Review

WINTER 1971

OAK RIDGE NATIONAL LABORATORY

STATE OF THE LABORATORY ISSUE

THE COVER: Two residents of Midland, Michigan, compare impressions on a visit to the Laboratory to learn something about nuclear power reactors. The story of how civic leaders of this town are dealing with the problems involved in the siting of a reactor nearby is told on page 25, "The Midland Encounter."

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Review

OAK RIDGE NATIONAL LABORATORY

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FEATURES

OAK RIDGE NATIONAL LABORATORY
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State of the Laboratory — 1970

By ALVIN M. WEINBERG

WE IN THE NUCLEAR COMMUNITY have been comfortable in the belief that our work — providing a great new source of energy — is an unmitigated and obvious good. It therefore comes as a perplexing shock to realize that the nuclear community is confronted with what seems to be a crisis of public confidence. Opposition to nuclear energy, which was first expressed publicly seven years ago by David Lilienthal, has mushroomed. Fanned by well-meaning, but in my opinion poorly informed, scientific polemicists, articulate, though not large, segments of the public are casting doubt on aspects of nuclear energy that we had long since taken for granted. Where we insist nuclear energy is clean, our critics claim it is dirty. Where we insist nuclear energy is safe, our critics claim it is unsafe. Where we insist it is needed for our ultimate survival, our critics say it is unnecessary. As one professional ecologist from Amherst College said in the *American Scientist* (p. 618, November–December 1970), "... the technological solutions ... [based on nuclear breeders] to sustain a world population of 20 billion people are far more immoral than were the decisions to use the A-bomb and H-bomb."

I think I do not exaggerate the intensity of feeling that has developed with respect to nuclear energy, nor the profound consequences to our Laboratory, not to say to the world, of a loss of confidence in nuclear power. It is for this reason that this year, rather than simply reviewing what has happened at ORNL, I shall focus my remarks on the attack on nuclear energy and our response to it. I shall review what we at ORNL have done and will continue to do to validate nuclear energy: how we establish the facts and the uncertainties about our new source of energy, and thus try to shed light on a debate that has become acrimonious and noisy.

We at ORNL have a particular responsibility in this matter. The main American line of reactor development, the pressurized water reactor, had its origins here at ORNL some 25 years ago. There are still many at the Laboratory who remember the lively discussions with H. G. Rickover over how best to propel a nuclear submarine, and how the pressurized version of the water-moderated MTR won out. Thus, insofar as the debate about reactor safety centers around specific characteristics of water-moderated reactors, we who helped set this course cannot properly avoid being involved.

But there is a larger issue. It has been our Laboratory, perhaps more than any other, that has concerned itself with the broadest and longest range implications of nuclear power: the idea that with an inexhaustible source of energy man could free himself from material want, essentially forever. Thus in the delicate and uncertain balance of risks and benefits that must be struck for every modern technology, and particularly for nuclear power, ORNL has been most persistent in pointing to the ultimate benefit: nuclear energy is needed, ultimately, to forestall Malthusian catastrophe. It therefore falls to us to weigh, and reweigh, as new facts become available, the other side of the balance: the risks in our new energy source. Have we been properly responsible in assessing these risks? Have we addressed ourselves soberly and scientifically to ferreting them out? Have we been adequately imaginative and inventive in setting right any engineering deficiencies? I believe our record on all these questions has been excellent; and it is about this record, particularly as it has developed during the past year, that I wish to speak this evening.

The Attack on Nuclear Energy: Low-Level Effects

The first, and perhaps most violent, attack on the nuclear enterprise is now centered on the claim that very low doses of radiation — such as 170 millirems per year, which is the average dose allowed to the general public according to 10 CFR 20 (*Code of Federal Regulations*, Title 10, Part 20, "Standards for Protection Against Radiation," U.S. Government Printing Office, 1970) — will in fact do great harm. If one assumes that induction of cancer is linear down to the lowest doses and dose rates, and if the entire United States population were subjected to 170 millirems per year, then one calculates, though with great uncertainty, that as many as several thousand additional cases of cancer *might* be caused annually. Oak Ridgers, notably J. B. Storer and K. Z. Morgan, have spent many hours during 1970 pointing out the uncertainties and weaknesses in these calculations. Most vulnerable is the assumption that all, or nearly all, people would ever receive anything like 170 millirems per year; and much of the argument hangs on this point. But the argument gets highly political and emotional at this stage. The relevant point as far as ORNL is concerned is: Are there in this part of the controversy truly scientific issues that can be settled by the methods of science?

With respect to the question of linearity of response with dose, the answer seems clear to me. As far as genetic effects are concerned, if the response were linear with dose, Marvin Kastenbaum, collaborating with members of the Statistics Department, has estimated that to find a one-half percent effect (with 95 percent confidence) at a total dose of, say, 1 R would require exposure of eight billion mice. Clearly the seemingly simple question, Does 170 millirems per year of radiation cause genetic damage? can be stated in scientific terms, but science cannot provide a simple yes or no answer by direct experimentation. This is the dilemma that faces us in all of the arguments about low doses.

There is evidence that repair mechanisms exist, even for hard radiation. Following the discovery by R. B. Setlow and his co-workers of an enzyme that repairs DNA damaged by exposure to ultraviolet, similar repair systems have been found in cells exposed to x radiation. But the existence of a single repair mechanism cannot be said to close the issue. I repeat what seems to me to be the only adequate statement: that, as far as genetic damage is concerned, there is no way of really ascertaining whether or not the dose-effect curve is linear down to very small doses. Under the circumstances the linear hypothesis has been accepted as the most prudent.

With respect to the effect of dose rate, W. L. Russell showed some twelve years ago that several hundred R of x and gamma rays delivered at 9 millirems per minute produced many fewer mutations both in males and in females than the same doses delivered at 90,000 millirems per minute. And during 1970 Russell and his colleagues have found that in female mice given 400 R total dose, more than twice as many X-chromosome losses occur when the dose is delivered at 80 R per minute as when it is delivered at 0.6 R per minute.

To keep the record straight, however, it should be mentioned that, though the mutation rate in females continues to diminish with decreasing dose rate, the frequency in males at 1 millirem per minute is no lower than at 9 millirems per minute. Nevertheless, the evidence taken as a whole seems to point to some reduction in mutation rate with decreasing dose rate, a fact that was not taken into account when permissible levels were established.

As for somatic effects, notably induction of cancer in mice, John Storer, J. M. Yuhas, E. B. Darden, G. E. Cosgrove, H. E. Walburg, and L. J. Serrano have been continuing the large-scale experiments undertaken several years ago under the leadership of A. C. Upton.

The first of these large-scale experiments, which is still nearly two years from completion, is designed to determine precisely which diseases and which cancers can be induced in mice by radiation exposure. Large numbers of animals have been exposed to as low as 10 rads total lifetime dose in an attempt to establish empirically the shape of the various dose-response curves. The animals are maintained under very carefully controlled conditions to avoid as far as possible interactions with other environmental variables; and we expect these experiments within the next few years to shed additional light on the somatic effects of low doses of radiation.

To summarize, biological effects at these extremely low levels and low dose rates are undoubtedly very small. Whether they are truly zero we cannot say; and therefore, as a matter of prudence, we assume the linear hypothesis. In making this assumption, we most probably do not underestimate the risk. Beyond this, there is little science can say about low-level radiation hazard.

What, then, can a scientific establishment like ORNL do to clarify the issues and minimize the risks? First, we can, as engineers, design devices that reduce to an absolute minimum radioactive effluents; but there will always be some effluent and some hazard, however small. We must therefore also seek, in whatever way, to minimize or counteract the somatic and the genetic consequences of chronic doses of radiation.

As for counteracting genetic damage, one idea would be to terminate pregnancy if there is evidence that the fetus is genetically abnormal. How this might be done is illustrated by recent work of J. D. Regan, R. B. Setlow, and W. L. Carrier on xeroderma pigmentosum. This is a very rare disease caused by absence of the enzyme that normally repairs DNA that has been damaged by radiation. Individuals suffering from this genetically caused anomaly are extremely sensitive to sunlight, and they often die of skin cancer at an early age. Now Regan, Setlow, and Carrier have developed an assay which enables one to determine, in cells from the fetus, whether the repair enzyme is present. Where it is absent, there is strong evidence that the baby will suffer from xeroderma pigmentosum; and the fetus could be artificially aborted. Regan, Setlow, Carrier, and their colleagues are working out the details of their assay method in the expectation that it will be used to test, in utero, the next fetus of a couple who already have had the tragic experience of bearing a child with this fatal disease.

This example illustrates how one might intervene in the future if a genetic anomaly is suspected; it is also a beautiful example of how basic research on ultraviolet induction of thymine dimers has led, by virtue of elegant experiment and insight, to a medical advance. Of course this approach will be useful only in a small fraction of genetic abnormalities. I mention it merely to illustrate the *possibility* of combating genetic defects.

The other objective is of course to cure or prevent cancer. Now the cancer problem is a bit like the thermonuclear situation: crests of optimism are followed by waves of pessimism, and it is often difficult to say just how seriously to take the current optimism or pessimism. My own estimate of the situation, which is much influenced by the views of Frank L. Horsfall, Director of the Sloan-Kettering Institute for Cancer Research and a member of our ORNL Advisory Council, is that we may indeed be in for a significant break in cancer. A very hot approach to cancer research now seems to be the tumor-specific transplantation antigen: cancer cells seems to display specific antibody signatures that distinguish them from normal cells. Therefore, if one can stimulate the body to produce antibodies that react to the tumor, one has the hope of protecting against the tumor.

Experiments directed toward these strategies of cancer therapy were begun in the MAN Program and are now also being conducted by the carcinogenesis group in the Biology Division. I mention one very suggestive set of experiments conducted by Joseph H. Coggin of The University of Tennessee in collaboration with Edrick L. Candler, Jr., and other members of the MAN Program. One theory holds that in cancer normal cells mimic their prenatal state; hence the antigens characteristic of cancer should be similar to those found in fetal tissue. If this is true, then fetal tissue ought to stimulate an immune response against cancer. And indeed, this is what Coggin, Candler, and their associates have found: that irradiated tissue from hamster, mouse, and human fetuses when injected into adult hamsters confers immunity against challenge with tumor cells in as many as 70 percent of the animals.

It would be foolish, not to say misleading, to suggest that these experiments might lead at any specific time to a cure for cancer, or that a cure for cancer is sufficiently close at hand to quiet, in the short run, the argument about biological damage from low radiation dose. Yet there is now before Congress a bill sponsored by Senator Ralph W. Yarborough that would launch a Manhattan Project to cure cancer. I would therefore insist that what we do at ORNL and elsewhere in resolving the sticky problem of cancer might well have strong implication, in the long run, for resolving the unending argument concerning the somatic effects of low-level radiation.

Turning to the contributions of our engineers to reduction of routine effluents from nuclear power plants and from reprocessing plants, I mention briefly three developments. First is the demonstration of a new radioactive iodine cleanup system at TRU which reduces the release of iodine there essentially to background, an improvement of more than 100 over the performance of a simple charcoal absorber.

Second is the "voloxidation" treatment of spent fuel, which sequesters 99 percent of the tritium and most of the iodine and rare gases into a small volume from which they are relatively easy to trap.

Finally I mention the demonstration at ORGDP that krypton and xenon are very soluble in freon, and therefore can be removed quantitatively from a gas stream by scrubbing the stream with freon at low temperature.

These developments at Oak Ridge, and similar developments elsewhere, justify our belief that in the future it will be possible at reasonable cost to reduce radioactive effluents from chemical processing plants essentially to zero. Thus parts of the nuclear cycle that typically have been responsible for most of the low-level release to the environment can now be completely cleaned up; I would expect that these new techniques will be incorporated into all radiochemical plants.

Another major development in the control of routine nuclear wastes is the decision to proceed with Oak Ridge's salt mine repository in Lyons, Kansas. The Atomic Energy Commission has allocated $25 million for this purpose, thus culminating some 13 years of work and planning at ORNL aimed at sequestering radioactive wastes, finally and forever. That ORNL is ready to move when public clamor is loud is a stroke of good fortune; the AEC General Manager, R. E. Hollingsworth, assures me that the salt mine is perhaps the most important project the AEC is now undertaking.

