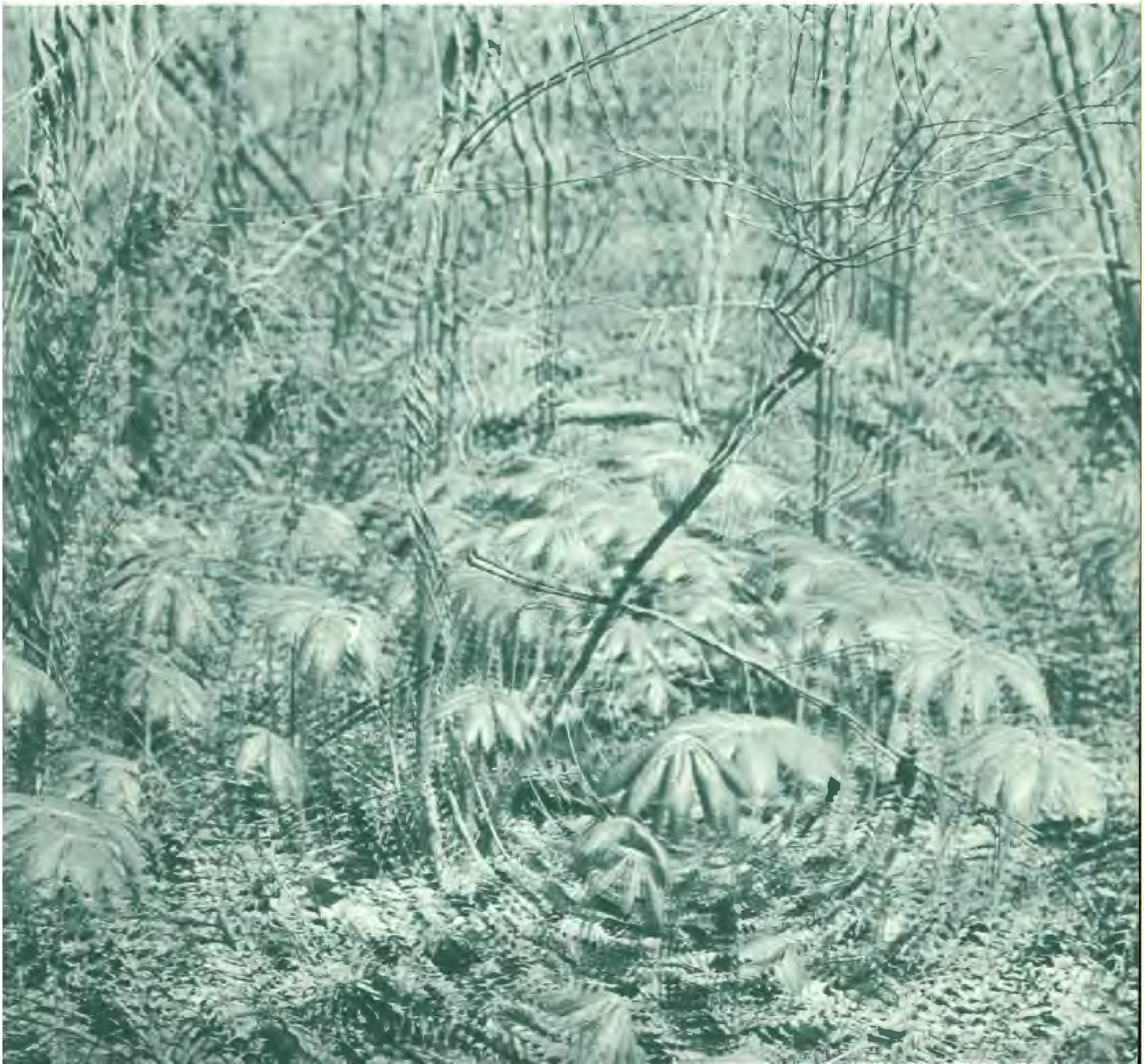
Review

SPRING 1973

OAK RIDGE NATIONAL LABORATORY



THE COVER: The plants' eternal cycle of sprouting, budding, leafing, blooming, bearing, and dying back is a rhythm closely attuned to all of us. In his article, opposite page, Fred Taylor postulates the predictive powers of this rhythm, suggesting that crop management could benefit by its messages. On the cover is a clone of mayapple, or mandrake.

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Review

OAK RIDGE NATIONAL LABORATORY

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OAK RIDGE NATIONAL LABORATORY
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The word phenology, a syncope of "phenomenology," has been used since 1853 to mean a study of the influence that climate has on such recurring annual events as the blooming of flowers, the coloring of deciduous leafy plants, or the migration of birds. (And for those of us who need further help, a syncope is a word that in its new use has somehow lost one of its syllables.) Fred Taylor, who photographed the flowers for the current ORNL Annual Report, is a botanist in the Division of Environmental Sciences currently engaged in a Ph.D. program at UT on forest ecology. He has a theory that the ancient practice of looking at the blooming dates to predict the harvest, fallen into disrepute with the advent of "scientific agriculture," was dropped prematurely. In a paper delivered recently to a University of Minnesota symposium on "Phenology and Seasonality Modeling," he stated his theory in depth. This article is taken from his talk, which is available in its entirety from the Department of Commerce's National Technical Information Service under the title "Phenodynamics of Production in a Mesic Deciduous Forest," EDFB-IBP 72-8.

Can the MANDRAKE predict the crops?

a new use for phenology

By FRED TAYLOR

LIFE CYCLE PHASES of plants begin with seed germination, progress through vegetative stages to reproductive cycles, and terminate in senescence and leaf drop. While the sequence of these events is determined by the genetics of the species, the onset of a phase is often controlled by one or more environmental factors. It can be a response to moisture, light, or temperature thresholds, and so is often manifested with some regularity corresponding to seasonal changes.

Associated with the phenodynamics of plants are changes in biomass and productivity. Such quantitative changes reflect an influence of meteorology, for instance, on production in agricultural systems, and also offer help in analyzing natural ecosystems such as deserts, grasslands, and forests. Because of these seasonal influences, the period during which a particular event occurs can be predicted providing the meteorological information is available. Accurate predictions of phenological

events have been made this way by climatologists and agrometeorologists to select optimum seeding dates and in the planning of other agricultural practices. Further applications of this knowledge have been used in crop pest control to maximize production.

In terms of economic investment, quantitative phenology, called phenometry, has been successfully applied to predict harvest dates, resulting in the scheduling of agricultural practices to ensure both quality and an even flow of produce to food processors. In fact, in defining the beginning, duration, and end of seasons, phenological records are found to be better indices to the bioclimatic character of local areas than recordings made by mechanical instruments. This suggests that specific plants and selected phenological events could make good indicators on which to key major land management practices and productivity studies, such as those performed in the International Biological Program.

The Oak Ridge Site

The Oak Ridge site, one of five selected areas of the Eastern Deciduous Forest Biome in this program, is committed to intensive ecosystem analyses and the development of process models, such as photosynthesis and nitrogen and mineral cycling. Many aspects of these studies can be evaluated and predicted on the basis of phenological considerations. For example, the beginning and end of the growing season define the period over which to assess growth rates, accumulation, and turnover of standing crop.

Most of the AEC reservation has remained relatively undisturbed except for some recent forest management practices. The approximately 15,175 hectares, mostly forest, have long been used for ecological research. Since access to the area is denied to the general public, the reservation affords maximum protection for long-range ecological studies. This protection further offers a unique continuity of year-to-year study of single individuals or communities.

The general area was formerly classified as an oak-chestnut association within the Ridge and Valley Province. As individual chestnut trees were eliminated by the blight, suppressed or codominant species like oak and hickory replaced the chestnut as major canopy components. Its proximity to the Cumberland Mountains relates the present oak-

hickory association to the mixed mesophytic associations of more moist cove and slope forests. It is near the base of ridge and slope habitats that yellow (tulip) poplar becomes abundant in secondary communities with a rich understory flora reminiscent of more mesophytic communities. A yellow-poplar-dominated forest ecosystem has been the subject of numerous experiments. The phenodynamics within the major forest association (oak-hickory) and certain subcommunities, as observed to be influenced by meteorological parameters, indicate the importance of short-term phenological events to functional processes within the forest ecosystem.

