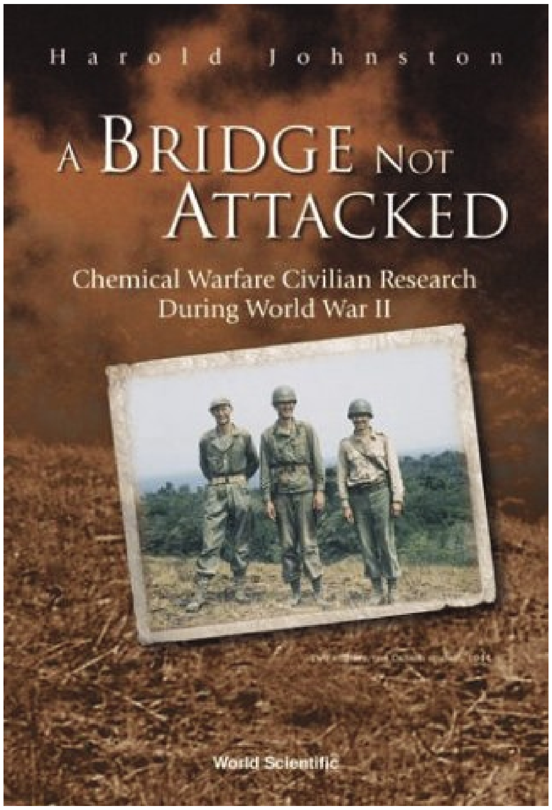
A Bridge Not Attacked

by Harold Johnston



A Bridge Not Attacked:
Chemical Warfare Civilian Research During World War II
By Harold Johnston
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Harold (Hal) Johnston graduated from Emory University in 1941 with a bachelor's degree in chemistry. He entered Caltech that fall and, like most of the campus, soon became involved in the war effort. What started as a laboratory project to improve gas masks soon led to a three-year campaign of field studies in places as far-flung as Florida and Panama. This article is adapted from his recent book on the subject, which is part personal memoir and part historical document.

Johnston resumed his graduate studies when the war ended, earning his PhD in chemistry in 1948. But the years spent tracing the dispersion of poison-gas clouds changed him from an inorganic chemist to an atmospheric one: he was one of the pioneers in the field in the 1950s. He was at Stanford until 1956, returned to Caltech for a year, and spent the rest of his academic career at UC Berkeley. Over the years he worked on the reaction kinetics of ozone, fluorine, chlorine, and the oxides of nitrogen, publishing 165 papers and one book.

Johnston was one of the first to recognize that human activities can have global atmospheric effects. A paper he published in Science *in August 1971 showed that the nitrogen-dioxide exhaust from a proposed fleet of supersonic transport aircraft (SSTs) could, depending upon the exhaust's vertical distribution, lead to global stratospheric ozone reductions of from 3 to 23 percent. This led Congress to set up the first major stratospheric research program—the Climate Impact Assessment Program, or CIAP. CIAP, in turn, provided the basis for F. Sherwood Rowland and Mario Molina's Nobel Prize–winning discovery (with Paul Crutzen) of the effects of chlorofluorocarbons on the ozone layer. Johnston was elected to the National Academy of Sciences in 1965 and the American Academy of Arts and Sciences in 1972. His awards include the Tyler Prize for Environmental Achievement (1983), the National Academy of Sciences Award for Chemistry in Service to Society (1993), and the National Medal of Science (1997).*

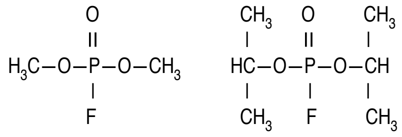
PICTURE CREDITS:
32 – Caltech Archives; 34, 35 – Roscoe Dickinson; 35 – John Otvos; 36, 37 – U.S. Army; 38 – Hal Johnston; 38 – Bob Mills

I entered Caltech on September 20, 1941, three weeks before my 21st birthday. At the end of the winter quarter, I joined Professor of Physical Chemistry Roscoe Dickinson's secret project for the National Defense Research Committee, or NDRC. (Dickinson had received the first PhD ever conferred by Caltech, in 1920.) The NDRC was a civilian organization formed in 1940 to perform contract research for the military on a wide range of problems. In the realm of chemistry alone, NDRC scientists developed such diverse products as napalm and hydraulic fluids.

"One breath of it and you die within a day, drowning in your own water and blood, and there is nothing anybody can do about it." I felt an almost pleasant tingle of fear.

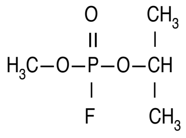
Our task was to determine how long a gas mask's charcoal filter would give protection. To do this, we flowed a stream of air and the poison gas through a bed of charcoal and then through counterflowing distilled water. Any unfiltered gas would dissolve, changing the water's conductivity, which was continuously recorded on a strip of chart paper. Thus we could measure the time it took for the gas to break through the bed. We repeated the tests with different amounts of the gas, at different temperatures, and with different charcoals. The War and Navy Departments carried out experiments of this sort by collecting about a dozen samples of the outflow from the bed as time passed, and submitting each sample to a tedious chemical analysis called titration. Dickinson's method was at least a hundred times faster and gave much more information.