Our decision to go to salt for permanent high-level disposal is one of the most far-reaching decisions we, or for that matter any technologists, have ever made. These wastes will be hazardous for up to a million years. We must therefore be as certain as one can possibly be of anything that the wastes, once sequestered in the salt, can under no conceivable circumstances come in contact with the biosphere. What gives us such assurance about waste disposal in salt?

The primary reason is that the beds of salt, simply because they are still there, could not, since the Permian period (250 million years ago), have been in contact with circulating ground water. The beds are in a geologically stable region, and have shown no signs of earthquakes. The salt is 1000 feet below the surface, beyond the extent of all previous continental ice sheets. This all but precludes the possibility of disinterment of the wastes during their hazardous lifetime by natural processes operating from the surface.

One gets a sober feeling building something that will be of consequence to the society a million years from now — a feeling I suppose like Cheops must have had when he built the Great Pyramid almost 5000 years ago. The responsibility is indeed a heavy one. It is therefore reassuring that a National Academy of Sciences Committee on Radioactive Waste Management has just issued a report ("Disposal of Solid Radioactive Wastes in Bedded Salt Deposits," U.S. Government Printing Office, November 1970) in formal support of our plans for establishing this repository. It is also gratifying that the citizens of Lyons, Kansas, support the project.

Finally, there is the question of shipment of radioactive fuel. If, by the year 2000 we have 940,000 megawatts of nuclear power (as is predicted by the AEC) of which two-thirds are liquid metal fast breeder reactors, then there will be 7,000 to 12,000 annual shipments of fuel from reactors to chemical plants, with an average of 60 to 100 loaded casks in transit at all times. Projected shipments might contain 1.5 tons of core fuel with a decay time of 30 days, 300 kilowatts of thermal power, and radioactivity of 75 megacuries. By comparison, present casks from light-water reactors may produce 30 kilowatts and contain 7 megacuries.

Obviously, design of a completely reliable shipping cask for such a radioactive load is a formidable task. W. E. Unger, J. P. Nichols, A. R. Irvine, L. B. Shappert, and General Engineering have been working on this task for several years, and they have come up with casks that ought to do the job. As presently conceived, the heat would be transferred to air by liquid metal or molten salt; and the cask would be provided with rugged shields and seals that are resistant to deformation that might be caused by a train wreck.

The shipping problem looms as a difficult one, and it may be that we shall have to change our basic strategy. We may decide to cool fuel in place for 360 days before shipping; this reduces the heat load sixfold, and increases the cost of power by only about 0.2 mill per kilowatt-hour. Or a solution, which I personally prefer, is to cluster fast breeder reactors in nuclear power parks which have their own on-site reprocessing. We estimate that it would cost only 0.1 mill per kilowatt-hour more to reprocess in a nuclear park serving 10,000 electrical megawatts than to ship to a central plant serving 50,000 megawatts; this of course does not count the added cost of power transmission and pollution abatement. But the logic of this trend seems strong, and has led to establishment of Hanford as a nuclear power park.

Other Second-Order Effects — Thermal Pollution

Part of the confused argument over nuclear energy is focused on thermal effluents. Water-moderated reactors operate at about 33 percent thermal efficiency, modern coal plants at 42 percent. Thus a PWR releases about 1.45 times as much waste heat to the environment as does a fossil-fueled plant of the same size. This is certainly a disadvantage of the nuclear plant, but it by no means implies that conventional plants have no problem. It is therefore frustrating to the nuclear community to find the argument about thermal effluents so much directed at nuclear systems.

Three avenues are open to us in resolving the question of thermal emission from power plants, and ORNL is actively involved in all of these. First, we study the actual ecological and biological consequences of such emissions in order to assess the true size of the problem. Second, we invent benign ways of disposing of the heat (such as cooling towers), or of using it beneficially, to heat greenhouses or aquaculture ponds, for example. And third, we help develop reactor systems that operate at higher thermal efficiency. By way of example, I mention some work of C. C. Coutant and his group in the Ecological Sciences Division on interspecies competitions among fish as a function of temperature. If trout and bluegills are provided with chopped-up earthworms, then at temperatures below 22.5°C the trout will outeat the bluegills; at temperatures above 22.5°C, the bluegills will outeat the trout. I suppose heat effluents from a reactor would be called thermal pollution by trout fishermen, thermal enrichment by bluegill fishermen.

Avoiding the Large Accident

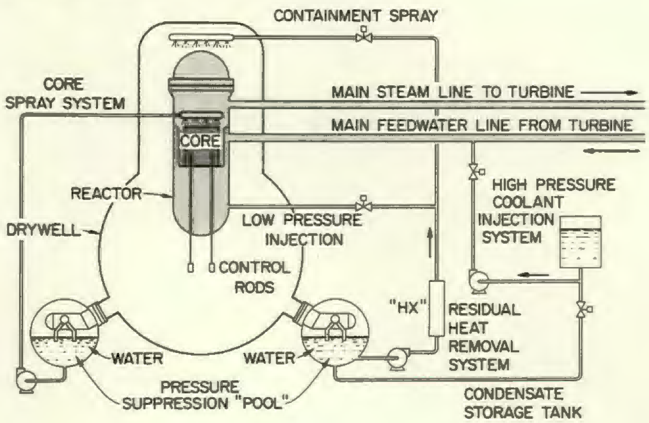
The other major point of attack by the opponents of nuclear energy has to do with the possibility of a catastrophic failure of a nuclear reactor and its containment, or of the inadvertent release of large amounts of radioactivity from spent fuel. One cannot say categorically that such incidents are impossible: nuclear reactors *are* radioactive, and a nuclear fire is intrinsically more dangerous than a coal fire. But obviously the probability of such occurrence has been made extremely small; one of the prime jobs of the nuclear community is to consider all events that could lead to accident, and to keep reducing their probability, however small it may be. On the other hand, there is some danger that, in mentioning the matter, one's remarks may be misinterpreted as implying that the event is likely to occur.

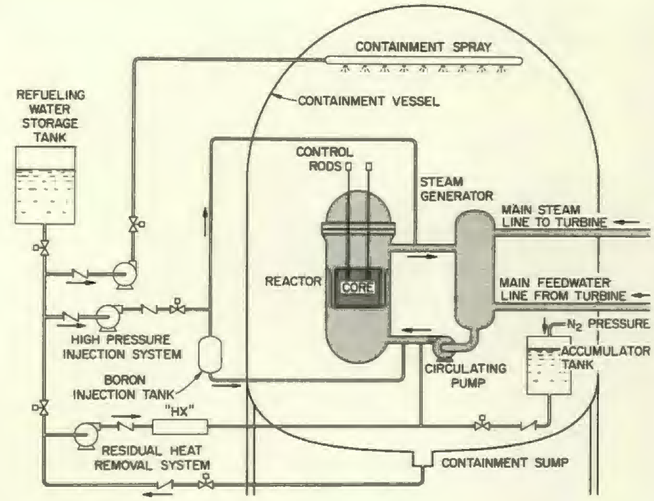
To better understand the problem, study the accompanying schematic drawings of a large boiling water reactor and a pressurized water reactor. One must remember that three barriers prevent radioactivity from being released: fuel element cladding, primary pressure system, and containment shell. In addition to the regular safety system, consisting primarily of the control and safety rods, there are detailed and elaborate provisions for keeping the reactor fuel cool in the event of a loss of coolant. In the BWR, there are sprays above the reactor core that spring into action within 30 seconds of an accident. In the PWR, water is injected from below under pressure from gas-pressurized accumulators. In both reactors there are additional systems for reducing the pressure of steam in the containment vessel; this system also washes down or otherwise helps remove any fission products that may become airborne.

These emergency core cooling systems are part of what are called engineered safety features in reactors. It is on the integrity and operability of these that one depends for avoiding meltdown in

PWR Emergency Cooling Systems

BWR Emergency Cooling Systems





the extremely unlikely event of a failure of the primary cooling system. In analyzing the ultimate safety of a light-water reactor, one tries to construct scenarios — improbable as they may be — of how a catastrophe might occur; and then one tries to provide reliable countermeasures for each step in the chain of failures that could lead to catastrophe. The chain conceivably could go like this: first, a pipe might break, or the safety system might fail to respond when called upon in an emergency. Second, the emergency core cooling system might fail. Third, the fuel might melt, might react also with the water, and conceivably might melt through the containment. Fourth, the containment might fail catastrophically, if not from the melt itself, then from missiles or overpressurization, and activity might then spread to the public. There may be other modes of catastrophic failure — for example, earthquakes or acts of violence — but the above is the more commonly identified sequence.

Now let me reiterate that listing all such failure modes by no means makes them credible: in fact, before any reactor can be licensed, every conceivable failure mode that could lead to serious public hazard must be judged highly improbable, or incredible. Therefore the nuclear community, and we at ORNL, have a continuing responsibility to examine every possible failure mode and to do whatever is required to reduce its probability of occurrence to as near zero as possible.

Let us begin with the first step in the chain — failure of the safety system to respond in an emergency, say, when the bubbles in a boiling water reactor collapse after a fairly routine turbine trip. Here the question is, as E. P. Epler has stressed, not that some safety rods will work and some will not, but rather that a common mode failure might render the entire safety system inoperable. Thus if all the electrical cables actuating the safety rods were damaged by fire, this would be a common mode failure. To make a common mode failure absolutely incredible is probably impossible. One goes a long way toward achieving this end if, as Epler has strongly advocated, each big reactor has two entirely independent safety systems that work on totally different principles. For example, at the graphite reactor we always had, in addition to the nine regular safety rods, tubes that could be filled with boron balls in an emergency. The Hanford N reactor recently did call upon samarium balls when its safety system failed; and, as expected, they worked perfectly. Epler's work is gratifying since he has, almost single-handedly, impressed on the entire nuclear industry the necessity for coping with common mode failure; his persistence has, I believe, diminished the chance that the first link in the chain of catastrophe — failure of all safety systems — will ever occur.

Another common mode failure, which would of course affect more than the safety system, is an earthquake. Nuclear reactors must be built so as to withstand earthquake accelerations of 0.1 to 0.5 g; moreover, no nuclear reactor is allowed to be built on an active fault.

We at ORNL have had a program to assess the danger from earthquakes for half a dozen years, first under R. N. Lyon and now under G. D. Whitman. This year a team of soil engineers from UCLA in collaboration with Julius Foster of

ORNL have simulated earthquakes at the EGCR by the use of large vibrators and by exploding as much as a ton of dynamite a few hundred feet from the structure. These experiments are extremely important since they show that one can partly simulate earthquakes in an actual completed plant, and judge experimentally, rather than by speculation, just how serious an earthquake in the vicinity of a reactor might be.

The next step in the chain is the failure of the emergency core cooling system and the engineered safeguards. What can we do; what have we done to understand this step and guide design to make failure of the engineered safeguards incredible?

Here a group under P. L. Rittenhouse is examining the effect on coolant flow of the catastrophic failure of a bundle of fuel rods. The point is to decide whether, if cooling were lost, the rods would block the water passages and thus prevent the emergency core cooling system from flooding the reactor. Work of this sort helps set limits to the power density at which fuels should operate.

The third step is the melting of the fuel element. For almost 10 years G. W. Parker and his associates have been studying the release of fission products from molten fuel. By now the work is far more sophisticated than when one simply melted small pieces of UO_2. To mention a single experiment at TREAT, ORNL has shown that, for Browns Ferry type fuel, most of the iodine-131 (the most hazardous volatile product) was in a reactive form (I_2 and HI). Moreover, as experiments by T. H. Row and L. F. Parsly at the Nuclear Safety Pilot Plant show, the containment spray (which is actuated at the same time as the emergency core cooling system) will remove these species very effectively.

As for the fourth step, work is under way by Joel Witt and his colleagues on integrity of the pressure vessel and by W. L. Greenstreet and his associates on developing methods of analysis for pressure piping and components; W. K. Ergen has studied the integrity of the containment system. Of all the components of a PWR, the pressure vessel is probably the most important. If the pressure vessel, as well as the rest of the primary system, maintains its integrity at all times, no incident can present a serious hazard to the public. To reassure ourselves as to the incredibility of a catastrophic pressure vessel failure, Witt's colleagues are devising ways of judging the integrity and safety of very large steel sections by extrapolation from experiments performed on small specimens. The experiments are difficult and expensive; yet the conclusion one must draw is that nuclear pressure vessels are designed very conservatively, and their failure is indeed all but incredible.