Phenology of Flowering

Plant growth usually diminishes with the onset of the reproductive phase, that is, flowering. Advent of flowering therefore becomes an indication of maximum productivity within a species. Among selected species, or "indicators," the dates of first flowering observed by regional networks of observers form the basis of maps to portray the arrival of the growing seasons and to offer guides to agricultural practices. Records of dates of first flowering for major species at the Oak Ridge site have been taken since 1963. These records have been summarized in a "phenogram" illustrating the mean date of first flower between 1963 and 1970. An early peak in the number of species flowering, followed by a decline and a subsequent increase in early summer, has been widely observed. Such a phenomenon, resulting in a bimodal distribution in the number of species flowering, probably indicates a distinction between spring and summer blooms.

And there are, indeed, within the local forest ecosystem, two periods of peak flowering in the first six months of the year, representing spring and early summer: mid-April (week 16) and early June (week 23), respectively. The first is composed of woodland species, mainly herbaceous, while the early summer peak represents species indigenous to thickets, roadsides, or openings within the forest. The peak in mid-March, at week 11, represents those species probably most responsive to day length, such as the mustards.

As suggested, the decline in flowering following the spring peak is very likely related to the leafing-out process, or canopy closure by dominant overstory species. Following the spring maximum

in flowering, most of the leaves of the tulip poplar, the dominant canopy species of the lower slope forests, have emerged, and the transmission of incident radiation to ground level is accordingly reduced. By the 24th week, with total leaf emergence, light transmission is reduced to 11%. Light extinction through layered canopies is an important consideration in the photosynthetic efficiency of plants. Within the more mesic sites of the oak-hickory forest association, the period of peak flowering (i.e., the largest number of species in bloom) occurs before extreme reductions in solar radiation, thus maximizing photosynthetic efficiencies. The second peak flowering season occurs after the canopy has closed. However, this floristic component in the development of the forest ecosystem is composed of species found in more open habitats, like roadsides and forest edges, where light transmission is not significantly reduced. Early canopy development by dominant tree species and subsequent reduction in light transmission to the forest floor might seriously alter the photosynthetic efficiency of understory species. Consequently, the completion of the life cycle phases might result in a reduced seed crop, jeopardizing the continued existence of certain species within the ground layer.

Influence of Climate

A phenogram has been constructed, showing the range in flowering periods for 133 species on the Oak Ridge site over a period of seven years, 1963—70. Considerable range in date of first flower within a species is evident, especially among herbaceous plants, a variability that is due in part to differences in meteorological factors.

In fact, the relationship between the thermal environment and phase of development of certain plant species was noted early. Since those early observations, numerous investigators have attempted to predict with greater precision the dates of seeding, flowering, and harvest. A result has been the concept of heat units, or degree-days, since employed by agriculturalists, with varying success, in attempts to characterize the developmental phases of plants from trends in the thermal environment. In thermal summation techniques, degree-days have been used to predict plant development in such homogeneous systems as corn or peas. The development of the heat unit concept and subsequent modifications have been discussed in detail, where microclimatic differences have been identified as a major consideration. Such considerations have proven to be important to the

phenological development in managed forest or natural ecosystems. Degree-days have been used to depict differences in thermal environments between successive years. Within the heterogeneous structure of a forest ecosystem a common temperature threshold has little validity for analytical techniques, since not all species respond strongly to temperature.

If a diagram depicting flowering within the oak-hickory forest during 1970 is superimposed on a phenogram illustrating mean date of flowering between 1963 and 1970, the early spring peak is shown to be advanced by seven days to week 15, whereas the early summer flora is delayed seven days to week 24. Monthly average temperatures for 1970 compared with the same periods offer little explanation for the variability. However, by characterizing the thermal environment by the summation of daily mean temperatures, one observes that the thermal climate at year day 112 (week 16), the mean peak flowering period between 1963 and 1970, is attained nine days earlier in 1970. This suggests that the earlier spring flowering in 1970 was related to the thermal climate. Likewise, the thermal environment during 1970 for the early summer component was also attained earlier (by 14 days) than the same temperature regime during the mean peak flowering period between 1963 and 1970 (year day 161). Flowering was delayed seven days in 1970, which suggests that factors other than temperature were more important.

As noted previously, the early summer flora (week 23) is composed of species indigenous to more open, dry habitats, where moisture may be a more important consideration to plant development. Total precipitation during 1970 suggests that there was a moisture deficit (approximately 10%) in comparison with the ten-year mean between 1961 and 1970. During the first half of 1970, four of the six months received below-normal precipitation. Especially noticeable is the deficit during May, which was probably critical for plant species occupying drier sites. This deficit occurred prior to the early summer flowering peak and could be responsible for the delay previously mentioned. Moisture, light, and temperature interactions are all known to advance or delay timing of certain phenological events. These alterations, in turn, influence other processes active within the ecosystem: availability of food base to secondary producers, pollination, and rates of accumulation and turnover of standing crop are examples.

Climatic Effects within Species

In addition to understanding the responses to environmental factors, it is essential, in determining the growing season, to know how each phenological event in a single season is related to its preceding or successive phase, especially for productivity studies. Such relationships between events are easily discernible in the construction of phenological strips or spectra. Within forest ecosystems composed of diverse species, these graphic illustrations identify species that may be in competition because of their similar phenological development. Certain widely distributed species have been used as indicators for phenological mapping in the Eastern Deciduous Forest Biome and in broader regional areas. Only species having very conspicuous phenophases with distinct events are suitable for spectral analyses and regional phenology projects. Ten species were selected for detailed observations for the construction of phenological spectra and to relate to current productivity and process studies. Those selected represent the first woody species to initiate growth, the red maple; the dominant canopy species of slope forests, the yellow poplar; and certain shrubs and herbaceous species in the areas under intensive study. Particularly noticeable is the almost simultaneous development of the reproductive phase of shrub species most characteristic of local forest ecosystems: dogwood, redbud, and serviceberry. This series of phenological events precedes the canopy development of the yellow poplar. Four of the species under study are observed at other sites also within the Biome to form the basis of regional or statewide phenology programs. Variation in seasonal productivity within species and between similar ecosystems can be illustrated through comparative analyses of successive or concurrent phenological spectra and quantitative measures of biomass or increment of growth.

Phenodynamics of Production

Phenological studies, where complemented with quantitative measurements, provide insight into the interactions between individual species of communities and their environment. The importance of various phenological events to the total productivity of single species suggests that each component within a community could be analyzed in a similar manner. Such studies of growth analysis would permit the construction of more precise models of production for specific ecosystems.

In the mesic tulip-poplar-dominated slope forest at Oak Ridge, estimates of the biomass and productivity of the herbaceous layer have been inferred from limited data. These estimates were obtained during one growing season and provided no relationship to phenological development and environmental interactions. By comparison, the herbaceous layer in an oak-hornbeam forest in Europe was evaluated phenodynamically during two growing seasons, so that variations in monthly production were directly related to climatic conditions. Such analyses provide explanations for variations in total productivity within communities from one season to the next.

Growth analysis data are presented for a single species, mayapple, at the Oak Ridge site, to exemplify the role of phenology in productivity studies. The summation of similar analyses for major or key components within communities would provide more precise estimates of the total productivity of the herbaceous layer. Mayapple is used as an example because of its social occurrence and rapid phenological development in the spring. As one of the most conspicuous herbaceous species to initiate spring growth in the eastern deciduous forest, mayapple offers potential as an indicator species for regional studies. During their first three years the nonreproductive individuals display a vertical shoot growth with developmental forms distinctly different from the older, reproductive plants. Mature plants annually produce a single leaf from underground, horizontal shoots. Plants within mature mayapple clones or populations exhibit an almost simultaneous response in phenological development. The time from leaf emergence through the litter to date of first flowering spans approximately three weeks. Distinct populations or clones of mayapple occur frequently within slope or mesic northerly exposed forests. In the yellow poplar forest study area, mayapple is best established in social units on low, moist sites. Being partially clonal with moderate longevity, the perennial populations vary in area, appearing elliptical to circular in shape. Clone size among 21 populations averaged 21 square meters and ranged from 0.7 to 176 square meters. Plant count per community varied from 10 to 683, with an average of 167 plants per population.

Regression analysis of computed area to number of plants permits an estimate of population density, providing the size of the clone is known. Within the broader definition of the oak-hickory association, density per square meter is less than 1, whereas within the average clone or population it is closer to 8. Excavation of the rhizome system of a clone revealed that the average age of viable underground shoots was six years, with obvious decay of older segments, and that the population had increased in radial growth at the rate of a decimeter per year. This particular population had therefore occupied the site for approximately 45 years. Phenodynamic studies of specific populations spanning many years could provide data depicting the impact of seasonal or annual climatic variations on production.

