantum mechanics had predicted that the hydrogen molecule, H₂, should appear in two forms: ortho, with the nuclei of the two atoms spinning in the same direction (parallel), and para, with the two nuclei spinning in opposite directions (antiparallel). It further predicted that at room temperature or above, three-quarters of the molecules in a mass of hydrogen should appear in the ortho form and a quarter in the para, and that at its boiling point almost all of them should appear in the para state.

But for years nobody observed this phenomenon. (The two forms should be distinguishable by their thermal conductivity.) Then, in 1927, D. M. Dennison pointed out, in the *Proceedings of the Royal Society*, that the transition from the ortho to the para state might be a slow process, taking, perhaps, several days, and that if the investigators waited a while before making their measurements, they might get some interesting results.

Urey, Brickwedde and others in this country, as well as Clusius and Hiller in Germany looked into the question exhaustively between 1929 and 1937, and the results were indeed interesting, and when the propellant community got around to looking them up, disconcerting. The transition *was* slow, and took several days at 21 K. But that didn't matter to the rocket man who merely wanted to burn the stuff. What did matter was that each mole of hydrogen (2 grams) which changed from the ortho to the para state gave off 337 calories of heat in the process. And since it takes only 219 calories to vaporize one mole of hydrogen, you were in real trouble. For if you liquefied a mass of hydrogen, getting a liquid that was still almost three quarters ortho-hydrogen, the heat of the subsequent transition of that to para-hydro-

gen was enough to change the whole lot right back to the gaseous state. All without the help of any heat leaking in from the outside.

The answer to the problem was obvious—find a catalyst that will speed up the transition, so that the evolved heat can be disposed of during the cooling and liquefaction process and won't appear later to give you trouble; and through the 50's, several men were looking for such a thing. P. L. Barrick, working at the University of Colorado and at the Bureau of Standards at Boulder, Colorado, came up with the first one to be used on a large scale—hydrated ferric oxide. Since then several other catalytic materials have been found—palladium-silver alloys, ruthenium, and what not, several of them much more efficient than the ferric oxide—and the ortho-para problem can be filed and forgotten.

By 1961 liquid hydrogen was a commercial product, with Linde, Air Products, and several other organizations ready to sell you any amount you wanted, and to ship it to you in tank car lots. (The design of those tank cars, by the way, is quite something. Entirely new *kinds* of insulation had to be invented to make them possible.)

Handling liquid hydrogen, then, has become a routine job, although it has to be treated with respect. If it gets loose, of course, it's a ferocious fire and explosion hazard, and all sorts of precautions have to be taken to make sure that oxygen doesn't get into the stuff, freeze, and produce a murderously touchy explosive. And there is a delightful extra something about a hydrogen fire—the flame is almost invisible, and at least in daylight, you can easily walk right into one without seeing it.

A rather interesting recent development is slurried, or "slush" hydrogen. This is liquid hydrogen which has been cooled to its freezing point, 14 K, and partially frozen. The slushy mixture of solid and liquid hydrogen can be pumped just as though it were a homogeneous liquid, and the density of the slush is considerably higher than that of the liquid at its boiling point. R. F. Dwyer and his colleagues at the Linde division of Union Carbide are responsible for much of this work, which is still in the development stage.

The 30,000-pound Centaur, and the 200,000-pound J-2 are the largest hydrogen-oxygen motors which have been flown, but motors as large as 1,500,000 pounds (Aerojet's M-1) are on the way.* All

* It's a shame that Tsiolkovsky didn't live to see the M-1. It stands twenty-seven feet high, the diameter of the throat is thirty-two inches, and that of the nozzle exit is almost eighteen feet. At full thrust it gulps down almost 600 pounds of liquid hydrogen and a ton and a half of liquid oxygen per second. Konstantin Eduardovitch would have been impressed.