The gases were kept in a fume hood in 65 Crellin, in glass bulbs immersed in Dewar flasks filled with dry ice. Though the bulbs' contents were gaseous at room temperature, they were liquid or solid at dry-ice temperature. The first bulb contained phosgene, Cl_2CO; it was our calibrating gas. It was used as a war gas in World War I. Phosgene dissolves slowly in water and passes through the nose and throat with only moderate irritation, but it reacts in the lungs to form two units of hydrochloric acid, HCl, which corrodes the lungs and can lead to death within 24 hours. The second bulb contained chlorofluorophosgene, ClFCO, which was much more toxic. It breaks down in the lungs to form one unit of hydrochloric acid and one unit of hydrofluoric acid, HF, which is a weaker acid but does more physiological damage. The third bulb contained sulfuryl chloride fluoride, $ClFSO_2$. It gives one unit of sulfuric acid (H_2SO_4) plus the acids carried by chlorofluorophosgene. The material in the fourth and fifth bulbs had arrived recently and came with special warnings. These organofluorophosphate gases quickly shut the pupils of the eyes, paralyzed the lungs, and were fast killers.



$$H_3C-O-\underset{\underset{\textstyle F}{|}}{\overset{\overset{\textstyle O}{\|}}{P}}-O-CH_3 \qquad \underset{\underset{\textstyle CH_3}{|}}{\overset{\overset{\textstyle CH_3}{|}}{HC}}-O-\underset{\underset{\textstyle F}{|}}{\overset{\overset{\textstyle O}{\|}}{P}}-O-\underset{\underset{\textstyle CH_3}{|}}{\overset{\overset{\textstyle CH_3}{|}}{CH}}$$

"Dimethyl poof," or dimethyl fluorophosphate (above left), and "diisopropyl poof" (above right) are close chemical cousins to sarin (below). Sarin was developed by the Germans during World War II, but was never deployed by them. It gained notoriety when used by a Japanese cult in a 1995 rush-hour attack on the Tokyo subways that killed a dozen people and injured thousands more.



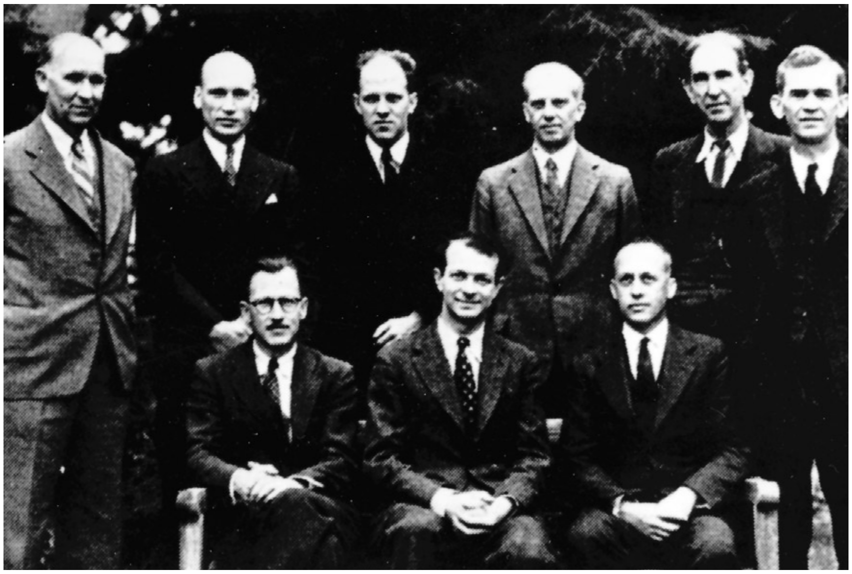
$$H_3C-O-\underset{\underset{\textstyle F}{|}}{\overset{\overset{\textstyle O}{\|}}{P}}-O-\underset{\underset{\textstyle CH_3}{|}}{\overset{\overset{\textstyle CH_3}{|}}{CH}}$$

Our group nicknamed them "dimethyl poof" and "diisopropyl poof," with the "poof" standing for the PF in fluorophosphate.

My immediate supervisor was John Otvos [PhD '43], a skilled and patient teacher, even to one as green as I. John was a third-year graduate student, or would have been if he had not joined this NDRC project. On my first day, John showed me around. When we came to a second hood, he lifted a small sealed glass tube out of its dry ice slush; it was about half full of a clear liquid. He said in a matter-of-fact voice: "That is sulfur decafluoride, S_2F_{10}, or S-10. It is a close relative to sulfur hexafluoride, SF_6, which is about the most unreactive compound known to chemistry. S-10 is colorless, odorless, and four times as poisonous as phosgene. One breath of it and you die within a day, drowning in your own water and blood, and there is nothing anybody can do about it." I felt an almost pleasant tingle of fear.

S-10 does not dissolve in water, or strong acid, or strong alkali. We had to pass it down a hot Monel metal tube to break it down so we could detect it in the conductivity cell. I wondered how such an inert molecule could do so much damage. The molecular structure is $F_5S\text{-}SF_5$, two identical twins joined together. I came up with a theory—*disproportionation*: one goes up and one goes down. Somehow in the lungs S_2F_{10} disproportionates into SF_6 and SF_4. SF_6 is stable. SF_4 is a reactive beast. It tears water apart to make four molecules of hydrofluoric acid and one molecule of sulfurous acid (H_2SO_3).

One of our tasks was to manufacture enough S-10 for the army to test in a small bomb. Associate Professor of Inorganic Chemistry Don Yost [PhD '26] had come up with a relatively high-yield synthesis that used the large fluorine generator in 121



Some of Caltech's chemistry faculty in 1937. Rear row, from left: Howard Lucas, associate professor of organic chemistry; Arnold Beckman (PhD '28), assistant professor of chemistry; Bruce Sage (MS '31, PhD '34), assistant professor of chemical engineering; Stuart Bates, professor of physical chemistry; James Bell, professor of chemistry; and Yost. Front row: Dickinson, Linus Pauling (PhD '25), professor of chemistry; and William Lacey, professor of chemical engineering.