Beginning with leaf emergence, on year day 99, plants were harvested at each of eight life phases. Plants were compartmentalized into leaf, stem, flower or fruit, and rhizome-root for biomass estimates. Estimates of rhizome-root biomass during the leaf emergence phase (year days 99 to 102) were considered standing crop, so that subsequent estimates would reflect changes throughout the life cycle. Biomass increased for each component with the progression of phenological development, reaching maximum dry weights during the phase of unripe seeds and fruits, prior to the onset of senescence at year day 162. At that time rhizome-root (1970 increment) showed an increase of 21% in dry weight above the standing crop estimate at leaf emergence. Leaf and petiole (aboveground stem) accounted for 57%, whereas unripe fruits and seeds represented 17% of total dry weight.

The underground rhizome system grows terminally, producing an overwintering apical bud. The 1971 underground shoot was initiated during the reproductive phase, from year day 116 to 123, and showed an increase of 4% in dry weight prior to senescence of aerial parts. At the onset of senescence at year day 162, leaves began to change color. This phase was followed by leaf and fruit drop and subsequent decreases in dry weight. Seeds are distributed by insects and small mammals feeding on ripe fruits. Further increases in the increment to the 1971 underground shoot system were accompanied by decreases in dry weight of the 1970 season's growth, indicating that the rhizome functions as a storage organ for subsequent growth processes. By the end of the growth period, on year day 240, each increment in rhizome-root, both 1970 and 1971, accounted for 25% of the biomass, with the other 50% consisting of fallen leaf, fruit, and seeds. Biomass was summarized per unit area within the oak-hickory forest, where plant density is less than 1 per square meter, and on the basis of the average mayapple population in the *Liriodendron* forest study site, with its density of 8 per square meter.

Productivity estimates per unit forest area or average population within the intensive study area reflect growth above the standing crop of rhizome-root for 1970 at leaf emergence. Productivity is maximum during the flowering phase, year days 103 to 116, decreasing rapidly through fruit development and leaf coloration phases. The decrease in production between year days 163 and 181 represents a loss due to the consumption of ripe fruits by animals and insects. As indicated in biomass data, unripe fruits and seeds account for as much as 17% of the total dry weight. The phenological spectrum above the productivity curve illustrates the relationship of various events to changes in productivity.

The phenological development of the understory herbaceous flora coincides with the development of dominant canopy species in the deciduous forest. Many such events can be correlated with changes in functional processes in the ecosystem: for example, photosynthesis and respiration. Arrival of host-specific phytophagous insects and secondary consumers might well be predictable for certain species. Availability of food base among some species often coincides with the arrival of specific predators, especially migrant birds.

A study of the phenodynamics of production at different locations reveals the interactions of meteorology with growth processes. Similarities in phenological development between sites provide bench marks for productivity profiles across regional boundaries. Associated with the phenological development of species or communities are changes in biomass and rates of productivity. And these short-term phenological events, in turn, represent inputs into the litter layer, subsequently affecting the mineral cycle of the ecosystems. Phenological considerations in productivity studies can be of value in making predictive models of the system's functions, thus providing some measure of the impact of man or man-related activities on natural ecosystems.

AMW COMMENTS

Research managers can be divided into two groups: strategists and institutionalists. Strategists plan a detailed scientific or engineering strategy — complete with PERT charts, milestones, landmarks — and then get people to carry out the master plan. Institutionalists state broad objectives and then create the instrumentalities — individuals, groups, even whole laboratories — that work out their own detailed strategies for reaching these objectives. No matter the scale on which the research manager works — a group leader in a laboratory, a laboratory manager, or a program manager of a very large national scientific program — one can generally discern which of these basically orthogonal ways of looking at research management dominates.

From sitting on a variety of committees concerned with national research planning in many agencies, I would say the strategists are gaining dominance at the Washington level. As a laboratory director, I instinctively am an institutionalist, if for no other reason than that I, no more than anyone else, am uncomfortable at being told in detail what to do and how to do it; I therefore view this trend with some concern.

It seems obvious that the choice of the appropriate strategy depends upon one's a priori *assessment of the state of the art. Here I would distinguish between states of feasibility and pre-feasibility. The development of a high-powered nuclear reactor was feasible in principle once Fermi had shown that he could sustain the first chain reaction — or even sooner when, in early 1942, he first constructed a subcritical reactor with* $k > 1$*. By contrast, the development of a thermonuclear reactor must be judged to be in the pre-feasibility state, at least until the Lawson criterion is exceeded (for a D-T reaction the product of ion density times containment time is greater than* 10^{14}*).*

All would agree that where basic feasibility has been assured, a centralized, heavily programmed approach controlled outside the institutions doing the work can be effective. Note that I say can be *effective; the Manhattan Project was to my mind a striking counter-example. To be sure, General Groves was a tough guy who insisted on getting things done expeditiously. But he delegated the entire research, development, and engineering strategies to the people who were actually doing the work. Los Alamos had complete control of how to go about making an atomic bomb; the Metallurgical Laboratory at Chicago and Clinton Laboratories at Oak*

Ridge developed the underlying technology of plutonium production; and Du Pont built the Hanford reactors in what, in retrospect, was record time. Nevertheless we must concede that, 30 years later, things are different. The blank checks of Manhattan Project days are unheard of. What we do now is under very much more public scrutiny, and much as some of us who at the time used to gripe about General Groves would like to resurrect him, his methods are no longer a real option. Much of our big technological development — the moon shot, for example — has been quite properly in the hands of the strategists.

On the other hand, where the art is still in the state of pre-feasibility, all my instincts tell me that the strategists are wrong and the institutionalists are right. And indeed, in the controlled thermonuclear research (CTR) program, which most would judge still to be in a state of pre-feasibility, the four individual CTR laboratories enjoy a considerable degree of autonomy to work out their own approaches.

The tension between strategists and institutionalists can never be avoided, simply because there is never enough money to go around. Some central control is absolutely necessary, because an institution will never voluntarily commit suicide by giving up a pet project of its own in favor of a competitor's pet. Yet I think there is a big difference between allocation of resources — a prerogative that can never be sacrificed by the central manager — and detailed adherence to a centrally conceived master plan, in those cases where feasibility has yet to be established.

I use as examples of the two different philosophies the moon shot and CTR. But from where I sit it seems to me that this doctrinal conflict is emerging in many parts of the national research scene that have traditionally been run by institutionalists. For example, the National Cancer Attack Act of 1971 in Section 407 requires that the Director of the National Cancer Institute, "with the advice of the National Cancer Advisory Council, plan and develop an expanded, intensified, and coordinated cancer research program encompassing the programs of the National Cancer Institute, related programs of the other research institutes, and other Federal and non-Federal programs." Section 410A further requires the NCI Director to "prepare . . . a plan for the program during the next five years." There is considerable talk in Washington about mounting a centrally directed, massive attack on cardiovascular disease. Even in the National Science Foundation, which traditionally has been a strong haven of the institutionalist philosophy, we see some evidence of the strategists moving in, at least in the more applied parts of NSF, such as the RANN Program.

I may be misreading the tea leaves, but I think not. These seem to be the days of the strategists. And, in a general way, this is probably a reflection of our society's concern for relevance, for science that demonstrably helps the society. The strategists put a man on the moon; shouldn't they be able to cure cancer or heart disease or create nice, stable thermonuclear plasmas that exceed the Lawson criterion?

Thus I can understand some of the frustration that has led to the strategists' ascendancy. And indeed, in no case should the two doctrines be applied exclusively. Obviously a balance is needed. To the institutionalists I would offer a piece of advice that runs like this: institutions always must earn the right to autonomy. This right they earn by two commitments — a commitment to excellence, and a commitment to the overall objectives of those who give them money. They must always be prepared, even eager, to have these commitments reviewed, and if deficiencies are found in either of them, to make the changes necessary to correct the deficiencies. To the strategists I have only this to say (and here I confess to sounding like an old-timer who likes to give lectures): Remember that General Groves had much institutionalist instinct. Tough he was, but once he had confidence in an institution and its people, his support was unstinting. I hope present-day strategists, whether in hardware projects or in medical research, always remember this trait of the successful General.