these use electrical ignition. Hydrogen and oxygen are not hypergolic but they are very easily ignited. Gaseous oxygen and hydrogen are admitted to a small pilot chamber, where they are touched off by an electrical spark, whereupon the pilot flame lights off the main chamber. Some work has been done on making oxygen hypergolic with hydrogen, and L. A. Dickinson, A. B. Amster, and others of Stanford Research Institute reported, late in 1963, that a minute quantity (less than a tenth of 1 percent) of O_3F_2 in liquid oxygen would do the job, and that the mixture was stable for at least a week at 90 K (the boiling point of oxygen). O_3F_2 , sometimes called ozone fluoride, is a dark red, unstable, and highly reactive liquid produced by an electrical glow discharge in mixtures of oxygen and fluorine at temperatures around 77 K. It has recently been proved that it is really a mixture of O_2F_2 and O_4F_2 . However, it doesn't seem likely that electrical ignition of hydrogen-oxygen motors will be supplanted for some time.

The ultimate in hydrogen motors is the nuclear rocket. As we have seen (in the chapter on performance) the way to get a really high performance is to heat hydrogen to 2000 K or so, and then expand it through a nozzle. And that is just what a nuclear rocket motor does. A graphite-moderated enriched uranium reactor is the energy source, and the hydrogen is the working fluid. (During development, one peculiar difficulty showed up. Hydrogen at 2000 K or so dissolves graphite—it goes to methane—like hot water working on a sugar cube. The answer—coat the hydrogen flow passages with niobium carbide.)

The Phoebus-1 motors, tested at Jackass Flats (lovely name!), Nevada in 1966, with an 1100 megawatt (thermal) reactor, operated successfully at the 55,000-pounds thrust level, with a specific impulse of 760. (Impulses above 850 are expected soon.) The power (rate of change of thermal energy to mechanical energy) was thus some 912 megawatts, which implies that the reactor was working somewhat above its nominal rating. The chamber temperature was about 2300 K.

The Phoebus-2 series nuclear engines, under development, are expected to operate at the 250,000-pounds thrust level; greater than the thrust of the J-2 and the reactor power (thermal) will be about 5000 megawatts. This is twice the power generated by the Hoover dam—and the reactor generating it is about the size of an office desk. An impressive little gadget.

Liquid fluorine work started about the same time as the liquid hydrogen work did. JPL, starting in 1947, was the pioneer. It wasn't

particularly available at that time, so they made and liquefied the fluorine on the site, a feat which inspires the respect of anyone who has ever tried to make a fluorine cell work for any length of time. They burned it first with gaseous hydrogen, but by 1948 they had succeeded in firing liquid hydrogen, and were using the latter as a regenerative coolant. And by the spring of 1950 they had done the same with hydrazine. Considering the then state of the technology, their achievement was somewhat miraculous.

Bill Doyle, at North American, had also fired a small fluorine motor in 1947, but in spite of these successes, the work wasn't immediately followed up. The performance was good, but the density of liquid fluorine (believed to be 1.108 at the boiling point) was well below that of oxygen, and the military (JPL was working for the Army at that time) didn't want any part of it.

This situation was soon to change. Some of the people at Aerojet simply didn't believe Dewar's 54-year-old figure on the density of liquid fluorine, and Scott Kilner of that organization set out to measure it himself. (The Office of Naval Research put up the money.) The experimental difficulties were formidable, but he kept at it, and in July, 1951, established that the density of liquid fluorine at the boiling point was not 1.108, but rather a little more than 1.54. There was something of a sensation in the propellant community, and several agencies set out to confirm his results. Kilner was right, and the position of fluorine had to be re-examined. (ONR, a paragon among sponsors, and the most sophisticated—by a margin of several parsecs—funding agency in the business, let Kilner publish his results in the open literature in 1952, but a lot of texts and references still list the old figure. And many engineers, unfortunately, tend to believe anything that is in print.)

Several agencies immediately investigated the performance of fluorine with hydrazine and with ammonia and with mixtures of the two, and with gratifying results. Not only did they get a good performance, but there were no ignition problems, liquid fluorine being hypergolic with almost anything that they tried as a fuel.

Unfortunately, it was also hypergolic with just about everything else. Fluorine is not only extremely toxic; it is a super-oxidizer, and reacts, under the proper conditions with almost everything but nitrogen, the lighter of the noble gases, and things that have already been fluorinated to the limit. And the reaction is usually violent.