Gates—up a flight of steps and down two hallways. John and Arthur Stosick [PhD '39] developed the procedure, and they manufactured some S-10 every day. The dangerous job of transferring the S-10 from the small bulb of each day's work was done only by Professor Dickinson or, sometimes, by Dr. Stosick. It took more than a month to fill a large bulb in Room 65 with the amount of S-10 the army needed.

The army came to collect the gas in their own glass flask attached to a carrying rack. The rack was too tall to fit in the hood, so Stosick clamped it to a heavy wooden desk. The S-10 was transferred by vacuum distillation: we connected the army bulb to the Caltech bulb, evacuated the system, and cooled the army's bulb to draw the S-10 over. Stosick mounted a large-mouth Dewar containing liquid air on an automobile jack, and slowly turned the crank. Upon contact with the army's bulb, some liquid air vigorously boiled away and formed a cold fog, which settled to the floor, spread out, and evaporated. Dickinson watched the S-10 in the hood and told Stosick at what rate to raise the liquid air. The whole process was over soon. The glass line to the army bulb was sealed off and disconnected; Dickinson was a master glassblower. Stosick slowly lowered the Dewar and removed it. They took off their glass-blowing goggles, removed the gas masks from their belts, sat down and smoked one cigarette after another.

We had a local telephone in the laboratory, but to make an outside call, Dickinson had a private line in his office nearby. He announced: "I'm going to telephone them to come pick it up. Art, why don't you take a walk outside and take it easy for a while." He added emphatically: "Everybody else stay away from it." A few minutes later, he came back: "They won't pick it up until about two p.m."

The laboratory was calm. Stosick was fabricating some electronic device. We did most of our analyses by ingenious circuits he had designed. Dickinson sat at his desk, busy with paperwork. John and I began work on one of the "poofs." The S-10 stood on the desk about half a meter from the hood and about three meters from John and me.

After lunch, I returned early, bypassing undergraduate students as they lined up to get into their one o'clock class. Dickinson and Stosick, talking together, got back somewhat before two o'clock. I heard loud and then soft exclamations from Dickinson. "*It's all gone*. Cracked. Hours ago, probably. I'll go call the army."

Dickinson told John and me, "They didn't anneal their glass bulb. It cracked and leaked. It's all gone."

Fear grabbed my throat and guts, my heart raced in irregular thumps. In my mind John was saying, "Colorless, odorless, and four times as poisonous as phosgene." Then I glanced quickly at the others. They showed no fear, and so I concentrated on not showing mine. I thought, "This is how soldiers can jump out of the trench together and rush at the enemy machine gun."

Dickinson conjectured that it probably had happened during lunch, because we hadn't heard anything. Furthermore, he said, the laboratory had excellent ventilation, air came in at the middle and went out the hoods, the S-10 had been close to the hood, and it probably had gone up the stack. Dickinson told John and me to go up to Gates and make another batch of S-10, while he and Art would call the army, return to clean up the laboratory, and carry on the tests John and I had started.

I recalled a preacher who had said he was once asked what he would do if told he would drop dead tomorrow, and he listed, one after the other, exactly those things he planned to do anyway. The preacher had said one should live each day as if it were the last. I silently noted that Professor Dickinson was advocating such a course for this day.

My thoughts continued along another path: Professor Dickinson had said that the ventilation system had carried it all up the hood, but I didn't believe it. Dickinson smoked much of the time and could blow elegant smoke rings. I had watched many a smoke ring distort, fold, stretch out, and drift around the lab until it became so dilute that it could no longer be followed. Thus, I knew that the laboratory air did not march from the fresh-air input to the hoods. It swirled and swooped all over the lab. And when the liquid-air fog had formed on the table just in front of the hood, the fog fell to the floor—it didn't go into the hood. As we walked through the hall toward Gates, I avoided looking directly at John and hoped John was not looking too closely at me. As we passed graduate-student friends in the hall, we did not speak to them. By the time we reached 121 Gates, the alarming fear of instant death had faded into a low-intensity, sad, sick feeling.

The fluorine maker was an awkward-looking machine. A large pot contained sodium fluoride

(NaF) and hydrofluoric acid. The pot had to be brought up to a moderately high temperature to melt the salt and acid mixture, and a direct electrical current was applied to form fluorine gas, F_2. The F_2 was diverted to a reaction cell that was heated on one end and cooled on the other. There the fluorine reacted with sulfur to make harmless SF_6 and highly poisonous S_2F_{10}. Finally, the freshly prepared S-10 was slowly vacuum distilled to remove trapped HF and SF_6. The process took three hours to complete, and then the system had to be cleaned up.

On the laboratory bench were two large open jars of a paste to be used if any HF got on a person's skin. HF causes deep ulcers that take a long time to heal. Safety called for rubber gloves and face shields, but operating the machine called for fast, free fingers and a clear sight of what went on. We wore goggles, but took our chances with bare fingers even though the machine sometimes spat out specks of hot sodium fluoride, smoking with HF. To see if the machine was making fluorine, John would pick up a small cotton ball with long metal tweezers, moisten it lightly on one side with alcohol, and hold it up to the exit. When the cotton ball caught on fire, it meant that fluorine was coming out.

We had been proceeding with this particular batch for a while when Professor Dickinson knocked on the door, then opened it with his key and asked how things were going.

John replied that there were no problems.

Later Dickinson checked on us again.

When we were done, John put the sample in a padded box, and we took it down to 65 Crellin.

I recalled from one of the hazards sheets that "the first symptom of acid poisoning of the lungs shows up as a bitter taste in the mouth when one smokes a cigarette." From the dense haze in the room it was clear that Stosick and Dickinson had been smoking heavily, and they continued to do so. Dickinson said, "We have had a long day, but we need to stick around until six at least. Have a seat, boys. If I had some beer I would offer it to you."