As you see, the work on nuclear safety is complex and is spread among many engineering and applied scientific divisions. We have integrated much of this work under the general direction of W. B. Cottrell; he is director of the Nuclear Safety Program and of the Nuclear Safety Information Center. This center, now in its eighth year, employs 30 specialists from the Laboratory who spend between 10 and 50 percent of their time reviewing the literature, preparing state-of-the-art reports, publishing *Nuclear Safety*, and generally providing a central focus for research in nuclear safety throughout the U.S. and abroad.

Another integrative activity is the preparation of Reactor Development and Technology nuclear standards. "Standards" in engineers' parlance means the codification of rules of sound practice that engineers must observe in carrying on their business: design, quality assurance, materials, testing — in short, all the activities that an engineer engages in that conceivably might affect the safety and reliability of a plant. To the present, under Myer Bender's general direction, 97 tentative standards have been prepared and issued at ORNL by the General Engineering and Construction and Instrumentation and Controls Divisions; another 107 standards have been submitted for RDT approval. Though this work is detailed and somewhat tedious, it is extremely important in establishing standards of excellence and prudence for the entire nuclear industry.

If one stands back and tries to assess whether an uncontained nuclear accident is absolutely impossible, one would have to say, no, it is not *absolutely* impossible; there is some extremely small possibility of an accident. Critics of nuclear power often point to the 1957 AEC study (WASH-740, "Theoretical Possibilities and Consequences of Major Accidents in Large Nuclear Plants," March 1957) which predicts that 3400 people might be killed and $7 billion worth of property might be damaged if a 200-electrical-megawatt reactor dispersed its fission products. But the critics do not mention that WASH-740 gave no credible scenario leading to the accident it postulates. Moreover, absolutely no credit is taken for realistic mechanisms, some of which I have described, that would greatly reduce the driving force of the accident and reduce the airborne concentration of fission products. Thus in trying to weigh this risk one must first point out that the probability of an uncontained accident is extremely small; and second that, because there are many factors that would mitigate the situation, even an uncontained accident would be considerably less serious than suggested in WASH-740.

Long-Range Approaches — Fission and Fusion

In the longest run, we probably will shift to nuclear energy sources that have even less environmental effect, particularly less thermal effect, than do the current generation of water-moderated reactors. The newer sources include the high-temperature gas-cooled, liquid metal fast breeder, gas-cooled fast breeder, molten-salt breeder, and thermonuclear fusion reactors. Let me describe briefly some developments at ORNL, particularly those that relate to safety, in each of these systems during the past year.

Each one of the new reactor types possesses certain inherent elements of safety, as well as characteristics that one must engineer around if the reactor is to be adequately safe. Thus the liquid metal fast breeder, which is the main line of advanced reactor development, operates at low pressure with a coolant that transfers heat very efficiently, has high boiling point and high heat capacity, all of which impart inherent safety to the system. On the other hand, the power density of the LMFBR is very high, sodium is inflammable, and the reactivity increases if a large void or vapor bubble forms in the reactor. The gas-cooled fast breeder suffers from the high pressure and low heat capacity of its coolant; on the other hand, it is immune to the formation of a void, and its pressure vessel is made of prestressed concrete so that a catastrophic depressurization of the main vessel is incredible. The MSBR operates at low pressure with a fuel that is molten to begin with and which operates at much lower power density than does the fuel in solid-fueled reactors; on the debit side, fission products are deposited throughout the fuel circulating system, and therefore all surfaces of the primary system must be cooled in the event of an accident that causes the fuel to drain.

Since all of these systems are still at a relatively early stage of development, it is possible to incorporate into the fundamental design mechanisms and precepts that, ab initio, serve to avoid or mitigate the consequences of an accident.

To take an example from the LMFBR, a reactivity excursion in such a reactor is limited by the Doppler effect — that is, the temperature broadening of the capture resonance lines in fissile and fertile material. To estimate this effect we must know the resonance absorption spectrum of uranium-238 and plutonium-239, and to this end ORELA provides the most accurate data produced anywhere in the world. And, once one understands the Doppler effect thoroughly, one can incorporate into the design features which maximize the effect and thus reduce the size of a prompt excursion.

Or consider the possibility of blocking a few fuel passages and causing a few pins to fail: Is it conceivable for such failures to propagate in an LMFBR from one fuel element to the next (possibly leading to a large-scale meltdown)? This matter is being investigated by M. H. Fontana and R. E. MacPherson in the fuel failure mockup, a sodium loop in which fuel pins are simulated by electrically heated rods. The point is that data of this sort is used to design the core so as to minimize the necessity for second-line engineered safety features.

Our main contribution to understanding the safety of gas-cooled fast breeders has been an analysis of what happens if one loses pressure on the main gas circulating system. How much time would one have to start emergency cooling, and what would happen if melting did occur? J. P. Sanders, R. S. Holcomb, and O. W. Burke find encouraging answers to both these questions: one has half a minute of grace before one has to start the emergency cooling, and, even if meltdown occurs, it appears to be quite feasible to keep the molten fuel from breaching the containment.

In the HTGR work, J. L. Scott and his colleagues have developed a carbonized resin fuel particle that can sustain extremely high temperature excursions, 1800°C, and therefore add safety to a system already notable for its inherent safety.

In the molten salt system, I mention two matters that have implications for safety: continuous removal of rare earths, and tritium. The metal transfer process, in which the fuel salt is contacted with molten bismuth, and the bismuth is then contacted with molten lithium chloride, has been demonstrated on laboratory scale. Rare-earth fission products do indeed move from the fuel into the acceptor salt, whereas thorium remains behind. This demonstrates that on-line removal of poisonous fission products is probably feasible, and therefore the fuel in an MSR would be intrinsically less radioactive than is the fuel of a solid reactor. But, more important, on-site reprocessing implies that molten-salt reactors would not be burdened with the awkward shipping of radioactive fuel elements.

Sequestering of tritium in an MSR is proving to be very difficult. Tritium tends to pass through hot metals: from fuel through intermediate salt to steam. One clear-cut, but awkward, solution to the tritium problem has been found during the year: introduce still another intermediate salt, such as sodium nitrate—sodium nitrite, which positively will tie up the tritium as T_2O. This scheme adds to the complexity of the system, but not to an impractical degree. It is reassuring to know that there is at least one solution to the mean problem of tritium, and we hope other less awkward ones will be found.

Some point to fusion power as the clean energy source. Fusion still rides on the wave of optimism that has surged in the last two years, and certainly the Oak Ridge program has taken on a new excitement with major experiments like ORMAK and IMP about to be turned on, and with the new microwave-heated bumpy torus in the offing. This very optimism has kept Oak Ridge in the forefront in the matter of engineering the fusion reactors, and here it is that the safety and environmental aspects have been given further scrutiny.

The greatest hazard in a fusion reactor lies in its huge tritium inventory — typically a hundred million curies. Tritium has a half-life of 12 years, and, to prevent atmospheric buildup, the leakage rates have to be kept below about one-millionth of the inventory per day. Can this be done? I think it can; although there is the same problem of diffusion through hot metal as we have in MSR, nevertheless this is the kind of problem that Oak Ridge has the strength to resolve. What then about catastrophic release? A paper by Herman Postma gives comparative figures as between tritium release from a fusion reactor and iodine-131 release from a fission reactor of equal electrical output. Not only are there four times as many curies of iodine as of tritium; but, because tritium is so much less noxious to biological systems, we end up with fusion presenting at least ten thousand times less potential hazard than fission.

The third consideration with regard to comparative safety deals with residual radioactivity and afterheat in the structures of the two kinds of reactors. It was a surprise to learn from Don Steiner that a niobium reactor vessel of a fusion plant would have almost as much induced activity, and therefore almost as much afterheat, as a fast breeder reactor, but further reflection shows that there are compensating factors. First of all, niobium was a blind choice as far as radioactivity is concerned; if one wants to sacrifice a little on the tritium breeding, the radioactivity can be reduced. Secondly, the heating is spread over a large area, so that cooling is much easier. Finally, the fusion fire has little net energy content, and it will always fail safe. There is no possibility of the reassembly of a critical mass.

Overall, one must continue in the opinion that fusion poses fewer potential challenges to the environment than does fission, but nevertheless it must be carefully watched.

The Benefits of Nuclear Power

I have devoted all of my remarks thus far to an assessment of the risks of nuclear power and what we at ORNL are doing to understand and to reduce these risks. What about the benefits, particularly the ones that concern us at Oak Ridge?

In weighing benefits against risks in the large-scale use of power reactors, we must place in the balance those benefits that directly flow from the use of nuclear power rather than the indirect ones, such as isotopes and basic research. The direct benefits are both long range and short range. Even some of the aggressive critics grant that nuclear power in the long run will be a great boon, that mankind must eventually have an alternative to fossil fuel simply to survive. But they ask, Why should we proceed with nuclear power at our present pace?

The simplest answer is that we need more energy now from every source; and nuclear power is cheaper and is less damaging to the environment than is fossil-fueled power. Only with respect to heat rejected from water reactors does nuclear power do worse than fossil. Chemical effluents from nuclear plants are essentially nil. Even as regards radioactive effluents, a fossil-fueled plant emits more biologically damaging radioactivity than a pressurized water reactor. Of course the effluents from presently designed radiochemical plants more than make up for this; but, as I have said, near-zero-release chemical plants are feasible.

There is another compelling argument for nuclear power in addition to its potentially negligible impact on the environment and its availability at a time of extreme shortage of energy. This is the role that nuclear energy might play in helping to resolve two of the world's most urgent problems — hunger in India and refugees in the Middle East. In dealing with both of these matters, ORNL has been heavily involved.

As for the Middle East, we have completed our studies implementing the Baker Resolution and have forwarded them to the State Department. Is it too fantastic to believe that nuclear power and desalting may present new options to the warring parties and therefore help the cause of peace in the Middle East?

The Indo-Gangetic Plain Project has been submitted by the Bhabha Atomic Research Centre to the Indian Government for approval. The plan, as developed by our Indian colleagues in Trombay, calls for 25,000 tube wells energized by two nuclear reactors. The entire project could be completed by around 1980. When completed, it would produce an additional eight million tons of grain per year.

Technological Assessments — Benefits Versus Risks

The controversy over nuclear power starkly illustrates the difficulties of weighing benefits against risks in modern technology. No matter what technology one considers — television, the supersonic transport, the automobile, the computer — one is always beset by the question of how to assess risks and how to balance the risks against the benefits. Recognition of this dilemma led to the reports by the National Academies of Science and of Engineering on technology assessment — that is, the process of systematically examining the physical, biological, even social, side effects of new technologies (National Academy of Sciences, "Technology: Processes of Assessment and Choice"; National Academy of Engineering, "A Study of Technology Assessment and Choice," U.S. Government Printing Office, July 1969).

In most cases these side effects have strong environmental impacts. As D. J. Rose has put it, technology assessment is the future tense of environmental abatement. Thus every one of the concerns about nuclear power — low-level radiation effluents, thermal effluents, the remote possibility of accident — impinges on the environment.

It was therefore quite natural for the Laboratory this year, in responding to the Commission's directive to expand our work on the environment, to think broadly of technological assessment and the environment. This is the title of the new research program directed by J. H. Gibbons and sponsored by the National Science Foundation.

The NSF project was kicked off in June by a summer study headed by D. J. Rose. The work was divided among six groups — Energy, Materials Resources and Recycling, Environmental Indices, Regional Modeling, Information, and Communications. Since we at ORNL knew little about several of these fields that have large social components, we invited a number of consultants to work with the group for about three months.

The summer study proved that it was possible to launch an interdisciplinary attack on problems of the environment here at ORNL. Perhaps most gratifying was the interest shown in the project by many basic scientists at the Laboratory who had always had a personal interest in the environment but had never had an opportunity to put their interests to professional use. For example, Robin A. Wallace, of the Biology Division, has made quite a reputation for himself and for ORNL by writing what has become one of the best reports on mercury contamination; it certainly has the best name — "All You May Ever Want to Know About Mercury."