Alvin M. Weinberg

The Resident Polyglot

odd problems met in the course of operating an office of language services

By FRANCOIS KERTESZ

BESIDES THE WELL-RECOGNIZED NEED for translations of technical articles from other countries, there are myriad foreign-language problems encountered in an organization the size of Oak Ridge National Laboratory. A number of these carry an urgent need for response; the individual cases are usually too small to warrant recourse to an outside agency. Let's take a look at some of the situations:

Foreign-language correspondence. As a result of greatly increased interchange among scientists, the incoming mail contains many letters in French, German, Russian, etc. While much of it consists of routine requests for reprints, invitations to submit papers, announcements of meetings, etc., many of the letters include more personal messages. It seems that a new etiquette has evolved in international contacts, allowing each party to write in his or her own language. This is logical because it is easier to find at either end a person who can translate a foreign-language letter than one who can write in an unaccustomed language. These letters usually require immediate attention.

The handwritten signature causes difficulties; in most European countries it is not customary to type out the correspondent's name; the recipient does not know who wrote the letter and wants help in deciphering the illegible signature. (The French abbreviation M. for *Monsieur* is occasionally mistaken for the first initial of the writer.) It is also hard to establish the marital status of female correspondents — and no equivalent of "Ms" is yet in widespread use abroad.

Foreign-language letters, not addressed to a specific individual. Many people do not realize how large ORNL is, with offices located over a wide geographic area, often not in touch with each other. Letters arrive from abroad, addressed simply to the Laboratory, without mentioning the individual or even the division that they want to reach. Such letters, not addressed to anybody in particular, written in a language that nobody can understand, are routed to the Laboratory Records Department, whence they are forwarded to the Office of Language Services. Translation of the

Francois Kertesz, who has contributed previously to the *Review*, leaves ORNL this year for Geneva, where he will be European representative for Informatics, Inc. Kertesz, whose degree in electrochemistry came from the Sorbonne, joined ORNL's technical staff two decades ago and was soon called into the services that eventually established him as the Laboratory's resident polyglot. He was born in Nagyvarad, Hungary, a town that was also known by its Austrian name as Grosswardein and that is today Oradea, Romania. He is fluent in six languages and can read and write several more, and at a laboratory like ORNL, engaged as it is in an ongoing dialogue with the entire scientific world, his interpretative ability has proven invaluable. The following article is an exerpt from a longer piece he has written for the *Journal of the American Society for Information Science.* It itemizes some of the nontechnical functions that can fall to an office of language services in a large research institution.

letter alone is not sufficient; we must be aware also of the Laboratory's current activities in order to direct the mail to the proper person. The letter might be a bill from a French publisher requesting an overdue subscription fee for journals, in which case it will be sent to the Library; it might be an angry note from a German hotel manager, announcing that Dr. Jones, for whom a room was reserved, did not arrive when expected and now the hotel, having turned away customers, wants to be paid for the room; this letter will be directed to the Travel Office.

Occasionally, a letter actually intended for the Technical Information Center of the AEC or the Medical Division (research hospital) of the Oak Ridge Associated Universities is misdirected to ORNL. Our office must decode the true recipient from the general context.

"Crackpot" letters. The fame of the Laboratory has spread beyond the nation's boundaries, and would-be inventors, people who have a new theory to explain the mysteries of the universe, and prophets of doom who hold scientists responsible for all miseries, sit down at their typewriters — or worse, pick up their pens — to outline their systems or to record their dissatisfactions. Usually, the careless appearance and lack of letterhead betrays this type of correspondence, but prejudice based on appearance is a danger; in many parts of the world, people do not attach as much importance to neat typing and correct format as in this country. It is not easy to separate the misunderstood genius from the impractical dreamer, especially on the basis of a single letter. Martin Gardner's book, "In the Name of Science," gives some good hints on how to distinguish oddballs, far removed from the mainstream of science, from original and unorthodox thinkers. Even if the letter cannot be forwarded to anybody, it should be answered, preferably in the same language.

Letters from the general public. In addition to maintaining contact with newsmen and feature writers, the Public Relations Office is also the chief spokesman of the Laboratory to the general public. Members of this formerly inarticulate public are increasingly sensitized toward environmental and public health considerations evolving from the widespread use of nuclear energy for power generation. The Laboratory, a well-known representative and effective advocate, is therefore at the receiving end of advice and expression of opinion in many foreign tongues from abroad.

Sometimes a question unexpectedly attracts the attention of the public; then, without any warning, the dam breaks and the letters start pouring in. This type of correspondence usually includes a fair share written in foreign languages, often in hasty, hard-to-decipher handwriting. This occurred a few months ago when wire service news dispatches focused worldwide attention on some aspects of local cancer research. After the story broke, the Laboratory was besieged not only by a flood of correspondence and telephone calls from this country, but also by letters, aerograms, and cables from Argentina, France, Germany, Italy, and other parts of the world. Communications arrived from both laymen and scientists.

All the letters deserved an understanding answer, and the availability of linguists on the staff was a boon to those who had to cope with the problem.

Technical interpretation. The local linguist-scientist is occasionally called to serve as an interpreter. In spite of the prevalence of English in scientific communication, the foreign researcher is acquainted primarily with the written record; on his first trip to this country he is often overwhelmed by rapid oral discussion. Sometimes he understands everything that is said but is unable to express himself with ease. This usually becomes apparent only after the first few hours of the visit, because as a general rule, visitors overestimate their own linguistic ability. In response to frantic calls for help, the Laboratory's Office of Language Services usually manages to send somebody to the rescue. Although none of us has any formal training in interpreting, we found that earnestness and some knowledge of language were very helpful to change the visit of a group of foreign experts from a complete disaster and waste of time for everybody into a mutually beneficial exchange of information.

"VIP" visitors. The Laboratory has been host to ex-kings and presidents, senators, ambassadors, and other public figures from many lands. Visiting VIP's might believe that they do not need to bring an interpreter because they speak English, but after a concentrated briefing on a variety of subjects feel the need for additional explanations in their own tongue. Often a senior linguist-scientist may take charge of the whole event, and on the basis of his overall knowledge of the Laboratory's facilities, he may act as the host and guide as well as interpreter. Some of the more than one hundred foreign scientists on temporary duty at ORNL have been very helpful on such occasions.

Prerecorded briefings. In order to provide further assistance to such special visitors, a general review of the Laboratory's overall program was tape-recorded in German and synchronized with pertinent color slides. The results were very satisfactory; although the visitors understood English, listening to the narration in their own language softened the cultural shock. In view of this successful experience, it is planned to prepare such slide shows in other languages also. The operation of large facilities, such as reactors and accelerators, can be explained much better if the visitor knows what to expect because he was prepared by a slide show in his own language. Although not done yet, consideration is being given to following the example of radio-guided tours at certain museums, such as the National Gallery in Washington, to describe large pieces of scientific equipment in a variety of languages on the spot, not only with a slide show, but also by using portable cassette recorders or short-range broadcasting.

Reverse translation of abstracts. The increased participation of the Laboratory scientists in international meetings and symposia increased the demand for the translation of abstracts of papers into one or several of the official languages specified by the organizers, usually French, Russian, Spanish, or German. This requires a considerable working familiarity with the current literature of the subject in the language in question, and therefore, the few hundred words of an abstract or summary without the context of the whole article usually present more problems than translation of long articles into English. This is another occasion when we are able to take advantage of the expertise of our foreign research participants who are willing to help with the terminology in their mother tongue.

Assistance to foreign research participants. Nobody is immune to language problems, and our guest scientists themselves occasionally feel the need for linguistic assistance. Before they are formally admitted to work at the Laboratory and in some cases afterwards, they must satisfy a variety of legal requirements which may involve the submission of a birth certificate, high school and college diplomas, employment records, etc., to the proper authorities. These documents must be properly translated and certified, and often present their own nomenclature problems. Although these problems are well known by translation agencies in large cities, where translation of personal documents is a bread-and-butter business, at the Laboratory the translator is often hard put to find an American equivalent for certain terms. An example is the French *baccalaureat*, which is not a bachelor's degree but a highly prized diploma given to successful candidates at the end of high school studies, enabling them to enter the university. On one occasion, we had to translate the whole text of a Dutch diploma, written in Latin. The U.S. official did not accept the "foreign-language" original document, although the diplomas of many American employees are written in Latin, and I am sure that few personnel managers care to know the exact meaning of the lengthy text on the sheepskin that begins *Lectoris salute ....*

Assistance to staff members assigned to foreign countries. The Laboratory not only hosts foreign research participants but also sends many of its members abroad for periods of years. They work in foreign lands as exchange scientists, holders of Fulbright or Guggenheim fellowships, as project leaders within the framework of the "sister laboratory" program, etc.