Safety called for rubber gloves and face shields, but operating the machine called for fast, free fingers and a clear sight of what went on.

Then, uncharacteristically, he began to talk about himself.

I silently noted that somebody else had been thinking that afternoon. Reminiscences continued as did the smoking, with an occasional set of smoke rings. No one complained of a bitter taste. No one referred to why we were sitting around waiting, nor to any aspect of the S-10 spill.

Eventually, a telephone call came. Captain Everett, the army doctor, wanted to see me. He listened to my chest, front and back. "Lungs completely clear. No change at all. You and John were closest to the leaking bulb. Since you are clear, the others must be also. Tell me, where do you think that gas went?"

With a detached sense of relief I said, "Right up the hood, I guess."

Everett asked jovially, "Well, Hal, were you frightened today?"

"Part of the time."

The doctor chuckled, "Part of the time, eh. Well, I was frightened for the four of you all afternoon."

The army asked us to replace the S-10. Within a month, we did it. Professor Dickinson selected a Pyrex bulb, added attachments for conveniently filling and emptying it, then thoroughly annealed it. He packed the full bulb in an insulated wooden box made to his specifications by the carpenter shop. We were allowed to follow him to his automobile, but he would not let anyone go with him to deliver the material.

It was another month before the army got around to testing it. The explosion took place in a barn. The rats and goats died as expected, but the explosion produced a strong sulfurous odor, wiping out the gas's advantage of being odorless. Interest in S-10 ended after this test. Our regular work continued, and soon expanded to include the cyanides.

* * *

In July 1942, a group of university scientists who were working on chemical-warfare problems met in Evanston, Illinois; this group later became NDRC Divisions 9 (Chemistry) and 10 (Adsorbents and Aerosols). (By war's end, the NDRC would have 20 divisions; Division 10 alone had 77 projects going in 18 universities and five industrial laboratories.) Many potential chemical-warfare gases had already been identified, synthesized, tested for toxicity, and tested for how well gas masks stopped them. The group discussed what else should be done. Professor Yost spoke forcefully to the effect that, having solved our most urgent defensive problems, we should learn how to carry out offensive gas warfare if that was to our advantage. He added that we did not know how performance was affected by wind, temperature, time of day, cloudiness, and terrain. Professor Wendell Latimer of the University of California at Berkeley supported Yost's position, and it became the policy of Division 10, which was formed soon after.

Our first job was to learn something about meteorology and to design and build portable instruments to measure air temperature and wind speed from heights of zero to four meters. Such ground-level meteorology was called "micrometeorology." We were to obtain measurements near cities from the Mexican border to San Luis Obispo to see if there were any regions especially vulnerable to gas attack. Professor Latimer's group was to make similar measurements from San Luis Obispo to the Oregon border.

Measuring air temperature out of doors is not a trivial task. If a bare thermometer is placed in the air, it will absorb sunlight and read too high. If it is shaded from the sun, it will absorb infrared rays from the ground and again give a spurious temperature, too high or too low. If the thermometer is shaded above and below, the shades may absorb sunlight or ground heat and change the local air temperature. To obtain true air temperature, we mounted a small copper-constantan thermocouple within two concentric tubes of thin metal and rapidly aspirated air through the tubes and across the thermocouple. We built a mast of four-inch aluminum pipe in three detachable sections, affixing the thermocouple tubes at heights of one-third, one, two, and four meters. We used a direct-current motor powered by two storage batteries to drive a large vacuum cleaner that sucked air through the tubes and out the base of the mast. These masts were later manufactured by the Wheelco Instrument Company for other researchers.

To measure wind speed, we bought a set of commercial cup anemometers, but they gave results in terms of numbers on a dial. It was a nuisance to use a stopwatch to start and stop the anemometer readings, and then to subtract the numbers in a notebook. Professor Yost's group had the Caltech machine shop put three aluminum cups on a vertical axis that spun on a jewel mount, and at every complete round a small needle contacted a drop of mercury to give a brief electrical pulse. Art Stosick hooked together a set of vacuum tubes that counted the pulses to give a continuous recording of the wind speed. The anemometers were mounted on a mast of their own, at one- and two-meter heights. The Lane-Wells Company later manufactured 36 of these instruments for other groups.

The machine shop also built a few British "gustiness meters." This was a wind vane free to move up and down and left to right. It had a lightweight pen on its downwind side, which put ink marks on a sheet of curved paper. We let it operate for two minutes, and the pattern of scribbles indicated the degree of horizontal and vertical turbulence.

Otvos (on ladder) and Johnston set up the micrometeorological gear near Rosamond Dry Lake in the Mojave Desert some time in 1943. The thermocouple mast and its suction system are visible behind Otvos' ladder. The center mast has two anemometers on it, and the mast in front of Johnston carries a British gustiness meter. Note the Buick in the background.

New cars were not available for nonwar work, but Dickinson was able to buy a new 1942 Buick station wagon for our project. It had real leather seats and real wooden panels on the outside. It was an impressive automobile. It had two fold-up seats between the front seat and regular back seat, so that the car could carry eight people. The back seat could be folded down to give a large storage area, and there was a storage rack on the roof. Our entire meteorological station could be dismantled and stored in the back of and on top of the Buick.

We measured local conditions at the beach, in a desert, on a site up the side of the mountain, and at various spots in the city. We quickly discovered what meteorologists already knew: during much of the year there was a pronounced breeze from the ocean to the interior during the day. The cold ocean air slips like a wedge below the warmer land air, thereby creating a large-scale temperature inversion that would trap poison gas or, nowadays, smog. Greater Los Angeles was especially vulnerable to gas attack.