It can be contained in several of the structural metals—steel, copper, aluminum, etc.—because it forms, immediately, a thin, inert coating of metal fluoride which prevents further attack. But if that

inert layer is scrubbed off, or melted, the results can be spectacular. For instance, if the gas is allowed to flow rapidly out of an orifice or a valve, or if it touches a spot of grease or something like that, the metal is just as likely as not to ignite—and a fluorine–aluminum fire is something to see. From a distance.

But, as is usually the case, the stuff can be handled if you go about it sensibly, and if you want to fire it in a rocket, Allied Chemical Co. will be glad to ship you a trailer truck full of liquid fluorine. That trailer is a rather remarkable device in itself. The inner fluorine tank is surrounded by a jacket of liquid nitrogen, to prevent the evaporation and escape of *any* fluorine into the atmosphere. All sorts of precautions—pilot trucks, police escorts, and what not—are employed when one of those trucks travels on a public road, but sometimes I've wondered what it would be like if a fluorine tank truck collided with one carrying, say, liquid propane or butane.

The development of large fluorine motors was a slow process, and sometimes a spectacular one. I saw one movie of a run made by Bell Aerosystems, during which a fluorine seal failed and the metal ignited. It looked as though the motor had two nozzles at right angles, with as much flame coming from the leak as from the nozzle. The motor was destroyed and the whole test cell burned out before the operators could shut down.

But good-sized fluorine motors have been developed and fired successfully, although none have yet flown in a space mission. Rocketdyne built Nomad, a 12,000-pound motor, burning fluorine and hydrazine, for upper stage work, and Bell developed the 35,000-pound Chariot for the third stage of Titan III. This burned fluorine and a mixture of monomethyl hydrazine, water, and hydrazine, balanced to burn to CO and HF, and to have a freezing point considerably below that of hydrazine. And GE has developed the 75,000-pound X-430 fluorine-hydrogen motor.

Ordin at LFPL, from 1953 on, and then the people at Rocketdyne, in the late 50's and early 60's, investigated the possibility of upgrading the performance of an RP-Lox motor by adding fluorine to the oxidizer (fluorine and oxygen are completely miscible, and their boiling points are only a few degrees apart), and found that 30 percent of fluorine in the lox raised the performance by more than 5 percent, and could still be tolerated (Rocketdyne burned it in an Atlas motor) by tanks, pumps, etc. which had been designed for liquid oxygen. And they got hypergolic ignition, as a bonus. The mixture of liquid fluorine and liquid oxygen is called "Flox," with the usually appended number signifying the percentage of fluorine. For maxi-

mum performance the combination should burn (with a hydrocarbon) to HF and CO, which means that Flox 70 is the best oxidizer for RP-1—at least as far as performance goes. The specific impulse of RP-1 and liquid oxygen (calculated at 1000 psi chamber pressure, 14.7 exhaust, shifting equilibrium, optimum O/F) is 300 seconds, with Flox 30 it is 316, with Flox 70 (which balances to CO and HF) it is 343 seconds, and with pure fluorine it drops to 318.

Fluorine is not likely ever to be used for the big boosters—all that HF in the exhaust would be rough on the launching pad and equipment, not to mention the surrounding population—and it's more expensive than oxygen by orders of magnitude, but for deep space work it's hard to think of a better combination than hydrogen and fluorine. It's on its way.

The future of ozone doesn't look so promising. Or, to be precise, ozone has been promising for years and years but hasn't been delivering.

Ozone, O_3 , is an allotropic form of oxygen. It's a colorless gas, or if it's cold enough, a beautiful deep blue liquid or solid. It's manufactured commercially (it's useful in water purification and the like) by the Welsbach process which involves an electrical glow discharge in a stream of oxygen. What makes it attractive as a propellant is that (1) its liquid density is considerably higher than that of liquid oxygen, and (2) when a mole of it decomposes to oxygen during combustion it gives off 34 kilocalories of energy, which will boost your performance correspondingly. Sanger was interested in it in the 30's, and the interest has endured to the present. In the face of considerable disillusionment.