We are now formulating an environmental program for the coming year for consideration by the National Science Foundation. We have reason to expect that NSF will continue to respond positively to this Oak Ridge Work, and that the project will go forward.

Technological Fixes, Social Issues

Let me return to the dilemma nuclear energy faces, and particularly the dilemma an institution such as ours faces. As our studies with NSF have brought out, environmental concern, including concern for nuclear reactors, is not simply a question of technology. Public attitudes, public fears, public understanding are involved in the ultimate acceptance of nuclear energy, just as they are involved in acceptance of other new technologies. Nor is it any longer a foregone conclusion that every new technology will be accepted by the public. The recent Senate action on the supersonic transport is an example of this.

What, then, can a scientific and engineering institution like ORNL do to ensure the viability and validity, and the public acceptance, of our enterprise? First we can do, and we must do, that which we do best: use our scientific expertise to assess, both experimentally and theoretically, precisely what the risks are. We must not try to underestimate them; but neither must we exaggerate them. Second, we must devise the technologies and the technological fixes that diminish even further the already extremely small risks; and we must persevere where necessary to get these better systems adopted in practice.

We may have to venture into fields far from our traditional ones, to examine what the mechanisms of public misunderstanding are, and what can be done to reduce this misunderstanding. And we may have to venture into a new round of public education, not unlike what many of us undertook immediately after the Hiroshima bomb was dropped.

I would propose as one step that all of us acquire some familiarity with the technical issues. To this end we are establishing a series of seminars on nuclear energy and the environment to which all staff members are invited; the seminar speakers will present the best information we have on these questions. Beyond this, I would hope that we can help spread information to the scientific community at large — notably the scientific departments at the universities, since in many cases it is to the university physics, chemistry, engineering, and biology departments that the public turns for expert opinions on nuclear risks.

And finally, I would hope that each of us will rethink the terms of the risk-benefit balance that is being struck by nuclear energy: the possibility of forestalling Malthusian catastrophe, but with a means that poses an extremely small, but nonzero, risk. To those of us who have lived and worked at ORNL almost all our lives, and who see the risk in its proper proportion, this seems like an absurd imbalance in favor of the larger benefit. And indeed, herein lies the crux of the matter. For the risk that I believe must be balanced against the benefit of abundant nuclear energy is not the far-fetched and misplaced concern over low-level radiation or even the extremely unlikely catastrophic nuclear accident. Rather it is the catastrophe that will surely beset the many billions who come after us unless they have an adequate source of energy. Insofar as this generation has a responsibility to future generations, it has the responsibility to develop this new technology: to explore its possibilities, to ferret out its shortcomings, to correct them, and thus to present the future with the means for its survival. This we are now doing at Oak Ridge, and this is what we, as fully responsible scientists and human beings, must continue to do until the full potential of nuclear energy is achieved.

Ellison Taylor, director of the Chemistry Division, has been an Oak Ridge chemist since 1945, before which he taught at Utah and Cornell. An experimental scientist who has the reputation for tolerating nothing less than the most rigorous of research techniques, his sensibilities were affronted by what he was sure was a lack of skepticism and sufficient investigation in the claims, in 1963, of the discovery of a new form of water. This anomalous water, dubbed "polywater" because it was believed to be a polymer, resembled a gel more than a liquid, said its proponents, and moreover appeared to be a more stable form of H_2O. It was even postulated, however tentatively, that the new material, released into the environment, could conceivably turn all the seas and rivers to Jello. Convinced that other factors were at work in this discovery than had been accounted for, Taylor took up the ensign for classical chemistry, worked off and on for two years on the subject with ORNL consultant Max Bredig, and favors us with the following account.

The Great POLYWATER Doodle

or Two Years with the Wrong Water

By ELLISON TAYLOR

TODAY, I SAID GOODBYE to anomalous water. I threw away the capillaries for making it, rinsed out the density gradient tube with the tiny glass calibrating floats still hovering in it, and returned the borrowed microscope. After salvaging some clamps and ringstands and other reusable items, I filled one wastebasket with assorted junk, all once part of some setup for making anomalous water or measuring one of its anomalous properties. I could have filled another with recorder charts and Xerox copies of papers. Two years, two waste baskets. And two seminars. And one paper. So, I'm a little sad, and not just about the one paper. Each of the experiments was fun, because I did it all myself. But now there isn't any point in doing any more, and I'd better move on to something more important. There are people who ought to feel worse than I do, since they're dead wrong, but it isn't clear whether they know it or not. Max and I knew they were wrong from the beginning, and I suppose lots of people who never got involved knew it too, but none of the chief protagonists has given any sign of admitting it. Nor has the popular scientific press announced any decision. So, perhaps I'd better tell you why I'm positive that anomalous water doesn't exist.

About ten years ago, a Russian named Fedyakin reported that he had made a new form of liquid water. When he exposed fine capillary tubes (a few microns in diameter) to water vapor, some of them after a day or so accumulated short columns of a liquid that froze differently from ordinary water, and that expanded differently on heating. On the strength of that, he moved from an obscure technical institute to Moscow, to the laboratory of B. V. Deryagin, an internationally known surface chemist who was famous for having done a number of very difficult experiments, including a direct measurement of the van der Waals force of attraction between two macroscopic bodies. Deryagin immediately became the chief proponent of the existence of a new form of water and over the next few years he and Fedyakin and members of his laboratory reported further anomalies in the new material. The most striking of these were a density 1.4 times that of ordinary water and a viscosity 15 times as great.

Now it is easy to explain different *solid* forms of a particular substance, simply by different spatial arrangements of the constituent particles. There are, for example, six (perhaps seven) forms of ice (solid water), each with a characteristic arrangement of the individual H_2O units, and each form is stable in a particular range of temperature and pressure. This phenomenon of polymorphism is common in solids, but there are only a few examples of liquids that show two sets of properties. In liquids, the individual molecular units move about (that's why a liquid is liquid), and if there is to be more than one kind of a particular composition, then the individual units must be chemically different, not just arranged differently in space.

Deryagin recognized that a second form of liquid water would require some new structural entity bound together strongly enough to behave as a molecular unit. He suggested no particular formula, only that the water in the new form was polymerized (several H_2O units bound together in some larger unit). Because this new kind of water was formed spontaneously from ordinary water, it would have to be more stable than ordinary water, and Deryagin found, indeed, that the new liquid, once formed, was not changed into ordinary water until it was heated above 500°C.

That, with some minor further information, was the situation in 1966. All of the experiments had been reported in Russian journals, and they had failed to attract much attention elsewhere. In 1966, however, Deryagin summarized these experiments at a meeting of the Faraday Society, so that they did come to the notice of British and other Western scientists. Most of the notice appears to have been unfavorable, but two or three groups in England decided to try to reproduce the experiments. The real stimulus to awareness and interest in the West appears, however, to have been a series of items that were published in *European Scientific Notes*, a private but widely circulated publication of the London Office of the U.S. Office of Naval Research, followed by a summary in a Technical Report from the same office. These items described most of Deryagin's experiments and indicated the existence of programs in British laboratories, one at least partially in collaboration with Deryagin.

Although some of the properties attributed to the new form of water were markedly different from those of ordinary water, two features of the experiments pointed to an obvious, trivial explanation for the results. These were the random occurrence of condensation in only a few of any group of capillaries, and the fact that admitting liquid water to a capillary and then removing it prevented subsequent formation of anomalous water. These pointed so strongly to the presence of a soluble impurity that it seemed impossible to entertain any other explanation. Water vapor will condense on and dissolve any soluble material provided the partial pressure of the vapor exceeds that of the solution that is formed. A likely source of soluble material is dust, which almost always contains tiny particles of sea salt. This source of nuclei for condensation would explain the rarity of condensation and its random nature, and also the prevention of condensation by prior admission and removal of liquid water, which would rinse out a particle that could otherwise result in condensation. Furthermore, a solution would have properties different from those of pure water, so that in principle all of the results might be explainable on this basis.

A strong argument against the possibility of anomalous water could also be based on its nonoccurrence in nature. If it's all that stable, why isn't it there? However, a negative kind of proof is not conclusive by itself. You may never have seen a unicorn, but does that prove it can't exist?

That was the situation in the late summer of 1968. In spite of the compelling internal evidence that anomalous water was simply impure water, some people were taking the new material seriously, and it seemed time for somebody to take up for common sense. So, I decided to repeat some of Fedyakin's and Deryagin's experiments with a view to explaining them on the prosaic grounds of impurities.

I drew some fine capillaries, filled one end with distilled water without getting any in the other end, and then sealed off both ends (Fedyakin's second method). Then I put them away in a box and went away for a week. Upon examining the capillaries on my return, lo, there in one was a second column of liquid! Over the next few days, I followed the growth of the new column and the shrinkage of the old, just as Fedyakin had done. I hadn't doubted his observations (just his explanation), but it was satisfying to be able to repeat them so quickly.

What to do now? The amount I had collected was too small for me to measure the thermal expansion, which seemed the easiest measurement to make. I had hoped to measure electrical conductivity to demonstrate the presence of dissolved salts, but it proved too difficult to remove the sample from the capillary, even though I thought I had devised a suitable ultra-micro cell for making the conductivity measurement. What I did do was two things. One was to make larger quantities, by shifting to Fedyakin's first method (exposing open capillaries to water vapor from a pool of water in a closed vessel) and using larger capillaries; and the other was to study the location of the column in a particular tube. If the impurity hypothesis was correct, the short lengths of capillary occupied by the occasional columns were the original sites of bits of soluble dust, and the columns contained those bits in solution. If one evaporated a column by lowering the pressure (on both sides of it so as not to move it), then the dissolved material should reprecipitate on the wall at about the same place. And, if one reexposed it to water vapor, condensation should ensue at the same site. Further, if one moved the column to a new position along the capillary before evaporating off the water, the recondensation should be at the new position. I tried both of these experiments, and was gratified to see things happen just as I have described. The explanation of Deryagin for the formation of columns was that special sites of the capillary wall were catalytically active for the conversion of ordinary to anomalous water. The first experiment did not disprove that, since the second condensation occurred at the same place and could have involved a fixed catalytic site just as well as a bit of soluble dust. But the second experiment eliminated that possibility, because some special catalytic arrangement of the atoms of the glass would not move with the liquid to a new site.

I was pretty happy with my results, but an unexpected blow arrived in the June 27, 1969 issue of *Science*. There had been rumors of attempts to observe spectra of anomalous water and an article in that issue by E. R. Lippincott and G. L. Cessac of the University of Maryland, and R. R. Stromberg and W. H. Grant of the National Bureau of Standards reported just that. They showed both infrared and Raman spectra of material they had made in the standard way for making anomalous water. These spectra, although obtained with small amounts, showed impressive detail and the infrared spectrum was stated by Lippincott et al. to be "not a spectrum of any known substance." They pointed to the complete absence of the absorption bands observed with ordinary liquid water, and the presence of new ones. In the Raman spectrum, they again noted the absence of the typical water band and the presence of a strong new one.

Raman and infrared spectra arise partly from the interaction of the measuring light with vibrations of individual pairs of atoms and thus provide pretty direct information about the bonding between atoms. The infrared absorption band attributed to vibrations within the O—H group in alcohols, for instance, can be identified in the spectrum of any alcohol, the frequency differing only slightly from one alcohol to another. A similar band arises from the OH group in water, but with a detectably different frequency from that in alcohols (and different in the vapor from in the liquid). Lippincott et al. proposed a structure for the new material on the basis of the spectra — one in which separate water molecules were no longer distinguishable, but with all the O—H distances equal, whether within an original H_2O or between two originally separate water molecules. That is, they proposed a polymer in which all the bonds were short and strong (not quite so short as in the separate water molecule, but shorter than those between separate molecules in ordinary liquid water). The strong bonding could explain the high stability reported by Deryagin, the shorter bonds could explain the greater density, and the polymerization to larger molecules could explain the high viscosity. They even had the foresight to propose a new name, "polywater."

This was a rude shock. Neither the results nor the specific model answered the basic objections to a new form of water, but the publication of a new, "unique" spectrum produced and explained by a well-known spectroscopist opened the question of whether I could be wrong. Fortunately, Max Bredig, who had up to this point given only moral support, decided to enter the game. He was only slightly less rabidly against anomalous water than Kurt Kraus (who had made the statement that if this new water existed he intended to abandon chemistry), and he took this "proof" of its existence as a challenge to chemistry itself.