We could actually see these inversions on a small scale. We'd pack red phosphorus in cardboard cylinders about three centimeters in diameter and fifteen centimeters tall; when ignited by a thin magnesium ribbon as a fuse, each one produced a copious amount of smoke for a few minutes. During a temperature inversion, the smoke would boil up a few feet above the ground and then slowly spread gauze-like in broad flat sheets with little vertical mixing. We photographed these patterns of motion. Thus we had four different ways to characterize turbulence: temperature and wind speed profiles, the gustiness meter, and smoke patterns.

* * *

Professor Dickinson suggested we reduce the size and complexity of our gas-analyzing system so that it could be used in the field. In the laboratory, we had a 20-liter carboy of distilled water on a shelf. This water flowed by gravity along the inner surface of a half-meter-high burette and scrubbed soluble gases out of a counterflowing stream of air. The water then flowed through a cell that measured electrical resistance and recorded it on an Esterline Angus meter. It appeared to be a difficult job to miniaturize this system.

John Otvos did most of the work, but I made one contribution. I had grown up in rural northern Georgia, so Dickinson jokingly said I should use some feature of watering chickens to do our job. With that challenge, I played around with water and gas flows for a couple of days. I used a long, straight glass tube instead of a burette and attached an open T-tube at the upper end. I tilted the long tube slightly and let water slowly flow into the upper end. By controlling the tilt and by applying suitable constrictions to the incoming and

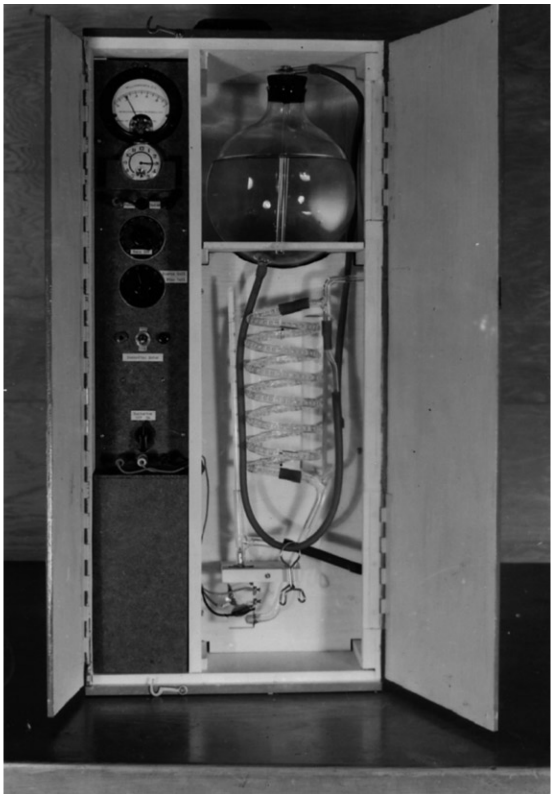
A red phosphorus flare going off in a stiff breeze at Rosamond Dry Lake, part of Edwards Air Force Base. In this vastness Yost's group could release moderately toxic sulfur-dioxide gas that they traced by chemical analyses, melding the dispersion data with Dickinson's micrometeorology.

outgoing flow of water, I could get air to enter the T and be picked up by the down-flowing water. With more adjustments I could get the incoming air to break up into round bubbles, and the flow down the tube looked like a string of pearls. The flowing water sucked in outside air, and the bubbles allowed any soluble gases to dissolve. This eliminated the need for an air pump, and replaced the heavy carboy with a compact two-liter bulb.

John replaced the straight tube with a spiral. He and Art Stosick designed and had the shop build a miniature resistance-measuring cell. The woodworking shop built a box that held the spiral tube in one compartment and the electronics, which Art developed, in another. A dry-cell battery provided enough power to run the machine for several days. John named the new machine "Egbert." More than 100 Egberts were eventually manufactured by the Caltech wood, glass, and machine shops, and they saw widespread use at several NDRC test sites.

* * *

In 1943 the United States and its allies began the difficult task of retaking islands in the southwest Pacific from the Japanese. These tropical jungles were, of course, different from the deserts of California and Utah and the pine forests of Idaho, where the NDRC and the Chemical Warfare Service (CWS) had tested chemical weapons and studied the travel of gas clouds. A 50,000-acre site in central Florida was chosen—the Withlacoochee Development Project of the Soil Conservation Service, U.S. Department of Agriculture. That November, Dickinson, Otvos, new recruit Bob Mills [MS '48], and I, along with Phil Hayward [PhD '49], postdoc Mike Kraus, and Ted Gilman [MS '46] from Yost's lab, packed 30 Egberts and assorted other gear into the Buick and a two-ton Ford flatbed truck and drove cross-country to Bushnell, Florida, about nine miles from the test site. There we joined civilians from other universities and a CWS detachment from the Dugway Proving Grounds in Utah. The commanding officer was Captain Jake Nolen, who was in his early 30s and had a PhD in chemical engineering from MIT. The NDRC group and the army officers lived in rented rooms, apartments, and houses in Bushnell. We had our meals at the local restaurant except during field tests, when we got in line and ate army chow. (After two years in California, I had come to dislike much Southern cooking, where vegetables were typically boiled for hours in the presence of salted fat hog-belly. I sometimes shook a heavy dose of pepper at spots in the food—under lifted portions of the mashed potatoes, and in turnip greens and meat—and then spun the plate. I would never know as I took a bite whether it would taste like black pepper or hog fat. That provided variety.) The enlisted men lived in tents in a flat area just south of town.



Egbert. Outside air is drawn into the glass T at upper right of the glassware compartment by water flowing from the two-liter flask up top. As the string of bubbles proceeds down the spiral glass tube, any water-soluble war gas gets scrubbed out. This changes the water's conductivity, which is measured through the wire leads that are connected to the glass cell at the bottom.