Employees assigned to other countries have occasioned many questions. For example, our Legal Department needs to understand fully all contracts the employee assigned to work in a foreign laboratory is asked to sign, in order to be aware of legal commitments. As a result of this, the Office of Language Services has had to peruse and translate many pages of a variety of legal documents, such as Swiss lease agreements, specifying the penalty for failure to remove snow from the sidewalk in front of the house; German regulations concerning work permits; etc. By now the appropriate local people are aware of the contents of these documents, and the volume of this type of work seems to be decreasing.

Telephone contact with foreign countries. In view of the large number of Laboratory employees who travel through Europe and the rest of the world, it is unavoidable that emergency business or family situations make it necessary to get hold of them on short notice. It is relatively easy to contact a person in a large hotel in Paris, but English is definitely not the *lingua franca* when trying to locate the teen-age son of a local scientist at a campsite on an island off the Atlantic coast of France, or to transmit a message to an Oak Ridger who attends a seminar held in a chateau at the outskirts of a small town in Normandy, or to find the conference chairman in Italy to ask for an extension of the deadline for the submission of a paper. (In the last case, the chairman's secretary advised us that her boss had departed for the weekend but probably could be reached at the home of his wife's parents!)

The Laboratory's linguists have completed successfully many such emergency contacts; in all of these cases, the situation had to be explained to impatient small-town switchboard operators in various European countries in their own language in order to enlist their assistance. The American overseas operators are helpful and courteous, but usually they speak only English. The French or Italian operators usually understand enough English to connect the American party with the desired number, but complicated arrangements are beyond their ability; one must talk in the language of the country and, most of all, be patient.

Sometimes our colleagues ask us to use our language experience to help them in the pursuit of their hobby or avocation. An ORNL chemist who is a pigeon fancier asked me to get in touch by telephone with a French pharmacist, a resident of a small town in northern France, and one who had the reputation of being a crusty curmudgeon, for the purpose of convincing him to send some of his famous champion birds to this country. The old gentleman, assuming that I was the pigeon fancier, talked in great detail about his hobby, which sounded like a more lucrative pastime than his profession, inasmuch as he was selling the eggs of his champions for several hundred dollars.

Language instruction. Language teaching is another occasional activity performed by multilingual staff members. At the time of each of the first two Geneva Conferences on the Peaceful Uses of Atomic Energy, many ORNL scientists were sent abroad — some of them for the first time. In order to assist them, crash courses in conversational French were organized for their benefit. When interest in Russia was at its peak, year-long Russian language courses were offered at the Laboratory to about a hundred participants.

Technical exhibits abroad. Because of its reputation, the Laboratory has played an important role in many scientific exhibits held in foreign countries. Thus, in connection with the first reactor ever shown to the general public, in 1955 (at the First Geneva Conference), and the large Fusion Exhibit at the Second Geneva Conference in 1958, linguists on the staff of the Laboratory were busy for many months checking the foreign-language signs, proofreading information brochures, and making themselves generally useful during the preparatory phase. Later, at the site in Geneva, they were training the guide-interpreters from the famous Interpreters' School of the University of Geneva in the rudiments of nuclear science. A staff member was also assigned to act as the AEC representative at the American Pavilion devoted to atomic energy questions at the 1957 Paris Trade Fair and as technical information director at the "Atoms in Action" exhibit in Bucharest in 1969. In both cases, knowledge of the local language was an important factor in the selection.

Full-time interpretation. In addition to helping with the information transfer between foreign visitors and their hosts at the Laboratory for a few hours, the combined technical knowledge and needed language skill of at least one staff member was recognized and used also outside of local needs. The services of Joe Lewin, nuclear engineer with the Neutron Physics Division and a native speaker of Russian, have been requested repeatedly by the AEC; he has acted as guide-interpreter of Russian scientific teams visiting the United States and delegations of American scientists in the Soviet Union while on leave from his regular job. His fluency in both languages, combined with his technical proficiency, give him considerable value in such situations. Most recently, he was interpreter for Glenn Seaborg when the latter visited the Soviet Union in the company of a few associates, on his last official trip, in August 1971, as chairman of the AEC.

THE AMERICAN SCIENTIST cannot dismiss the language problem; simplistic solutions, placing the burden on the shoulders of the other party in the communication process, are not valid anymore. For more than a decade after the war, many American technical men felt that a paper not written in English was not worth his time; the advent of Sputnik, Volkswagen, and Sony, with their implications for tremendous technical achievement in other countries, has resulted in major changes in the ranking of the leading industrial countries. As a result of this, the general outlook of English-speaking scientists is affected, and their belief in their supremacy must be reexamined.

Recent data reveal that although English is the leading language in printed scientific communications, Russian is firmly in second place in a number of fields: chemistry, physics, geology, mathematics, and biological sciences. Thus, a language long ignored by American scientists is now a major means of communication in science. The "language gap," or the Tower of Babel syndrome in international scientific communication, has been well recognized and deplored.

Several approaches used to solve language problems at ORNL have proven valuable. Computer-aided human translation and the fully automatic method represent interesting and not yet widely used approaches which are expected to become increasingly important.

Publication of cover-to-cover translations of foreign (mostly Russian) journals has probably passed its peak; the high cost and long delay in publication reduce the usefulness of these journals.

The National Translations Center, which translates individual articles, represents an important but only partially utilized national resource. It deserves to be better supported by all interested parties — the federal government, the library and information-handling community, the scientific societies, and the translation fraternity.

My experience at ORNL has convinced me of the need for a translation service at every large technical organization; such a service may be administratively attached to the existing library or the information-processing department. The efforts of the staff translator(s) can be best utilized by providing additional support through a contract with an established translation agency. With a translator on the premises, the geographical location of the agency is not an important consideration, but if the agency is to take on the bulk of the foreign-language problems I've mentioned, a nearby organization should be chosen. It is also desirable to establish contractual working arrangements with a single agency, because a valued, old customer can expect preferential treatment in case of urgent jobs.

A special effort should be made to exploit the often hidden supply of linguistic resources of the regular technical staff, taking advantage of in-house talent to handle the many specialized language problems which crop up unexpectedly in a large organization.

However, in spite of the advantages in using the supplementary skills of the technical personnel, there are also negative factors. Most multilingual technical men enjoy helping out their colleagues, but they consider themselves primarily as researchers and engineers and do not want to be burdened with routine translation assignments. They value recognition by their peers, but unfortunately, recognition is not yet readily given for the linguistic ability of technical men. This situation should change: in particular, the present anonymity of the technical translator should be eliminated. The literary translators succeeded in gaining recognition; the scientific societies and publishers would be well advised to accord official recognition to translations of excellence.

The reaction of management, and especially of the immediate supervisor, to a staff member's ability to render special services has considerable effect on the eagerness of the linguistically adept technical man to come forward to lend a helping hand. Our experience indicates that even when management formally authorizes a scientist to spend a certain number of hours a week in such activity, the scientist himself may be reluctant to accept the assignment, not only because of the low prestige attached to it, but also because it implies indirectly that the individual's regular work is so unimportant that it can be laid aside. This consideration is of primordial interest in a time of budget restrictions: no scientist or engineer wants to be found by his supervisor poring over a long foreign-language article, not related to his own work, that he is translating for somebody else. Regardless of higher management approval, it still appears that he is not sufficiently busy with whatever he is supposed to do. The solution may be to upgrade the self-esteem of translators by recognizing their contributions to the success of technical projects.

In this era of rapidly changing scientific, economic, and political relations, the American technical man must also revise his preferences; he can no longer afford the "splendid isolation" of his own language. The chauvinistic belief that everything worth reading has been published here or somewhere else in English, or has been "stolen" from us, has been deflated long ago. Twenty years ago, when he was chairman of the AEC, the late Gordon Dean stated:

> *Nature herself remains a "security risk" that cannot be controlled. The secret that has been learned by one may be learned by another. It takes brains, knowledge, experience, skill, and resources, but these are not the exclusive possession of any one nation or any one group of nations.*
>
> "Report on the Atom," p. 242
> Alfred A. Knopf, New York (1953)

Take A Number.....