After a few days of study, he came to me to discuss the paper. He pointed out that, in spite of the positive tone, it was very poorly written, in that the procedures were only vaguely described and the obvious blank experiments had apparently not been done. The ominous spectrum was still there in black and white, but we began to suspect that some unnoticed error might disqualify it. Max therefore set out to look at infrared spectra of water and various solutions, with the hope of finding some solution that would match the published spectrum. The problem of the new bands looked surmountable, since new bands could come from unsuspected solutes, but the "complete absence" of the normal water bands was discouraging.

In the meantime, the paper which so distressed us touched off a storm of enthusiasm. Numerous groups joined the search for polywater, some with support from government agencies (not the AEC). Articles appeared in the two *Timeses* (Chattanooga and New York), in *Time*, in *Popular Science*, and in *The Saturday Review*. Said *The Wall Street Journal*, "Good news: the U.S. has apparently closed the polywater gap, and the Pentagon is bankrolling efforts to push this country's polywater technology ahead of the Soviet Union's."

While all this was going on, I was trying to make larger amounts of material so we could get Raman and infrared spectra of liquid prepared in the correct manner. Strange reports had begun to come in (by word of mouth) that made this seem important. Some of the proponents indicated that it was easy to get condensation of salt water (I knew that), but that only once in a while was one of the columns really composed of polywater. It was even reported that some of the masters could tell just by looking which were real and which were spurious.

Further, it was said that the genuine material was yellowish and had the consistency of vaseline.

Information of this sort set me off on a study of cleaning methods. One source of soluble material in a capillary that we had recognized as possible in addition to dust was a residue of some cleaning agent. Deryagin, in a few papers, said he had cleaned the tubing from which his capillaries were drawn, and mentioned various agents including hydrofluoric acid, a substance which slowly dissolves glass. Lippincott was less specific, but mentioned "cleaned in the usual way," which suggested several possibilities, residues of which, trapped perhaps in some microfissure, could give rise to condensation and perhaps even to a yellow, viscous liquid. Furthermore, some of these materials might well produce the unusual spectra reported by Lippincott. However, try as I might with hydrofluoric acid, chromic acid, alkaline detergents, or what not, I never got any marked increase in yield of columns, and never got anything remotely resembling vaseline. I began to feel I must lack some important skill, or else that the climate of Tennessee was less favorable than that of College Park (or Columbus, since a new report from there was said to confirm Lippincott's original spectra). Our director (AMW), visiting in Princeton, even picked up a snide remark that the trouble was we were too uptight about it. There they were entrusting the manufacture to seniors, who presumably were not uptight.

Max, in the meanwhile, was progressing nicely. Using such common salts as sodium or potassium sulfate, carbonate, or bicarbonate, he could get a reasonable match to Lippincott's original infrared spectrum except that he couldn't quite get rid of one of the normal water bands. He had earlier theorized that an instrumental defect plus an operational error (of letting the infrared beam overheat the sample and convert it to vapor) could have given the anomalous spectra. We finally agreed that this required too large a coincidence, and Max based his subsequent efforts on finding a suitable impurity.

All of these activities brought us to the fall of 1969. More and more articles were appearing with new evidence (usually not very convincing) for polywater, and Max and I agreed it was time to try to work up what we had in an attempt to counter the deluge of misinformation and misinterpretation. We believed that we had a partial (not quite satisfactory) answer to the spectrum; I had demonstrated that "my" anomalous water behaved as it should if it were a condensate on an impurity, and I thought I could explain most of Deryagin's experiments. Max and I therefore undertook to write a rather long article of which my part would be a critique of Deryagin's work with some experimental items of my own, and Max's would be the duplication of the spectra by impurities.

A more careful reading of Deryagin's papers showed that essentially all of his results could be explained by the impurity hypothesis. The properties he measured were either those of a salt solution or were obtained by an obvious misinterpretation. The thermal expansion, which he frequently relied on as a measure of anomalousness, was easily explained. He usually found the temperature of maximum density (the minimum in the thermal expansion curve) shifted downward by 5—15°C from the value for water, 4°C. Solutions of various salts behave in exactly this way, an 8% solution of NaCl, for instance, lowering it by 21°C. Similarly, the freezing behavior, said to be so strange, is just that expected from a salt solution. If you freeze a salt solution and then melt it, there is a period when you can see little crystals of ice still floating in the already-melted liquid. Deryagin mistook these for liquid drops, and thought he had two coexisting liquid phases, indeed an anomaly.

He later discovered the drops were solid, but did not realize (or admit) that that changed the whole picture.

The viscosity, said to be 15 times that of water, was a clear misinterpretation. He measured it by timing the flow of a column through a capillary under pressure, calculating the viscosity from the length, time, and capillary diameter. For comparison, he ran ordinary water in the same capillary. What he found was that the first pass with either anomalous water or water took an unusually long time (indicating unusually high viscosity), but that with successive passes both came to a final value corresponding about to the viscosity of ordinary water. He stated that the ordinary water reached this state sooner and that the other liquid was therefore anomalous. This difference was documented for only two runs, and it is obvious that he was simply observing the usual surface effects that arise in capillary motion before the surface is uniformly wetted (by repeated passage of the liquid).

Deryagin's other experiments could be similarly disposed of, and I proceeded to work all of this into an article which would precede one by Bredig describing the spectroscopic results. He was still trying to improve the resemblance between the synthetic spectra and the originals, and we felt a closer match was worth waiting for.

About the first of January we sent the paper to *Science* and I began preparations for a seminar I was to give at Brookhaven. On the day before, I came down with flu, postponed the seminar, and spent the next few days in bed. Here, I had time to think about anomalous water, and tried to find some way to make one of our vague objections into a reasonable sort of proof. I have already mentioned the objection that it hadn't been found in nature. That was pretty convincing already, and I couldn't see any way to make it conclusive. It seemed unlikely that there would be two stable and very different bond distances between the same pair of atoms (short of some major change like introducing a double bond), but that was really a problem for quantum mechanical calculation, not casual thinking. But I kept coming back to the puzzling fact that no one could make more than minute amounts of the material, and finally I saw that this fact and the standard explanation for it were in contradiction with other observations, giving a logical and persuasive argument against the existence of polywater. My new argument went like this: Anomalous water is formed spontaneously from ordinary water. Since water doesn't convert to anomalous water except under special conditions (presence of a glass or silica surface), there must be a barrier to this conversion (an activation energy) which is only surmountable in the presence of a catalyst (the glass surface, or perhaps a few special sites on the glass surface). Since only a small amount of the new liquid is produced, something must poison the catalyst, and the only reasonable poison is the polymer itself. But if the polymer is the poison, it should be effective when it covers the catalyst with only one layer, not after accumulating a capillary full. The remainder of the capillary full must then be ordinary water condensed on and dissolving the polywater. This would limit the possible concentration of polywater to a few tenths of 1% (a simple geometric calculation). But various people claim to have produced polywater all the way from a few percent to essentially 100%. Thus a contradiction is reached which can most easily be resolved by the hypothesis of condensation on soluble impurities.

Well, I worked this into a short note, and sent it off to *Science* also. Max continued to try to improve his spectra, I turned my attention to the budget and other matters and waited to hear from our papers.

When we heard, we were more surprised than pleased. An Assistant Editor said (after keeping the papers two months) that *Science* was about to publish three papers attacking polywater and containing "experimental work" and therefore didn't need ours. After some hours of intermittent discussion (which I will leave to the imagination of the reader), we decided our only course was to wait for the promised papers to appear. Which they did, beginning with the March 27, 1970 issue.

The principal one, from Bell Labs and USC, reported the preparation of condensate according to the usual method and gave infrared spectra of combined samples that matched rather closely the spectra of Lippincott. But then these workers (D. L. Rousseau and S. P. S. Porto) had the material analyzed by a number of micro methods, and found that, in contrast to Lippincott's reports, they were loaded with impurities, in some cases up to ⅓ of their weight. The work was obviously carefully done and superbly reported; at last there was a report that could be properly judged because it described what was done.

It was clear from this that we had gone wrong on three counts. First, we had put too much stress on precise duplication of Lippincott's spectra. On close examination, we could see that there were small variations between these spectra and those of Lippincott, just as there were between Max's and Lippincott's and even as there were between the original Lippincott and the Battelle-Lippincott spectra. Everyone but us, both pro and con, was apparently more easily satisfied with just resemblance.

Second, I should have been more imaginative in the preparation step. I had noted solid or semisolid residues sometimes when I evaporated columns under the microscope; these people evaporated their product to just such a state and then handled it like butter. It didn't occur to me to consider this the material of interest.

Finally, we shouldn't have been so cost-conscious. We could have had analyses made, too, but we were just too cheap. The authors of the Bell and USC paper thanked 4 people for analyses. We didn't thank anybody and we didn't get published.

The other important paper (July 3, 1970) was by S. W. Rabideau and A. E. Florin of Los Alamos, and presented a similar story. The analyses were done differently and showed somewhat different impurities, but the general story was the same. An interesting sidelight was that *Science* had rejected about the same material a year earlier when it was publishing the early pro-polywater papers. Sour grapes? No, a balanced criticism of editorial policy.

From our point of view, these articles should have finished the great polywater illusion. In fact, we heard rumors that Lippincott was giving up and was expected to recant at the long-planned symposium on anomalous water at the National Colloid Symposium at Bethlehem (Pa.) in June. All of the big names had been scheduled for this meeting, and Max and I decided to go. At least we could be in on the final chapter. Since the announcement of the meeting called for late, short papers, I submitted an abstract based on my sick-bed invention.

Came the meeting. Deryagin gave a plenary lecture in which he summarized all (I think) of his work. He took note of some difficulties with impurities in other people's work, but claimed his results were not affected by this. Lippincott gave a paper in which he said impurities were a problem, but then he repeated essentially all of his earlier story. Rousseau gave the Bell-USC results and Rabideau those of Los Alamos. Allen, a quantum mechanician from Princeton, gave a long paper in which he explained theoretically all of the observed properties of polywater on the assumption that it was a polymer of water, and then concluded that it probably didn't exist. I attacked his treatment of the kinetics of formation and he avoided my criticism by not believing in polywater any longer. Two holders of contracts on polywater gave unimpressive papers. The organizer of the symposium said he believed the existence of polywater was about a 9 to 1 bet in its favor, but got away before I could get any of his money. Two Princeton seniors gave a tandem talk in which one introduced the other. Their x-ray study of polywater was extraordinarily poor. There were about ten short talks, including mine. One was an off-the-cuff calculation by a Britisher of the magnitude of the surface forces which might disturb the viscosity measurements (the point I had made before, but it sounded much better with a British accent). I take the lateness of the hour to be at fault, and will make no reference to pearls before swine in reporting that my talk drew only one question. I did get a phone call after I got back to Oak Ridge telling me it had helped to develop Allen's new point of view, namely, that his calculations, which previously "almost proved the existence of polywater," now proved it not to exist.

However, one unusual bright spot did emerge. All the papers, including the short ones, were to be published together in *Surface and Colloid Science*. I wrote my abstract into a short note and sent it in. Presently, I got it back with the referees' comments. One was a matter of clarification. One showed he didn't quite get the point. But the third said he'd like to have me amplify the paper to explain more about how the impurity hypothesis could explain things. A most perceptive referee. So, I expanded my note into almost the paper I'd originally sent to *Science*. Only now, because of its origin, it didn't have Max's name on it. Sorry, Max.

I hope by now you agree with Max and me that anomalous water or polywater was a great delusion. But you probably wonder how something like that can happen in science. Well, it almost happens lots of times and the present case turned out to be ideal for it. By its very nature, the phenomenon being observed provided samples that were almost too small to study at all. So, the obvious experiments couldn't easily be done, and the imagination of the proponents could reign almost unconfined. Besides, discovery is such fun. The hope of finding something new is what keeps us all going (even Casanova, I expect). But it's fatal to fall in love with your discovery: there is a wellknown tendency to overlook the faults of the beloved.

BOOKS

By JAMES W. CURLIN

Environmental Quality: The first annual report of the Council on Environmental Quality, 1970, U.S. Government Printing Office, Washington, D.C., $1.75.