At the test site, soldiers marked the ground with whitewash to show where the bomb would be located and where they would place goats. We defined a grid around the bomb where the Egberts and the hot-wire anemometers from Berkeley would be placed. We set up one micrometeorolog-

ical station in rough meadow terrain, and another in a grove of tall trees, which towered high above our thermocouple mast. For six weeks, we had three to five tests every week. The gases included phosgene, hydrogen cyanide (HCN), and cyanogen chloride (ClCN). Immediately after the bomb or bombs exploded, we put on gas masks and went in to check the instruments. Then, after a prescribed time, we'd return and turn off the recording meters, label and remove the chart paper, tightly wrap the instruments with waterproof canvas, and place them on the truck. Any malfunctioning instrument was brought back to town for overnight repairs. In one experiment, an air-dropped bomb made a direct hit on one of the Egberts.

I naively exclaimed, "On the basis of the second law of thermodynamics, I can say for sure that is impossible."

"Well, I don't know nothing about the second law of ther-mo-dy-namics, but I do know a dead goat when I see one." The major laughed heavily and was joined by some others.

NDRC scientists measured the travel of the war gases by concentric circles of automatic chemical samplers. Military personnel staked out goats on a grid of their own. The goat detail was handled by an old-time major from the chemical-warfare corps. After one large test, the chemists said that no significant amount of gas got beyond the 100-yard circle. But two goats on the 200-yard circle died shortly after being brought back to their pens. There was a conference to decide why we had the discrepancy between methods. The chemists defended their instruments. The major suggested that the gases might have passed the 100-yard circle in a highly dilute state and then come back together to be concentrated enough to kill the goats on the 200-yard line.

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Captain Nolen probed further: "Tell me, Major, sir, how many goats were on that 200-yard circle?"

"Eight," said the major confidently.

"How many died, sir, after they were brought back to their pens?"

"Two," was the reply. "Two of them died during that night."

"How many goats are in the pen that have not been exposed yet, sir?" Captain Nolen asked.

"How many do we have, Sergeant?" the major asked his assistant.

"They's only fifteen left now, sir. We're getting sort of low on goats," the sergeant said.

Captain Nolen, who signed every purchase order, including those for goats, pushed on: "Sir, how many goats have died in the last few days that were *not* exposed in the field?"

"Why, none, I don't think," the major exclaimed. "We haven't had any of them die recently, have we Sergeant?"

"Sir, the last few days we've had a right smart of Texas fever in them there goats," the sergeant said, nervously glancing back and forth between the captain and the major.

Captain Nolen pursued the point directly with

Opposite: The NDRC Division 10 group on San José Island in August 1944. Back row, from left: George Doyle (MS '48); George Cleland (PhD '51); Jim Pitts, of Northwestern; Ted Gilman (MS '46); John Thomas, from UC Berkeley; Lewis McCarty, University of Rochester; and Bill Roake, of Northwestern. Middle row: Bob Brinton, of Northwestern; Bill Shand (PhD '46); Bob Mills (MS '48); Phil Hayward (PhD '49); Pat O'Conner, from the University of Illinois; Clive Countryman, from Berkeley; Chet O'Konski and J. M. Thomas, both from Northwestern. Front row: R. J. Grabenstetter and Dave Volman, of Northwestern; Dickinson; Francis Blacet, the UCLA professor in charge of the group; Bill Gwinn, of Berkeley, the second in command; Jack Roof, of Northwestern; Otvos; and Caltech postdoc Mike Kraus.

the sergeant: "How many goats left in the pen died since the test four days ago?"

"They's been six of 'em died from Texas fever these last four days," the sergeant said.

Captain Nolen spelled out the conclusion: "Six out of fifteen died from Texas fever in four days, and they never got near the field. That's even more than two out of eight on the 200-yard circle. I think, sir, they probably died of Texas fever too."

The second law of thermodynamics was saved.

* * *

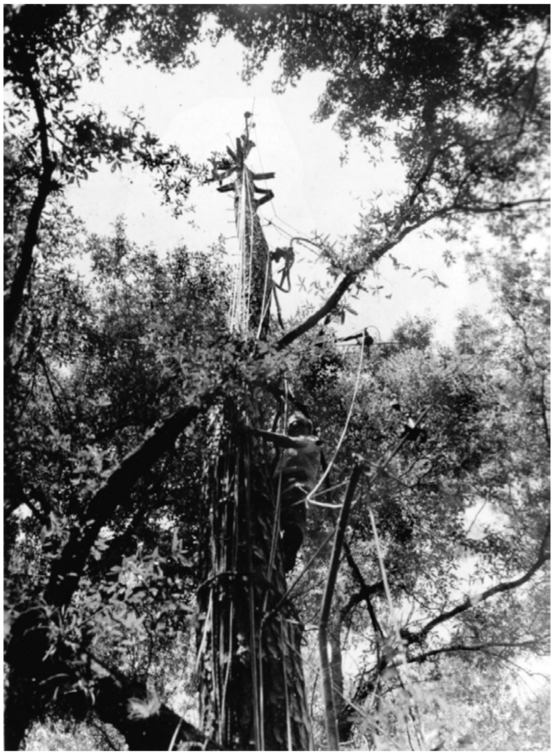
It was soon concluded that the semitropical forests of Florida were not a satisfactory substitute for the jungles of the southwest Pacific, and in early 1944 the Division 10 work was moved to San José, an uninhabited island off the Pacific coast of Panama. I remained in Bushnell, where the work switched to "persistent" gases, such as mustard, under Division 9. The persistent-gas group were mostly organic chemists, under the direction of Carl Niemann, another Caltech professor.