BY V. R. R. UPPULURI

OPTIMAL STOPPING

The following problem, known by different names as "The Secretary Problem," "The Marriage Problem," "The House Hunting Problem," "Googol," and others, has attracted the attention of some mathematical statisticians. Suppose a number n of applicants interview for the position of a secretary. If we could observe them all, we could rank them absolutely from the best (rank 1) to the worst (rank n). Suppose they appear for an interview one by one in a random order, and the decision whether each is hired or not has to be made immediately after the interview. After i number of interviews, we can observe the ith applicant's rank relative to the $i-1$ hopefuls already interviewed. The problem is to find an optimal procedure that will assure us the choice of a good secretary in the set of n applicants. In other words, if we hire the first person who takes the interview, we will never know whether we picked the best or not; similarly, if we do not hire the first, we might have rejected the best, since we cannot recall any of the applicants. Is there an objective approach for this problem?

One may set up a criterion, and ask for the best possible strategy. A possible criterion is to maximize the probability of hiring the best candidate. In order to accomplish this, the optimal strategy turns out to be the following: Interview a certain fraction $1/e$ of the candidates (without making an offer) and rank them. Hire the next one if the next one is better than all the previous candidates; otherwise keep interviewing till you find the applicant who surpasses the preceding ones. Remember, you have to hire the last one left, if you have not decided before that time. For large numbers of candidates, Columbia University's Robbins et al. have arrived at $1/e \cong 0.36787$. For a moderate number of candidates, like $n = 8$, or 9, or 10, it turns out that you interview the first four without hiring, and assign ranks to them. Then select the next applicant who ranks fifth. In adopting this procedure, you are assured that the probability of hiring the best is maximized. This is based on the assumption that the applicants appear for the interview in a random order. In the Marriage Problem, this procedure is analogous to passing up the first few prospects (call it experience, if you wish) and then accepting the very next one if it is better than all the previous ones.

The approach can draw upon other criteria. One that is also studied by mathematical statisticians is that of minimizing the expected absolute rank of the applicant who is ultimately hired.

ARE PRODUCTS SQUARES?

It can be easily seen that the product of any two consecutive natural numbers can never be a perfect square. Similarly, it can be proved that the product of any three consecutive natural numbers cannot be a perfect square. At the turn of the century a lot of research was done to show that the product of any k consecutive natural numbers is not a perfect square, for specific values of k. Is this true in general?

Bill Thompson came to Oak Ridge directly from the University of North Carolina graduating class of 1943, arriving on January 2, 1944, to work with Tennessee Eastman at Y-12. When he got here, he found that the dormitory to which he had been assigned was still under construction, and so he had been reassigned to the newly built barracks that were intended for the Army personnel expected to be posted here soon. A little over a year later, drafted into the Army, he was back living in the barracks again, a bona fide tenant. Bill began work at ORNL in 1947 in the field of isotopes production, operating in direct connection with the "Oak Ridge Pile" whose origins he recounts herewith. In 1949 he joined the Homogeneous Reactor Project at its inception, later working in the budget office and, more recently, in the Nuclear Desalination Program. He is at present with the Directorate of Licensing in AEC-Washington's Regulatory Branch. He is shown here reminiscing at the National Landmark where, thirty years ago, it all began.

Clinton Laboratories—the WAR years

heavy recall 30 years later

By W. E. Thompson

THIRTY YEARS AGO, on Feburary 1, 1943, construction work was started at the X-10 site. Looking back to those days, we can only feel amazement at the boldness with which the wartime atomic energy projects were planned and at the speed and success with which they were carried out. In 1942, even before the first nuclear chain reaction had been achieved, the Corps of Engineers purchased 92 square miles of land in the area between Clinton, Oliver Springs, and Kingston, under the guise (for security reasons) of establishing the Kingston Demolition Range. This area was intended for the large-scale production of enriched uranium-235 and plutonium needed for atomic bombs and was given the name Clinton Engineer Works. (The name Oak Ridge was not adopted until June 1943.) The army had originally planned to carry out all atomic bomb project activities at this site. Later, it was decided to locate the plutonium production facilities at a more remote site on the Columbia River near Hanford, Washington.

To see how fast things moved in those early days, let's look at a chronology of some significant events. On December 2, 1942, Enrico Fermi and his colleagues on the Metallurgical Laboratory staff achieved the first nuclear chain reaction in the Chicago Pile, and two weeks later, on December 16, the nation's Military Policy Committee recommended to President Roosevelt that a plutonium production pilot plant be constructed at Clinton Engineer Works. The President approved. On January 15, 1943, E. I. du Pont de Nemours and Company of Wilmington, Delaware, was selected to design and construct the pilot plant facilities in Tennessee, to be designated as Clinton Laboratories. By February 1, 1943, Du Pont was breaking ground for the "Clinton Pile" (now the Graphite Reactor) at a rural site which was more than five miles from the nearest electric power line. In less than eight months the pile was complete and the University of Chicago physicists had begun preoperational testing. The plutonium separation pilot plant was nearly complete, and testing of the equipment was started in that facility, also. Frank Bruce, who at that time worked in the analytical lab in the pilot plant building, remembers being impressed by the smoothness and the buisnesslike manner with which the Du Pont people carried out the preoperational tests and check-out procedures. This first large-scale radiochemical processing facility had five feet of concrete shielding around all the processing equipment, making it necessary to employ remote controls and to operate the system without being able to see it; but they made it work, right from the start.

The pile started up in November 1943, reaching criticality at 5:00 AM on November 4, almost exactly nine months after ground was broken. Arthur Holly Compton, director of the Met Lab, was spending considerable time in Oak Ridge in those days. Ernie Wollan recalls coming down by train from the Met Lab for the pile startup with a group that included Enrico Fermi and Norman Hilberry. Wollan looked for neutron leakage through the top of the shield and found the shielding to be adequate, as predicted. He also checked for neutrino emissions, which were expected to pass easily through the shield, if they were present. He established that the highest possible number of neutrino emissions anticipated could not present a radiation hazard.

After the pile had been operating a few weeks, the first "hot" runs to separate plutonium in the pilot plant were started on December 19, 1943. Among those operating the pilot plant at that time were John Gillette, Harris Blauer, Roscoe Pressley, Stanley Rimshaw, Harvey Mahlman, D. C. King, and Claude Keck.

The early research and development on fission, on uranium isotopes separation, on plutonium production, and on related matters had been performed mainly at Columbia University, the University of Chicago, the University of California, and Iowa State College under the Office of Scientific Research and Development, directed by Vannevar Bush. In August 1942, the Manhattan District was organized under the Corps of Engineers to carry out the large-scale construction and production activities of the atomic bomb project. In September 1942, Brigadier General Leslie R. Groves was placed in complete charge of the Manhattan District. It was he who selected the Du Pont Company to design and construct the Clinton Laboratories plutonium pilot plant and the production facilities at Hanford.

Stone and Webster Engineering Corporation had originally been selected as the overall engineering and construction firm for the Manhattan District, but it soon became apparent that the various parts of the work were too widely separated physically and too complicated technically to be handled by a single company. Stone and Webster built the Y-12 Plant and some of the townsite facilities, Du Pont built the X-10 facilities, and J. A. Jones Construction Company the K-25 complex. Many subcontractors came in to build the houses, dormitories, and miscellaneous buildings of the town. Roane-Anderson Company, a special subsidiary of Turner Construction Company, was the rental and maintenance agent for the houses, dormitories, and commercial property on the Clinton Engineer Works townsite.

On that February day in 1943 when construction started, several farms still occupied the X-10 site. The Bethel Valley road was in existence — along its old route north of the present road — but it was not paved. Solway Bridge and the old wooden-planked Edgemoor Bridge were standing, but there was only a ferry at the White Wing (State Highway 95) crossing of the Clinch River. J. A. Jones Construction Company installed a pontoon bridge to replace the ferry in 1942, and this bridge continued in use, with some modifications and improvements, until the present bridge was built in 1963. Railroad spurs were built by the Louisville and Nashville Railroad to serve the townsite and the Y-12 Plant and by the Southern Railway to serve the K-25 Plant, but there was no railroad spur to the X-10 site. All materials for the Clinton Laboratories had to come in by truck. Before Oak Ridge's rail spurs were built, "Byington, Tenn." (a small community near Karns) was the railroad destination for many L&N shipments.

Hezz Stringfield was hired by Du Pont as an accountant for Clinton Labs on March 1, 1943. His first work location was the Scarboro School (where the UT-AEC Agricultural Research Project is now located), which was used for offices until some of the buildings at the X-10 site could be occupied. When the roof and exterior walls of the Administration Building were finished on May 1, 1943, office workers moved in, even though the interior partitions had not been installed. These were finished while the building occupants moved about to stay out of the workmen's way. The Scarboro

School continued in use, too.