Jim Curlin, this issue's book reviewer, speaks as an associate director of the ORNL-NSF Environmental Program, to which he came from the Ecology Division. He is currently engaged, as well, in acquiring a UT law degree.

THE NATIONAL ENVIRONMENTAL POLICY ACT of 1960 purports, among other things, to "establish a national policy for the environment . . . (and) Provide for the establishment of a council on Environmental Quality." The Act further requires:

"The President shall transmit to the Congress annually . . . an Environmental Quality Report . . . which shall set forth (1) the status and condition of the . . . (environment) . . . ; (2) current and foreseeable trends in the quality, management and utilization of such environments and the effects of those trends on the social, economic, and other requirements of the Nation; (3) the adequacy of available natural resources . . . ; (4) a review of the programs . . . of the Federal Government, the State and local governments, for remedying the deficiencies of existing programs . . . , together with recommendations for legislation."

In conformance with the requirements of the Act, the Council on Environmental Quality issued its first annual report in August, 1970. The issuance of this report is momentous not only for its intellectual contribution to the understanding of the massive environmental problems facing the Nation, but because, as the President stated in his message of transmittal to Congress, "It represents the first time in the history of nations that a people has paused, consciously and systematically, to take comprehensive stock of the quality of its surroundings."

"Environmental Quality" is more than a mere report required by act of Congress; it is in every sense a handbook of the environment. The introductory chapter discusses concepts of ecology . . . "the science of the intricate web of relationships between living organisms and their living and nonliving surroundings." These interdependent living and nonliving parts make up ecosystems such as watersheds, lakes, and estuaries. Groups of ecosystems which occur in similar climates and share similar characteristics and vegetation are biomes; examples are the eastern forest, prairie grasslands, and desert. The earth, its surrounding envelope of life-giving water and air, and all its living things comprise the biosphere. Finally, man's total environmental system, and the thing we must be concerned with, includes the biosphere plus his interactions with the natural and manmade surroundings.

The concern is for what a growing American population has done and is continuing to do to the environmental system, with a sophisticated technology, by attributing high priority to convenience and consumer goods. Cause and effects are hard to distinguish. Some observers believe that solutions to our environmental problems will require development of a new societal ethic; others express confidence that a technological fix can provide self help; possibly the most rational view is that a combination of both will be required.

Our national efforts to clean up the environment have been less than adequate. We have failed in the past to recognize the interactions among the components of the environment, and have chosen to fragment the responsibility for environmental control among a number of mission-oriented agencies. Recent reorganizations within the Federal structure will overcome some of the deficiencies, but until we attack the roots of the problem instead of treating the syndrome, we can only expect things to get worse. A national policy for the environment is imperative, but the public must participate in the establishment of such policy. Only with the public consensus and confirmation can a national environmental policy succeed.

An informed public is the strength of the American political system. During 1970, the "year of the environment," the public was subjected to a potpourri of environmental oratory ranging from hysterical doomsday dithyramb to MadAve hard-sell. Americans need to resolve the environmental questions in their own minds and establish the priority for attacking environmental problems in a context with our other social problems. To do this the public needs the facts, filtered of as much vested bias as possible. The Council's Report provides this.

Some astounding facts surface in the Report: Less than one-third of the nation's population is served by a sewer system and an adequate treatment plant; one-third is not served by any sewer system at all; five percent is served by sewers that discharge their wastes without any treatment. Nevertheless, industrial discharges contribute three to four times the waste loads of municipal systems. The Federal Water Quality Administration estimates that it will take $10 billion capital investment in municipal sewage plants to meet present legal standards. On the other hand, water and air pollution from industry can be abated for less than one percent of all gross sales; furthermore, in most cases pollution control technology is available to accomplish the job.

This being the case, why haven't we gotten on with it? Pollution control legislation is filled with inadequate standards, and the legal remedies available to the government for non-compliance are insufficient. Unification of enforcement responsibility within the new Environmental Protection Agency will overcome some deficiencies in procedure, but legislative reform is still necessary.

If the responsibility for most water pollution is to be borne by industry, the public must accept the blame for a lion's share of air pollution. The bad actor is the automobile. Forty-two percent of the air pollutants are automotive in origin, and this will be the arena in which Americans prove their mettle: will the public accept expensive pollution control devices with concomitant reductions in fuel economy, and will Americans support the costs of mass transit and become less dependent on the family automobile?

Air and water pollution are holdover problems from yesteryear. We are also facing new challenges. Noise pollution is an increasing concern. Thermal pollution of both air and water adds a new dimension to an old problem. Population expansion — the primary cause of many secondary environmental problems — is even more difficult to approach because of sociological implications. The spiraling energy demand, diminishing mineral resources, super mobility, insufficient timber supplies, and urban problems are other areas for concern.

We have come to realize that many of our time-honored institutions are unable to deal effectively with environmental problems. Local governments exercise primary authority over land use and Anglo-American law has always respected the vested rights of property owners, but can we continue to tolerate the inefficiencies of poor planning and land use for the sake of legal tradition? There is developing a need for a national land use policy; one that can allocate use of the land resource according to the collective needs of the commonweal. It will take imagination and resourcefulness to build such an institution within the framework of the Constitution and common law. Planners and managers must begin to appreciate the enormous interrelated complexity of environmental systems, weigh the tradeoffs of potential environmental harm against the benefits of construction, look at alternatives, and incorporate environmental safeguards into the basic design of new developments.

In the future, technology assessment must be used to understand the direct and secondary impacts of technology innovation.

"Environmental Quality" carries a positive note. Not only are the problems inventoried and their consequences spelled out, but plausible solutions are suggested. It may be true that these solutions represent the official position of the Executive Branch, but the impression is given that the Council on Environmental Quality is a group of professionals dedicated to solving the environmental problems of this nation.

Reactor Chemistry's Roy Thoma received his degrees from the University of Texas, his native state, and has done additional graduate work at the universities of Colorado and Tennessee. He taught chemistry after his WW II service in the U.S. Navy at Sam Houston State College and Texas Tech, transferring to ORNL in 1952. His work here has been with the nuclear propulsion for aircraft and molten salt reactor programs. From 1965 to 1969 he served as the program chemist for reactor operations. Supplementing his account here of last summer's Environment School, Thoma has in his office documents of the School in great detail and cassettes of the lectures, as well as the complete library of enrichment literature that was provided the participants in Fort Collins. He offers it for perusal by anyone interested in looking further into the experience.

The Westinghouse Environmental School

By ROY THOMA

(The Westinghouse School for Environmental Management, a four-week course held last summer at Colorado State University, offered a comprehensive seminar in environmental problems, particularly those that are related to power production. Attending the school in Fort Collins were 28 representatives of public and private power utilities and the regulatory agencies that included the Atomic Energy Commission, Consolidated Edison of New York, and Pacific Gas and Light of California. Two of the participants were from Oak Ridge: Joseph Pidkowicz of Reactor Development and Technology, AEC, and ORNL's Roy Thoma of the Reactor Chemistry Division. Herewith is Thoma's account of the summer experience.)

IN A SPEECH to the students of the Westinghouse School for Environmental Management, Don Burham, chairman of the board of directors of Westinghouse, said to us:

"The world is facing many problems, but two rise above all others. Of course, World Problem No. 1 is how men can live together in peace. But World Problem No. 2 is how men can live on this earth without destroying its capability of supporting life. While the two problems are different in many respects, they are similar in that each, if unsolved, can result in man's ultimate destruction."

It is well known that Westinghouse is a major supplier of products that are related to power production, both here and abroad. It builds the pressurized water reactor; it also builds and sells steam turbines, electrical transmission equipment, atmospheric monitoring devices, and other components of the power industry. It has been in an excellent position, therefore, to recognize the imminence of problems that concern environmental quality. Obviously, as well, it has at this time a big stake in the nuclear power industry. As nuclear power furnishes a larger fraction of the nation's energy, moreover, Westinghouse expects to become even more prominent in the power industry. With these factors in mind, Westinghouse began last winter to formulate plans for the first school for environmental management. To direct it, they chose Jim H. Wright, a chemical engineer who had been a hotshot trouble shooter in the Power Systems Division for many years. Jim formulated his idea of the school in the following statement:

Four of the participants at the Westinghouse Environmental Management School learn how a spectrophotometer helps to determine the chemical constituents of natural waters. On the far right is Oak Ridge's Joe Pidkowicz of AEC-RDT.

"The problem of improving the environmental quality can be talked about by anybody at any time. It can be solved only where there is enough interest and determination applied to it by all parties. That harmony of purpose where all men are working toward a common goal is possible only where reasonable people first share the same information."

The curriculum for the four-week course assumed that the participants would come almost entirely from the public and private utility companies. Attendance was expected to be about 35. Possibly the registration fee of $6000 screened out some representatives. When all the checks were in the class numbered 28 men, among whom were representatives of some 16 utility companies. Five were from the AEC: besides the two of us from the national laboratories were William A. Williams, Desalting Branch; George Sherwood and ORO's Joe Pidkowicz, RDT; Charles Osterberg, Biology and Medicine; and Dick Grill of Regulatory. The Department of Interior and the Federal Power Commission sent representatives. Julio Fragoso, who has spent some time at ORNL, was there from the Puerto Rico Water Resource Authority. Our one European was Ramano Gasparini representing the National Electric Energy Board in Italy.

Most of the representatives from the utilities had been appointed by their companies to head formal antipollution efforts, and carried such titles as Supervisor for Environmental Quality, Director of Environmental Planning, Environmental Surveillance Coordinator, and the like. Five were lawyers. The effort had existed within the companies for some time, but had been consolidated into a formal structure and made part of the administration only within the past year.

Curriculum

The course curriculum focused principally on the relation of power production to the environment. Emphasis was given to legal requirements, legislation at the federal and local levels, research methods for testing pollution of water, air, and land, and new developments in environmental engineering.

The first week's study concerned atmospheric characteristics and their influence on air pollution, air quality criteria, and public health.

R. L. Smith, of the University of West Virginia's Department of Forestry, described various ecosystems and told how man's pursuit of energy sources has caused essential changes in many of them. He noted, for example, that thermal discharges can quickly change an "infertile" lake into a fertile one as algae build up and fish populations change.

Herbert Riehl, Colorado State professor of Atmospheric Sciences, spoke of atmospheric currents as carriers for polluting agents, and evaluated the capacity of different layers of the atmosphere to carry pollutants.

Maynard Smith, leader of the Meteorology Group at Brookhaven, discussed the effects of atmospheric turbulence on smoke and gaseous plumes. He illustrated dispersion patterns as they are affected by air turbulence, and demonstrated methods for calculating the combined total effect from multiple plumes, with their interaction. It was a gratifying surprise to me in this session to learn that ORO's meteorologist Frank Gifford enjoys international stature in the field of atmospheric turbulence. His work was cited repeatedly as definitive, and there were many references made to "Gifford's classic work in this field."

C. Stafford Brandt of the Agricultural Research Service discussed air quality criteria for effects on vegetation of sulfur dioxide and fluorides. He made the statement that nearly all vegetation alongside the roads in the northeastern megalopolis shows evidence of sulfur damage.

Emanuel Landau, from the newly formed Environmental Health Service, spoke on the relationship between air quality and public health. He discussed the establishment of air quality regions as set by the Air Quality Act of 1967. However, he noted, in the U.S. the primary responsibility for air quality standards is left to the states. He pointed out that particle size of pollutants has a distinct effect on the toxicity of other pollutants — SO_2, for instance — but that quantitative data on the synergistic effects are not known.

Dade W. Moeller, of the Department of Environmental Health at the Harvard School of Public Health, noted that the principal radiation exposure to man is of terrestrial origin, with primary exposure coming from construction materials; wood in houses is a much lower contributor than stone and concrete (moral: sleep on the second floor). Natural background, he said, is the greatest contribution to the radioactivity burden.

Moeller pointed out that the average exposure of the U.S. citizen from background radiation is 125 millirems per year. Reported natural background levels for the U.S. range from about 90 to 200 mrem/yr. Background radiation is only one of the many sources to which the world's population is exposed, however. Radiation-generating machines and radioactive materials constitute the principal man-made sources. The genetic dose from medical and dental x-rays in 1964 was 55 mrem/yr. "Probably some 10,000 physicians are licensed to receive radionuclides for use in the diagnosis or treatment of three to four million patients per year, and it is estimated that over 50% of the medical users dispose of their wastes through direct (unmonitored) discharge to the sewer," Moeller said.