For it has its drawbacks. The least of these is that it's at least as toxic as fluorine. (People who speak of the invigorating odor of ozone have never met a real concentration of it!) Much more important is the fact that it's unstable—murderously so. At the slightest provocation and sometimes for no apparent reason, it may revert explosively to oxygen. And this reversion is catalyzed by water, chlorine, metal oxides, alkalis—and by, apparently, certain substances which have not been identified. Compared to ozone, hydrogen peroxide has the sensitivity of a heavyweight wrestler.

Since pure ozone was so lethal, work was concentrated on solutions of ozone in oxygen, which could be expected to be less dangerous. The organizations most involved were the Forrestal Laboratories of Princeton University, the Armour Research Institute, and the Air Reduction Co. Work started in the early 50's, and has continued, on and off, ever since.

The usual procedure was to run gaseous oxygen through a Welsbach ozonator, condense the ozone in the emergent stream into liquid oxygen until you got the concentration you wanted, and then use this mixture as the oxidizer in your motor run. During 1954–57, the Forrestal fired concentrations of ozone as high as 25 percent, using ethanol as the fuel. And they had troubles.

The boiling point of oxygen is 90 K. (In working with cryogenics, it's much simpler to think and talk in absolute of Kelvin degrees than in Celsius.) That of ozone is 161 K. On shutdown, the inside of the oxidizer lines would be wet with the ozone–oxygen mixture, which would immediately start to evaporate. The oxygen, with the lower boiling point, would naturally come off first, and the solution would become more concentrated in ozone. And when that concentration approaches 30 percent, at any temperature below 93 K, a strange thing happens. The mixture separates into two liquid phases, one containing 30 percent ozone, and the other containing 75 percent. And as more oxygen boils off, the 30-percent phase decreases, and the 75-percent phase increases, until you have only one solution again – all 75 percent ozone. And *this* mixture is *really* sensitive!

So, after a series of post-shutdown explosions which were a bit hard on the plumbing and worse on the nerves of the engineers, some rather rigorous purging procedures were adopted. Immediately after shutdown, the oxidizer lines were flushed with liquid oxygen, or with gaseous oxygen or nitrogen, to get rid of the residual ozone before it could cause trouble.

That was some sort of a solution to the problem but not a very satisfactory one. Twenty-five percent ozone in oxygen is not so superior to oxygen as to make its attractions overwhelmingly more important than the difficulty of handling it. A somewhat superior solution would be to eliminate the phase separation somehow, and in 1954–55 G. M. Platz of the Armour Research Institute (now IITRI, or the Illinois Institute of Technology Research Institute) had some success in attempting to do this. He showed that the addition of about 2.8 percent of Freon 13, CClF_3 , to the mixture would prevent phase separation at 90 K, although not at 85 K. Which meant that if you had, say, a 35-percent mixture at the boiling point of oxygen, it would remain homogeneous, but if you cooled it to the boiling point of nitrogen, 77 K, the high concentration, lethal, phase would separate out. W. K. Boyd, W. E. Berry and E. L. White, of Battelle, and W. G. Marancic and A. G. Taylor of Air Reduction, came up with a better answer in 1964–65, when they showed that 5 percent of OF_2 or 9 percent of F_2 added to the mixture completely eliminated the phase

separation problem. And their addition didn't degrade the performance, as the Freon would have. Nobody has yet come up with an even faintly plausible explanation for the solubilizing effect of the additives!

One other ozone mixture has been considered – that of ozone and fluorine, which was thoroughly investigated during 1961 by A. J. Gaynor of Armour. (Thirty percent of ozone would be optimum for RP-1.) But the improvement over Flox 70 wouldn't be too impressive, and the thought of what might happen if the ozone in the oxidizer let go on the launching pad and spread the fluorine all over the landscape was somewhat unnerving, and I have heard of no motor runs with the mixture.

For ozone still explodes. Some investigators believe that the explosions are initiated by traces of organic peroxides in the stuff, which come from traces, say, of oil in the oxygen it was made of. Other workers are convinced that it's just the nature of ozone to explode, and still others are sure that original sin has something to do with it. So although ozone research has been continuing in a desultory fashion, there are very few true believers left, who are still convinced that ozone will somehow, someday, come into its own. I'm not one of them.