Finding enough construction workers to build all the plants of the Clinton Engineer Works plus the town was a chronic problem in 1943. The construction fell behind schedule by several months because of the shortage of workers. Since part of the trouble was lack of living quarters close to the job, the Scarboro School was once again brought into use for a while as a barracks for workers. To recruit a labor force, John Fiser, at that time a Clinton Labs personnel officer, drove a bus through rural areas of Georgia, Alabama, Mississippi, and Tennessee, not only signing up people to work in Oak Ridge, but bringing them back with him in the bus as well.

The mud and dust of 1943 and 1944 are outstanding characteristics of Oak Ridge in everyone's memories; Larry Riordan recalls that in 1943 the Medical Department issued face masks to workers who experienced difficulties because of the dust. It was not until after the war was over and a more permanent role for Oak Ridge began to emerge that paving of the roads was started. Prior to that, Roane-Anderson leveled the town's streets with road graders from time to time, treating them with calcium chloride to allay the dust and with gravel to combat the mud. The roads on the Clinton Labs site itself were paved as a part of the program started in 1948 to make the place more permanent.

There was no air conditioning of buildings during the war years, not even with window units. There were lots of electric fans, but when the dust outside made it impossible to have the windows open, the summer heat and humidity could be pretty oppressive. Some laboratories and instrument rooms were air-conditioned to protect electronic components from the temperature fluctua-

STATUS OF CONSTRUCTION ON JUNE 27, 1943. The Chemistry Building on the right is nearing completion; the stack for the chemical pilot plant is also nearly completed, and the stack for the pile is just getting started. The graphite machining shop, on the hill behind the Chemistry Building, is complete and in operation. The large white building with stack on the left is the steam plant. The tents were used for storage and for construction shops until additional buildings could be completed. At one stage, the cafeteria was in a tent.

VIEW OF REACTOR BUILDING AND CHEMICAL PILOT PLANT ON AUGUST 31, 1943. The concrete shielding for the radioactive materials processing cells of the pilot plant can be seen to the left of the reactor building. The openings in the top indicate the locations and sizes of individual cells. The fan house for drawing cooling air through the reactor is at the base of the stack on the right. The graphite machine shop is in the foreground. The Physics Building is to the left of the water tank.

tions and the high humidity of East Tennessee, but it was not until the 1950's that AEC construction criteria permitted the air conditioning of office areas. Summer temperatures above 100°F in office and work areas were not rare. I can remember papers sticking to sweaty arms while working at my desk. We used lots of onionskin paper for making carbon copies, and it stuck to damp skin worst of all. Carbon copies of everything were the rule in those days, and often the number of carbons would be as high as possible. It took really strong typing fingers to make eight or ten carbons. When electric typewriters became generally available in the early 1950's, they could only be obtained to replace manual machines if there were special factors to justify the higher cost. The frequent need to make many carbon copies was the justification most people used to try to get an electric typewriter.

In 1942, the scientists building the first pile at Chicago had the benefit of the results of experiments with subcritical "exponential piles" which had been assembled at Columbia University and then reassembled later at the University of Chicago, after Fermi's group had moved there to join the Metallurgical Laboratory. They knew that a nuclear chain reaction was theoretically possible: this was considered to have been experimentally demonstrated in subcritical assemblies as early as June 1942. The big worry was how to get enough uranium for fuel and graphite for the moderator at high enough purity to allow the chain reaction to take place. Facilities for making pure uranium metal on a large scale were just coming into production, uranium having been an exotic material not much in demand until then. For the first pile under the West Stands at Stagg Field, the nuclear pioneers could only find six tons of uranium metal and had to use uranium oxide for most of the fuel loading. Methods of making graphite were known, but the nation's production facilities did not have very large capacity, and the purity of the graphite, while adequate for electrodes, was not always good enough for use in a pile. Boron, a strong absorber of neutrons, was a troublesome impurity in the very early graphite pieces, but purification methods improved, and by the time the Clinton Pile and the Hanford reactors were under construction, uranium metal and graphite production rates were up enough to avoid further problems.

The Du Pont Company arrived at the specifications of the Clinton Pile on the basis of recommendations made by the Metallurgical Laboratory staff, particularly by Eugene Wigner and Alvin Weinberg, of the Theoretical Physics Group. Part of the February 16, 1943, letter transmitting these recommendations gives data on three different pile designs: one based on minimizing graphite requirements, one minimizing uranium fuel requirements and emphasizing "safety," and one providing a core structure which would give even greater emphasis to "safety." An interesting point is that the term "safety," as used in those days, does not mean nuclear safety or freedom from fear of containment failure. It refers rather to the probability of achieving a chain reaction when the installation is complete.

The first of the production buildings to go into operation at Clinton Labs was the graphite machining shop, where craftsmen took the extruded graphite bars as they came from the manufacturers and machined them to final specifications for stacking in the pile. Ray Oakes, who worked in the graphite machine shop, recalls that the machinists were instructed to stop work and take a shower when they were sweating so profusely that there was danger of drops of sweat falling on the graphite. The problem was that contamination introduced by drops of sweat might also contain "poisons" that could interfere with the operation of the pile.

Finished graphite pieces went into exponential piles, from which physicists could then measure neutron diffusion lengths. Indium foils were calibrated for neutron flux measurements in the exponential piles, which also provided other pile physics data. The nation's total supply of large graphite bars arrived at Clinton Laboratories for the building of the pile. Graphite machining started up in May 1943 and was finished in July except for special pieces. Exponential pile experiments took up July and August; in September, crews began stacking the 676 tons of graphite in the pile by hand. This took three weeks.

The University of California had been studying the chemistry of plutonium on an ultramicro scale since early in 1941, using plutonium produced by cyclotron bombardment of uranium. The Met Lab in Chicago started the development of a chemical process to separate plutonium from uranium and from fission products early in 1942. Still working with only fractions of micrograms of plutonium, the California chemists developed the Bismuth Phosphate Process, which was successfully used at Clinton Labs and at Hanford. On August 18, 1942, the Met Lab prepared the first pure chemical compound of plutonium free of carrier material — it was the first time anybody had seen plutonium — and on September 10, 1942, the first weighing of a pure plutonium compound occurred: 2.77 micrograms of PuO_2. By February 1943, Du Pont engineers were designing the plutonium separation pilot plant, and in March, excavation for the building foundations was under way.

The work of the chemists and physicists was complicated by the lack of data on the fission products. Many fission products had not been identified, the yields of various fission products were not known, nor were the half-lives, the radiations emitted, the decay schemes, or the neutron absorption cross sections. The possibility that some of the fission products might absorb so many neutrons that the pile could not operate was a real worry in the early days. Two of the rare-earth elements, samarium and gadolinium, were known to be among the fission products and to have high neutron absorption characteristics. Lyle Borst and his associates irradiated samples of these elements in the Clinton Pile to determine their poisoning effects. They concluded that these two elements would not cause serious problems. However, soon after the Hanford startup, the worst fears of the chemists and physicists were realized: the Hanford pile was indeed poisoned, and it appeared for a while that full-scale production operation might not be possible. Needless to say, Clinton Laboratories and the Met Lab accorded this problem highest priority. Soon they identified the culprit as fission product xenon, which has a neutron absorption about a thousand times as large as anything previously known. It was then possible to work out a method to overide the poisoning effects of xenon and thus to permit operation of the Hanford piles in a satisfactory manner.

The design power level of the Clinton Pile, 1000 kW, had been chosen with the knowledge that it would produce about one gram of plutonium per day at that power level. Designers wanted the concentration of plutonium in the irradiated uranium fuel to be at least one part per million. Expecting the pile to contain about 60 tons of uranium (approximately 60 million grams), they figured that after a couple of months at 1000 kW the fuel would contain 1 ppm of plutonium. They planned to process the irradiated fuel in $^1/_3$-ton batches at the rate of one batch per day, so the nominal plutonium production capacity of the pilot plant at Clinton Laboratories was $^1/_3$ gram per day.