At room temperature, mustard is a heavy, oily liquid, but it has some vapor associated with it. Mustard vapor, if breathed, destroys lung functions in a manner similar to and more severe than phosgene. A person wearing a gas mask can still be killed by vapor absorption through the skin. A droplet of mustard is irreversibly absorbed in less than a minute, producing large, crippling blisters. Heavy contact of the liquid on skin can be rapidly fatal. Contaminated ground or logs remained dangerous for weeks or more, as certain poachers discovered.

Since our observations only went up a few meters, while the forest canopy was about 20 meters high, I asked Captain Nolen if we could obtain a steel tower that would go up above the treetops. He said we had been assigned one, but it had been erected in Dade City and there was no chance of our getting another. I had noticed some tall pine trees that went above the canopy of the hardwood forest—could we strip one of them and put in steel climbing rods like those on telephone poles? A suitable one was found about half a mile from the test area, and in short order it was instrumented.

Under clear skies, our meadow station recorded strong temperature inversions at night, and during the day showed an "unstable lapse rate," that is, the air temperature decreased with height. This is convection at work—air cools as it rises—and had been well documented over grass lawns, cow pastures, barren hillsides, and desert soils. After we converted the tree into a micrometeorological tower, I wanted to make measurements there 24 hours a day, whether tests were being conducted or not. I was curious about the magnitude and timing of temperature inversions in the forest and of the relation between meadow and forest. I made arrangements with Captain Nolen to have recharged batteries delivered twice a day at both the meadow and forest stations.

We recorded continuous measurements through-



The micrometeorological tree tower, deep in the forest some 100 yards off the main road into the test area, had vacuum-aspirated temperature sensors at 0.3, 5, 10, 15, 20, and 25 meters above the ground; anemometers at 2, 5, 10, 15, and 23 meters; and a wind vane at 25 meters. All the instruments were mounted at least a meter away from the tree trunk on stubs of branches or on steel rods. The trunk itself was festooned with wires and vacuum lines leading to a small shelter on the ground that held the industrial vacuum cleaner, the two heavy-duty truck batteries that powered it, and the recording and calibration equipment. Note the soldier on the trunk about one-third of the way up from the bottom of the photo.

Micrometeorological tests resumed at Rosamond Dry Lake in the late spring of 1945, using the new gustiness meter. Here Mills sets out an anemometer early in the morning.

Observations were made at three locations: on a smooth, flat, vegetation-free surface; on a surface of cracked dry clay with upturned edges about an inch high; and in desert brush. Mills and Johnston commuted between the stations on a bicycle, and in the interim between the two camping trips, they built a sail for it. Here Johnston sails across the cracked clay.

out February and March 1944, when there were few leaves on the deciduous trees, and May and June, the time of maximum foliage density. The temperature differences between air in the forest canopy and the air above the canopy showed the same pattern as the meadow: inversion during the night and unstable lapse rates through the day. But from 0.3 to 15 meters above the ground surface, the patterns were totally different. During May and June the forest air showed a temperature inversion every hour of the day and night. During February and March, air in the forest had an unstable lapse rate from about 1130 to 1530 Eastern War Time, and a temperature inversion at all other times. The daytime inversion in the forest was a mystery.

We explained it as follows: sunlight absorbed by the leaves warmed canopy air to temperatures higher than the air above to produce an unstable lapse rate above the canopy during the day, as in the meadow. This sun-heated air in the canopy was warmer than the shaded air in the lower forest, which constituted the daytime inversion. During the early morning and late afternoon, foliage cut out almost all of the slanting sunlight, and the inversion was large. When the sun was nearly overhead, some sunlight penetrated to the ground to reduce or break up the temperature inversion during the middle of the day. We concluded that the denser the canopy, the greater the inversion, which would probably be highly important in tropical jungles. Poison gas clouds would persist much longer there than in open areas.

We ran into one difficulty with our overnight recordings. Well into our program, we found one or more of the ink traces would go dry in the early evening. We became doubly careful to fill the ink reservoirs, but we still lost some records. I drove out one night, brought along a strong flashlight, and set up a chair near the recorders. For a few hours I heard owls and other night noises, and then I heard a rasping sound in our box of meters. I turned on the flashlight and found the biggest cockroach I had ever seen. It was as big as a cigar and about that color, five or six inches long, and when I opened the case and tried to catch it, it flew away on big whirring wings. Later, someone told me it was a “blabberous cockroach.” The next day my sergeant took a soldier skilled in carpentry to the site. He installed two tight wood-and-screen doors, and stopped the other possible entries with putty. He did a good job, because we had no more trouble with ink-guzzling cockroaches.

* * *

Contour maps of the spread of mustard-gas clouds correlated best with the gustiness-meter readings. But after a two-minute exposure, the center of the paper was coated by a coil of overlapping red ink tracks. We measured the widths of the “average extreme excursions,” but it was difficult to say what the record meant. We needed a quantitative measure.

I designed a two-vaned instrument that measured vertical gustiness. One vane aligned the system with the wind; the other, streamlined to give a laminar flow around it, swiveled up and down on high-quality jewel mounts. The tangent of the angle of the vane from the horizontal plane was a measure of the instantaneous gustiness. Electrical contact was maintained with the vane, and the data could be read off a milliammeter or recorded on chart paper. The machine shop at Caltech built several copies of our new meter.