For the U.S., natural background accounts for a little over half the current genetic dose to the population, medical and dental x-rays account for about 30—40%, and something less than 5—10% arises from nuclear reactor operations, weapons testing fallout, and other man-made sources.

Sidney Edelman, special counsel in environmental affairs for HEW, reviewed the progress or the lack of it in air pollution cleanup since the Federal Air Quality Act of 1967. He said action has been slow and tedious. He predicted that new, tougher legislation, already in the works (the Muskie Bill), would be passed. He expects the first laws will be designed to impose stringent standards controlling SO_2.

Cyril Comar, at present the head of the Physical Biology Department at Cornell but from 1948 to 1954 head of the University of Tennessee Agricultural Research Laboratory in Oak Ridge, spoke on terrestrial radio-ecology. In appraising the effects of radionuclides, he introduced the concept of "criticality of factors," that is, ". . . those factors which are so important that no others can make any significant contribution to the risk." Of the critical organisms, he said, none can be considered to be as critical as man. When man is in trouble, the effects on other organisms no longer matter. Regarding the critical situation: in reactor programs, for instance, it appears in the reprocessing plant rather than in the reactor operation itself. There are also critical pathways, foods, organs. He defined strontium-90, iodine-131, and cesium-137 as the three nuclides of principal concern in the biologic cycle. He then traced the mechanisms of transfer of each through the food chain.

James Lodge, of the National Center for Atmospheric Research (NCAR), said that NCAR is attempting to estimate particulate concentrations in the atmospheres at high altitudes in order to study the albedo effect. Research in this area has disclosed that the sulfur concentration in the atmospheric layers at 65,000 ft have increased by about one order of magnitude in the last decade.

Water Pollution Next

Discussions of hydrology and river flow behavior followed in the second week, along with talks on water quality, marine biology, limnology, and biologically related monitoring programs.

To help us during this intensive learning period we had provided us an exhaustive library of pertinent material — about 90 lb of it, as a matter of fact.

Some period of the day was allocated to discussions, often lively, between the students and the day's lecturer. Frequently the evening meal was followed by an address.

A featured speaker during the second week was Sen. Gaylord Nelson, who enumerated some common abuses of natural resources and predicted that laws would be forthcoming that would prevent taking resources from the land or sea until they were really needed or until the technology exists to do it right.

Also during that week a lecture by D. W. Pritchard of Johns Hopkins was scheduled. Pritchard has a reputation as the world's foremost authority on the distribution of heated water discharges into natural water systems. Unfortunately, his appearance was prevented by an accident in which he had broken his leg, and so Westinghouse, rather than cancel his talk, sent a television crew to video tape it. When it was played back at the school, a two-way telephone system enabled the participants to talk directly to him for the question and answer session.

Other speakers at these events included Clarence Carlson of Cornell's Department of Fisheries, and UCLA's dean, Chauncey Starr. In addition, Rep. Craig Hosmer lectured later in the course when administrative law was the principal topic. There were, most of the time, no visitors during the lectures; on the day administrative law was discussed, however, some 35 lawyers from different parts of the country showed up to observe.

Laboratory Work

Throughout the course, we were given assignments in environmental problem-solving. These became increasingly complex with each project, until finally one came along that hit the gong. It had to do with an aquatic situation: we were given several pages of data on river conditions: flow rate, natural temperature variations, chemical constituents present at various times due to current activities of a paper mill, a steel plating operation that included dumping its pickle liquor in the river, and the effluents from a municipal sewage disposal plant. Given this situation, a nuclear plant was then added at a specific location on the river. With the volume of the cooling water discharge known, as well as the temperature rise, we were asked to determine the effect on the aquatic ecosystems present in the river.

Our response to this was a student protest, lined out in a formal presentation. Settlement of the dispute was eventually achieved, and in place of the problem the school was assigned a debate as to whether or not the new plant would eliminate the striped bass population from the river. Two study groups worked on this, and in the last week the matter was taken to court. A real federal judge was brought in, and each side argued its case in the trial.

(During this week some 20 members of the press arrived in Fort Collins to cover the school. The students became sharply concerned about coverage of the trial and stipulated that the press must be barred from attendance. I don't know what this cost the companies in press relations, but it was deemed to be far less expensive than the alternative.)

At the close of the testimonies, the judge decided that the intervenors, who were the plaintiffs, had an insufficient case, and that the plant should be built.

The Third Week

Field trips occupied the third week of the course. The first was to the University of Wisconsin's Environmental Awareness Center; there Philip H. Lewis, director of the Center, described the unfortunate results of insufficient planning and awareness of the interrelationships of natural environmental parameters. Inspection trips to appropriate locations in the Wisconsin area and laboratory demonstrations and lectures by members of the Center made the Wisconsin trip a valuable experience.

Next stop was TVA, which enjoys an image of world leadership as a pioneer in environmental management. Dr. O. M. Derryberry, TVA's director of the Office of Health and Environmental Science, recounted the history of TVA from its inception as an agency charged with developing the resources of the Tennessee Valley. The public health problems of reservoir creation were recognized early, leading to diagnostic studies of lake ecology. Dr. Derryberry noted that man-made environmental changes are acceptable when they occur with full knowledge of the effects and resultant planning.

Fred Thomas, chief of the Air Quality Branch, discussed TVA's experience with tall stacks, citing their benefits, costs, and shortcomings. He described TVA's pre-plant operation programs and post-operation field research work. He also described the methods they employ to determine whether, under unfavorable meteorological conditions, TVA coal-fired plants should reduce or curtail their output.

Another staff member gave us a comprehensive and critical review of the technology involved in controlling sulfur oxide emission. He covered a full range of absorbents from granular to finely divided solids and liquids, and ended up with a detailed cost breakdown for the wet limestone scrubbing process.

Milo Churchill, chief of the Water Quality Branch, talked about thermal effluents, citing data from Widow's Creek, Colbert, and Paradise steam plants. Most zooplankton, he said, seem tolerant of up to 96°F temperature water and even of the shock of passing through condensers.

Gil Stone of the Chattanooga TVA office sketched out the changes in proportion of TVA power generated by nuclear and fossil fuel plants and then described the radioactivity monitoring program at Brown's Ferry.

A slide lecture introduced us to the Colbert plant's environmental program, after which we toured the plant. The most pertinent feature of the tour was a direct look at the ammonia process SO_2 recovery pilot project.

Conclusion

To summarize, the experience of the course made it evident that the public has not yet given serious thought to weighing the costs and benefits of accelerated power production. One consensus of the participants was that with increased community resistance to siting of power plants, the specter of power shortage crises becomes very real. This suggests that for an institution like ORNL to contribute significantly to the improvement of environmental quality it will be important to achieve an identifiable technological advance in the very near future — and preferably one that pertains to pollution abatement. There are innumerable areas where the ingenuity of research teams such as ORNL has can contribute technically to the alleviation of pollution. A warm reception awaits new ideas in many ORNL offices today. Try them and see.

Take A Number

BY V. R. R. UPPULURI

THE PIGEONHOLE PRINCIPLE

Let us assume that the maximum number of hairs on a person in New York City is less than the population of that city. Then the pigeonhole principle asserts that there are at least two individuals in that city with the same number of hairs. (Finding those individuals is entirely a different matter.) Of course, this is a picturesque way of saying something like this: If we have 5 pigeons in 4 pigeonholes, there is at least one hole with two pigeons. To most people this is evident; nonetheless, the applications of this principle are fascinating.

One can use this principle in proving Ramsey's Theorem: Given a sequence of $n^2 + 1$ distinct integers, it is possible to find a subsequence of $n + 1$ entries which is either increasing or decreasing. As an example, consider ten soldiers, no two of them the same height, standing in a line. There are $10! = 3{,}628{,}800$ different ways the soldiers can arrange themselves, but in *every* arrangement at least four soldiers will form a series of ascending or descending height.

Would You Believe!

If you have a set of n integers, then there is a subset of them whose total is divisible by n. For example, suppose the set consists of $\{1,3,7\}$; there is a subset consisting of $\{3\}$ which is divisible by 3. If the set consists of $\{1,5,9,33\}$, then the subset is $\{1,5,9,33\}$ itself. Can one say that it is enough to prove this statement, when the set consists of n prime numbers?

The Midland Encounter

By COURTLAND RANDALL

A SPIRITED CITIZENS' GROUP crying out *for* a nuclear reactor? It happened, for the first time, in Midland, Michigan, last November. The occasion was a controversy over Consumers Power's proposed 1325 MW(e) plant with an 880-acre cooling pond on Midland's outskirts. The plant is also designed to deliver four million pounds of steam per hour to Dow Chemical Company, a nearby chemical processing complex. Since Dow is the principal employer in Midland, the plant's dual output is a key issue in the controversy.

A well organized, vocal group opposed to the reactor launched a public attack in anticipation of construction permit hearings in Midland on December 1. Six organizations banded together to form the Saginaw Valley Nuclear Study Group, including the Sierra Club (national); United Auto Workers of America; Michigan chapter of TROUT, Unlimited; West Michigan Environmental Council; Environmental Law Society of the University of Michigan; and the Citizens' Committee for the Environmental Protection of Michigan. Also concerned was Mrs. Mary Sinclair, who had been active in the Palisades reactor controversy across the state on Lake Michigan. She had marshalled the classic arguments, published them in an effective booklet titled "Nuclear Power and Public Concern," ad brought them forcibly to the attention of Consumers Power executives and officials of the State of Michigan. In testimony presented before the Michigan Public Service Commission on the Palisades issue in December 1969 Mrs. Sinclair attributed her interest to attending a "seminar in Environmental Science conducted by many fine scientists from a number of universities as well as AEC scientists from Brookhaven Laboratory. Up until this time I had believed what I had been told by promotional literature about nuclear power — that it was 'safe and clean.' However, here I became aware of the many serious problems in the nuclear power industry — and for the first time, I discovered that far from solving pollution problems, nuclear power plants were regarded by many scientists as the most dangerous source of environmental contamination that technology had yet devised. I followed up the research and scientific literature that was discussed, and compiled a report out of the most pertinent citations."

In a full-page ad in a Midland newspaper late in November, the Saginaw Valley Nuclear Study Group listed its concerns, some of which were the

When, last fall, a Michigan town turned to Oak Ridge for the information needed to see it through an anticipated reactor siting debate, Court Randall, chairman of Information and Exhibits Division at Oak Ridge Associated Universities, was given free rein to implement his own theory on how to avert the confusion often experienced in such encounters, by means of a meticulously prepared program of honest communication. Taking with him a team selected from ORAU's experienced "traveling teachers," and arming himself with lab materials and reading matter chosen for quick and thorough coverage of his subject, he moved in on the community last November and conducted a series of workshops for the concerned citizens. Herewith his abbreviated version of the experience. He has submitted a detailed report to ORAU, available to anyone who would like to study this extremely interesting venture in depth. Since his graduation from George Washington University, the author has served as a science educator for several agencies. Among his more interesting assignments was that of science consultant to the U.S. Science Exhibit at the Century 21 Exposition in Seattle in 1961. He joined ORAU in 1965, and has occupied his present position since 1968.

large size and experimental nature of the Midland plant, its proximity to a population center, specific exclusions regarding radioactive contamination in homeowners' policies, the unpredictable nature of thermal and radiological effects, the susceptibility of children and pregnant women to radiation effects, and the problem of combining radiation and chemical effects. The ad cited the recommendations of Teller and Morgan that nuclear power plants be placed away from population center, preferably underground; a request by the AEC Reactor Safeguards Committee for more research on large water reactors; a decision by a German safety minister to delay nuclear plants adjacent to a chemical complex in West Germany; and a statement that Louis H. Roddis (president of Consolidated Edison) had deplored lack of engineering standards for large reactors, among other arguments.

Consumers Power Company, battle-hardened by the Palisades experience, organized a statewide educational effort of admirable quality and proportions. Romney Wheeler, Director of Public Relations for Consumers Power, received the 1970 Atomic Industrial Award for public understanding for his educational program. However, the most effective counter-force to the efforts of Mrs. Sinclair and the Saginaw Valley Nuclear Study Group was Dow's position as a big energy user and a major employer in Midland. It was clear to responsible Midlanders that if Dow could not get low-cost process heat in Midland, it would have reason to expand its chemical operations elsewhere, perhaps on the Guld Coast nearer fossil fuel sources. Stimulated by such concerns, community leaders representing commercial interests determined to emphasize the benefit side of the ledger. But as of mid-October these people had not done their homework, and were disturbed by opposing arguments, particularly since the home of at least one of them was within a few thousand feet of the proposed cooling pond. So they turned to Oak Ridge for assistance.

Those of the ORNL staff, including its director, who have struggled across the science-religion gap in recent ORAU conferences, may have wondered if it was worth the effort. Well, it was, if only in the stimulation of one Dr. Wayne North, minister of the First Methodist Church in Midland, Michigan. He had attended, along with 30 or so of his colleagues, the second ORAU Science for Clergy Conference in the summer of 1968. Back in Midland, largely as a result of his Oak Ridge experience, he emerged as the logical leader of the pro-reactor forces. The Midland Nuclear Power Committee was formed in mid-October and North was on the phone to ORAU's Director Wm. G. Pollard a day later asking for speakers. We suggested that a workshop approach might be more productive than the contention-and-confrontation tactics of more formal speeches and panels of experts. Dr. North's group readily agreed.

Although North's committee was disposed to favor the reactor proposal in order to assure Dow's growth in Midland, the group was disturbed by the fearful questions raised by opponents and felt insecure in its own competence to answer them. The group turned to ORAU and ORNL rather than Consumers Power or Dow. To gain some intimate knowledge, and to kick off a promotional campaign in Midland, the MNPC accepted an invitation to visit Oak Ridge for a seminar, tour, and lecture program. Fifty Midlanders came to Oak Ridge at their own expense on a chartered plane on November 9. Highlights included a lecture by ORNL Director A. M. Weinberg at the Graphite Reactor, a two-hour question-and-answer panel of ORNL staff members selected for their interest and expertise, and a dinner address by Dr. Pollard. The questions asked by the visitors reflected their sober concern. They enquired about the safety of storing and shipping radioactive waste, whether the safety standards are strict enough, whether the cooling pond would cause undesirable fog, what would be the radionuclide concentration in fish, the effects of thermal release, the desirability of AEC's dual role, when controlled fusion would be a reality. They asked why AEC doesn't agree with Gofman and Tamplin.

Meanwhile the Information and Exhibits Division staff of ORAU studied the Midland controversy with the cooperation of Consumers Power Company and prepared for a two-week workshop series in Midland. Emphasis was placed on responding to the real questions in the minds of the Midlanders, and avoiding the local aspects of the controversy. The key element was determined to be emphasis on the basics of radiation and reactors.

Four members of the IED staff worked in Midland on the workshop series from November 12 to November 24. Several workshop modes involving one or more teachers were attempted. All worked well, but the team approach with two people appears to have been most practical. In all, we conducted 13 workshops and seven civic club lectures for a total audience of 580 influential Midlanders.

Although there were scientists and engineers on the MNPC and among workshop attendees, and in spite of the fact that Midland citizens are more technically oriented than average by virtue of the local industries, we set the level of our workshop presentations to that of the layman's understanding. We found they were by no means too elementary for our audiences. Unfamiliarity with the semi-technical vocabulary was the principal block, although this was not at first obvious. (One nice thing about workshops is that the direct, interpersonal communication often brings out recognition of the *real* problems.)

Of those attending the daytime workshops, about half were housewives and the rest were retirees and city workers. In the evening, most of the attendees were white collar workers, with a scattering of technical people and housewives.

Common questions involved possibility of reactor explosions, waste shipment and onsite storage of high level wastes, the effect on guidelines of a large number of reactors, and the time it will take to develop a fusion reactor. Some of the common misconceptions were: 170 mr/yr relates to a plant rather than a person, the AEC sets radiation levels, fusion power is just around the corner, waste shipment will be hazardous, etc.

Those who prepared and presented the workshops were young generalists with science teaching background who studied the nuclear controversy in considerable detail. They were experienced in handling a variety of group situations.

The style of presentation could be characterized as follows:

— Positive, confident, but not contentious exposition: thermal disadvantages were stated matter-of-factly; typical radiation exposures (background, x ray, jet trip, etc.) were listed on the blackboard along with "fence exposure" without the workshop director driving home the point. He let the audience discover the quantitative differences.

— Everyone participated in real experiments, with sources, counters, and shielding.

— Presentations and experiments stressed *basic* facts and figures on radiation and reactors, not details of local reactor.

— Presentations were flexible, and alive to key local issues. It took three days of modifications before we could sense precisely what Midlanders needed to know, and respond to it. Success was measured by questions following the programs.

— Workshops ran two hours, on a precise schedule. Typically more than half of the attendees remained an extra hour for less formal discussions. One session with three local teachers lasted until midnight.

One important aid in maintaining this style was the fact that we were *invited* to Midland by the MNPC. This committee also invited the workshop participants. We could always say, and did so on a number of occasions, that we had been asked by the MNPC to stress basics and not enter local arguments.

We know that this style was effective in Midland. An anecdote may be of interest. We had anticipated that the worst that could happen would be for a hostile young physicist from Dow, angry at his employer and the world, to attend and attempt to demolish our credibility. He did, in fact, materialize, though in gentler guise, as a Dow chemist working on control rod sealants. He was a member of one of the opposing groups. He participated in the workshops, trying to be helpful from time to time by adding items of technical information. After the program he announced his stance quietly to the teacher and complimented us on the program, saying, "Though I'm opposed, we need more of this basic knowledge in Midland and less scare tactics." He went on to explain his position, which might be interpreted as follows: **"We will most certainly have hundreds of reactors producing power in my lifetime. But the present jump from hundreds of MW to thousands is a great one, and by raising a little hell now, a few of us courageous souls are going to force the AEC, the suppliers, and the utilities to do their jobs right! If it costs a couple of years of tough economic sledding, so be it! It may even speed fusion."**

The greatest danger to our posture came in questions following the presentations, and the toughest questions followed the shorter civic club lectures rather than the longer workshops. Typical tough questions included the rambling "what if. . . ?" in which the questioner describes the worst imaginable catastrophe. If the teacher states categorically that the probabilities of that are too low for serious consideration, he risks offense, contention, and opinion-stating. He must say carefully, "Yes, anything *can* happen, but it's up to you (the questioner) to try to determine how probable is the sequence you described, based on your own reading, listening, and thinking." The second toughest question was "Mr. Lecturer, just what is your personal opinion on such-and-such an issue?" The speaker must politely decline to answer, perhaps saying, "My opinion is of no importance to you." Another pitfall was the rhetorical question: "In view of what you've just said about radiation in perspective, how can these people be so critical?" If the speaker answers, "That's a good question," he's lost the posture. Yet another problem occurred when members of the group fell to arguing among themselves and tried to draw in the workshop director with "Mr. Oak Ridge has just said that. . . ." This happened, and the teacher objected without offense.

At the close of one workshop a person, not very knowledgeable but inclined to favor the reactor, said, "This is all very good, but you fellows didn't convince us that nuclear power is the only way to go." Success!

The three-point outline as originally planned consisted of (1) the nature of radiation, its sources, and how it's controlled and regulated; (2) energy needs for electrical power, and the relationship between energy production and environmental quality; and (3) the workings of a reactor and reactor safety. This was modified during the first three sessions in Midland with the virtual elimination of point (2), apparently not an issue in Midland.

The Midland case is unique in many respects. It would be foolish to attempt a direct extrapolation to some other region undergoing a reactor siting controversy. What ORAU and ORNL did in the Midland case was prove that we can assess a local situation realistically, adapt to local needs, stay alive, and make a contribution toward the kind of "environmental literacy" called for in the President's message to Congress in which he introduced the First Annual Report of the Council on Environmental Quality (see page 18): ". . . We must seek nothing less than a basic reform in the way our society looks at problems and makes decisions. . . . It is . . . vital that our entire society develop a new understanding and a new awareness of man's relation to his environment — what might be called 'environmental literacy.' "

Scene from one of the workshops held in Midland to acquaint concerned citizens with those phenomena unique to nuclear technology. Adjusting the counter is ORAU's James Ogle, a onetime "traveling teacher."

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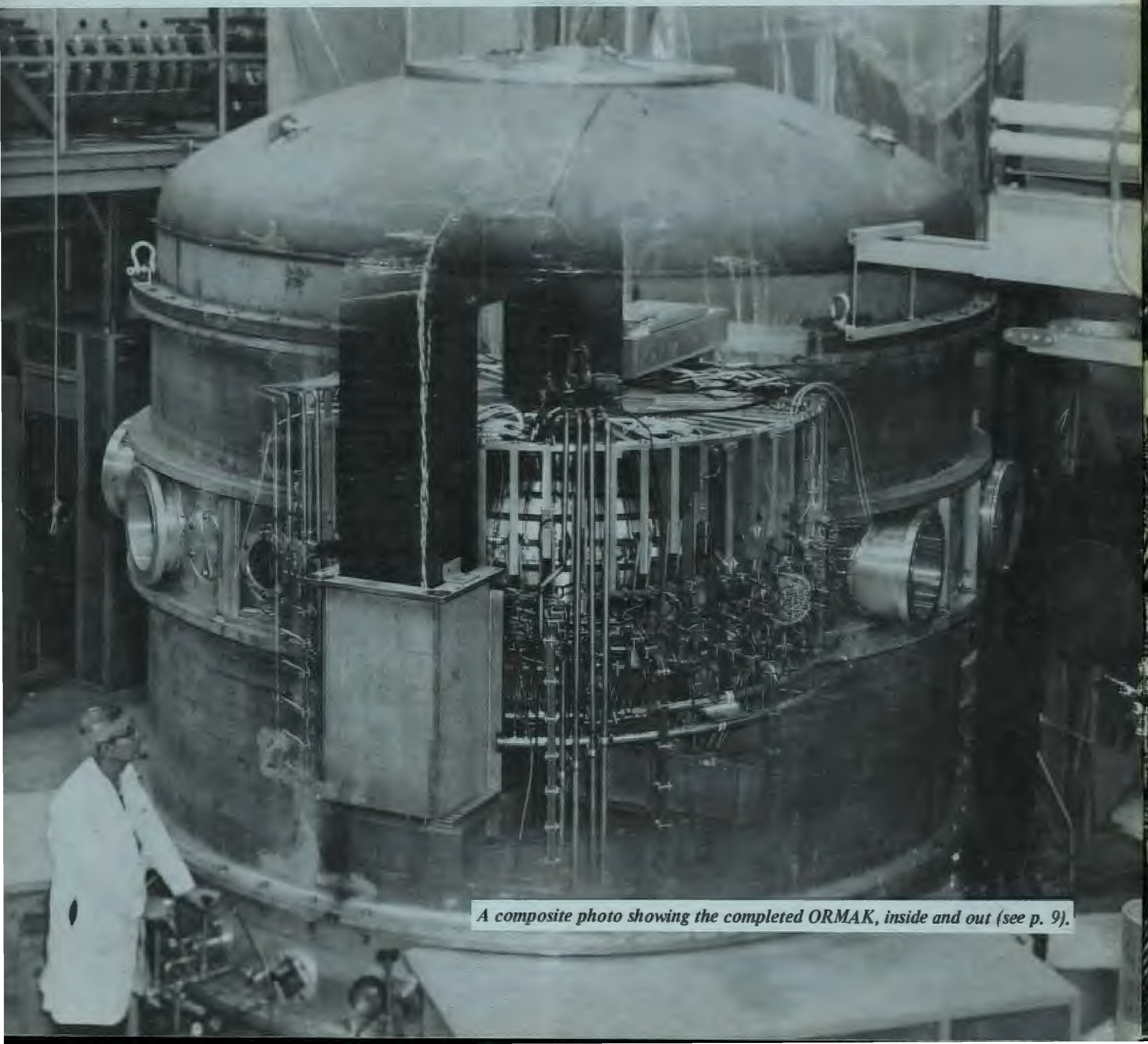

A composite photo showing the completed ORMAK, inside and out (see p. 9).