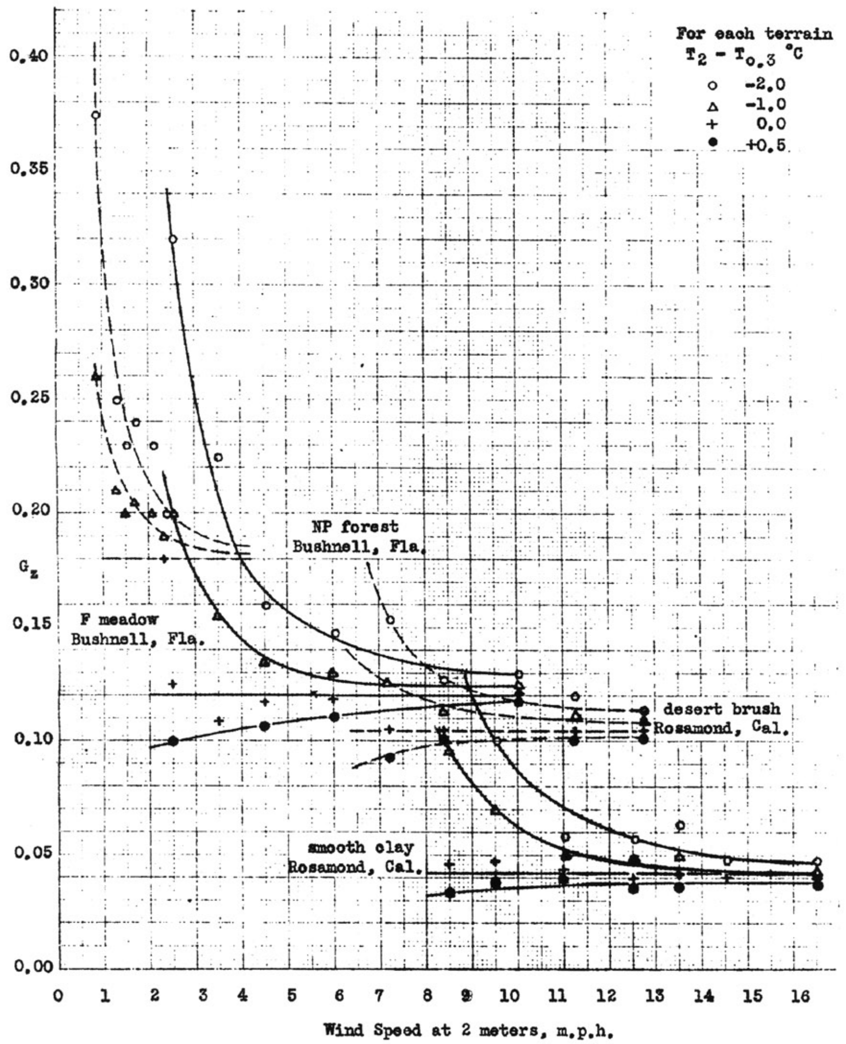
The British had developed a statistical diffusion theory to predict the three-dimensional dissipation of a gas cloud downwind of an explosion. Their spreading parameter, *n*, determined how strongly the dosage near the source decreased with distance, and we felt that should depend strongly on vertical turbulence. The British had found that the "R-value," the ratio of wind velocity at heights of two meters and one meter above the ground, was a good measure of vertical turbulence over smooth lawns, but we found it was not good in forests or rough meadows.

Bob Mills returned to Caltech from Panama in September 1944 and worked on Professor of Chemistry Linus Pauling [PhD '25] and Senior Research Fellow in Structural Chemistry Robert Corey's rocket-propellant project. In the late spring of 1945, I visited Pasadena. Bob and I borrowed a Caltech panel truck, and on two trips we camped out at Rosamond Dry Lake for several days, making measurements with our new gustiness meter and other instruments. It was windy day and night, especially in the afternoon, and we noted that our eight-foot ladder blew over when the wind reached 25 miles per hour. We slept outside on army cots. The truck was tall enough for us to stand up in, and we used it for our kitchen.

We were able to verify that the vertical gustiness correlated inversely with the spread of a gas cloud as it moved downwind. Furthermore, plotting the gustiness versus wind speed revealed a parameter, which we called "mechanical gustiness," in air of zero temperature gradient, i.e., where the temperature remained constant regardless of height. This parameter was the same for all wind speeds but different for each class of terrain: largest in the forest and jungle, much less in rough, bushy meadow, somewhat less again in desert brush, and very low on the dry lake bed. In other words, it measured the site's roughness.

Robert Merrill of the University of Chicago, Bob Mills, and I wrote this up in our project report in the summer of 1945. It was submitted to Major Nolen (he had been promoted) just after the atomic bombings of Hiroshima and Nagasaki that August. The report was classified Confidential, filed away, and forgotten.

So was it worth it? Professor Dickinson was a brilliant scientist, an artist, and a generous, liberal person. He felt good about working with terrible poisons to provide better gas masks. When the NDRC shifted from defense to offense, he supported the new emphasis, accepting that a recognizable capability to go on the offense was a necessary part of defense. Early in 1942, he said to me, "We are guarding a bridge that may never be attacked; we hope it will not be. If it is not attacked, our work has succeeded." □



This plot from the Bushnell project's final report shows vertical gustiness (G_z) versus wind speed at two meters off the ground for four different types of terrain. Each line represents the temperature difference between 2.0 meters and 0.3 meters above the ground: open circles are a difference of -2 °C and triangles are -1 °C (unstable lapse), crosses are 0° (thermally neutral) and solid circles are 0.5° (inversion). The thermally neutral lines are horizontal—the gustiness does not change, regardess of the wind speed. This is the "mechanical gustiness," which is caused by frictionally induced turbulence and is thus related to the terrain's roughness. The other component of vertical gustiness is convection-driven, which is greatest at low wind speeds. As the wind picks up, the convection cells are distorted and eventually destroyed, and the gustiness approaches the thermally neutral value.