One of the most difficult problems was that of developing a satisfactory method of encasing the uranium metal fuel slugs in aluminum to prevent their oxidation in the pile. The first attempt at solution involved hot dip processes with zinc-aluminum alloys, but these gave thin coatings which were easily penetrated by scratches in normal handling. Electroplating gave better coatings but required too elaborate a process. Aluminum cans, sealed by resistance welding, were adopted; but trouble with leaky welds developed. Prior to the first loading of fuel into the pile there was in fact a frantic period of developing procedures to test the aluminum jackets for leaks and to perform leak tests on the inventory of finished fuel slugs which had been canned by Alcoa. Initially the test was to heat the slugs to 300°C for ten hours under a hydrogen pressure of two atmospheres, immerse the slugs in liquid, and look for bubbles of hydrogen. Ted Arehart was assigned to this work when he came to Clinton Labs as a Du Pont employee in July 1943. After some 70 tons of fuel had been tested, the investigators discovered that some of the slugs passing the test contained hydrides, indicating that sometimes the hydrogen reacted with the fuel and thus would not make bubbles even though the jacket was leaky. Dick Lyon, with C. M. Cooper at the Met Lab, designed test apparatus to apply 200 psi of nitrogen pressure to the welded end of the slug and measure the deflection of the other end of the can to detect any bulge caused by nitrogen pressure leaking in. This test rejected 45% of the slugs that had been received at Clinton Labs, but the ones that passed went into the first fuel loading of the pile. Better methods of canning in aluminum with silicon bonding ensued, and better sealing resulted from arc welding under an argon atmosphere. A new and simpler test was adopted: heating the slugs to 500°C for ten days and measuring the weight change as a means of detecting oxidation due to leakage. Testing of the first batch of 104,000 of Alcoa's new slugs was completed in February 1945. With the new canning method and the new testing procedure, rejects ran less than 4%, and neither the Clinton nor the Hanford piles experienced much difficulty with failures of the aluminum jackets.

Within a few days after startup, the pile attained a power level of 500 kW, which gave a maximum slug surface temperature of 100°C. In a short time, a power level of 800 kW was reached by plugging some of the unfueled outer channels to increase the cooling air flow through the fuel channels and allowing the maximum slug surface temperature to increase to 150°C. During and after pile startup the Physics and Technical Division groups, under Lothar Nordheim and Miles Leverett respectively, were studying ways of increasing the power level. During the early part of 1944 it was found that a few rather minor changes could result in a substantial increase in the operating level and, consequently, in the rate of production of plutonium. A new fuel loading arrangement shortened the length of fuel loading in the center channels, provided medium-length loading in the intermediate channels, and long loadings in the outer channels. With the amount of uranium near the center reduced relative to that farther out, a higher power output was possible without risking too high a fuel temperature. The slugs with improved arc-welded jackets could tolerate the 200°C temperature. As a result of these changes a power level of 1800 kW was reached in May 1944.

At the time the pile was under construction, the largest commercially available blower for circulating the cooling air through the pile had a capacity of 30,000 cubic feet per minute. Two of these were to be installed in parallel for operation at 1000 kW. However, a 50,000-cfm blower became available and was installed in September, so that the pile started up with one 30,000-cfm and one 50,000-cfm blower. In the preoperational tests, Art Rupp measured air flows through the pile to determine the cooling capacity, and Sam Beall measured air flows through and out of the pile stack to estimate the dispersion and dilution of radioactive atoms in the cooling air. After the pile had been in operation for several months, still larger fans became available and were installed in June and July 1944. Within a month, one of the large fans developed serious bearing problems, causing such vibrations that it had to be replaced. During the following months, additional bearing troubles caused some shutdowns of comparatively short duration before the problems were solved; but the higher air flows, coupled with the other improvements, permitted routine operation at power levels up to 4000 kW. This increase to four times the design power level gave a corresponding increase in plutonium production. Even at this power level there were no difficulties with the operation of the pile. In ease of control, steadiness of operation, and reliability of performance, the Clinton Pile achieved an impressive record. There were no failures attributable to mistakes in design or construction — a remarkable fact, considering that this plant was designed on the basis of the meager data available in 1942 and was constructed without previous experience.

The first batch of irradiated fuel slugs was taken from the reactor to the chemical processing dissolver on December 20, 1943, and the first plutonium was shipped from Clinton Laboratories on January 3, 1944: 1.54 milligrams sent to the Metallurgical Laboratory. Although the chemical processing proceeded in batches, it was continuous in the sense that a new batch was started as soon as the previous batch had been moved to the next step in processing. The final purification of plutonium was done in the Chemistry Building by I. Perlman's group, which included Ray Stoughton, John McBride, Ed Bohlmann, and Joe Halperin.

By the end of January 1944, ⅓ ton per day of irradiated fuel from the reactor was going through the pilot plant, although the low pile power level and short operating time had not allowed the plutonium concentration to build up to the planned levels. By March the production rate was up to about 8 to 10 grams per month of purified plutonium. Through May 1944, shipments had totaled 30.737 grams, and soon afterward higher production rates reflected the increase in pile power.

From December 1943 to January 1945, the pilot plant processed 299 batches of slugs: about 100 tons at ⅓ ton per batch. An objective had been set to produce about 300 grams of plutonium, and by the end of 1944, shipments had totaled 271.396 grams of plutonium. Accordingly, plans were made for closing down the chemical pilot plant early in 1945, by which time more than the originally planned 300 grams would have been produced. The final regular shipment in January 1945 brought the total of plutonium up to 289.438 grams, and additional plutonium reclaimed and purified in the process of closing down and cleaning out the pilot plant equipment was shipped in February 1945, bringing the grand total of plutonium production from the Clinton Laboratories pilot plant to 326.390 grams. Two years after the start of construction, the objectives had been accomplished far beyond the original expectations.

The project cost was $12 million for construction of all facilities at Clinton Laboratories, plus $12.5 million for all operations through June 1945.

BOOKS

By Arthur H. Snell

An Eye on the Future

The Collected Works of Leo Szilard: Scientific Papers. MIT Press (1972). 770 pp., $17.50.

LEO SZILARD (1898—1964) was not by nature an executive. He was more an individual of the lone intellect. Not for him the organization of teams, the wrestling with regulations, and the drudgery of detail. Hence he has never been as conspicuous in the history of the nuclear age as have been men like Compton, Groves, Lawrence, and Oppenheimer. Yet he was just as farsighted as any of them. Above all, he knew where to concentrate his effort for maximum effect. Of course that was true of Fermi also, and Fermi was a more prolific genius. Yet in making this comparison another factor comes in: Szilard, with all of his scientific penetration, was also a great humanist. He liked to work where the problems were, and when he thought that humanistic problems transcended those of the laboratory, he shifted his interest and his efforts into what he felt to be more important.

This memorial volume of his scientific works (the record of his humanistic work is to appear separately) has been assembled by Bernard Feld, Szilard's principal collaborator in nuclear research in the United States, and Gertrud Weiss Szilard, whom he married in 1951 when he was 53. It is arranged according to the chronological phases of Szilard's activities, each having a gracious introductory essay setting Szilard's contribution into the technical perspective of the time. Thus we have Carl Eckart explaining how Szilard's graduate papers, written in Berlin, resolved the paradox of Maxwell's demon and the second law of thermodynamics; Maurice Goldhaber summarizing the contributions to early nuclear physics in England between 1933 and 1938; Bernard Feld writing on the Plutonium Project days; Aaron Novick describing his years of collaboration with Szilard in microbiology in Chicago. Following each of these essays, the associated Szilard papers are reproduced *in extenso*, many in photoreproduction of the manuscripts with his handwritten emendations, and this has the effect of making his contact with the reader personal and alive. Finally, the patent papers are reproduced, and here we see that Szilard, the physicist, biologist, humanist, had also a strong touch of the engineer.

But let us discuss some parts of Szilard's career a little more closely. Consider the paper on Maxwell's demon, prepared in 1925 and published in 1928. The demon, it will be recalled, sat by a tiny aperture in a partition that separated two chambers in a gas-filled enclosure. By exercising intelligence, it allowed the faster molecules to pass in one direction only, and thus permitted one chamber to get warm and the other cold; in other words, it caused the entropy of the system to decrease without expenditure of energy, in violation of the second law of thermodynamics.

The concept thus stated may seem artificial, but Maxwell recognized it as a paradox. Szilard reformulated the demon in more modern terms: a device, consuming negligible energy, can be imagined that would pass only the peaks of thermal noise, and thereby cause one part of a system to heat up at the expense of another. He found the key to the paradox in the fact that the device would have to sense the thermal noise, and this process of measurement and its utilization led to an increase of entropy that compensates the decrease caused by selection of the fast molecules. He proved this for the general case. This was the first connection that appeared between entropy and information, and although it would be too much to claim that Szilard was the founder of information theory, he did indeed make this early and significant contribution. It is easier to recognize him as a young man whose graduate-school thesis resolved the paradox of the demon, and considering that the paradox had stood for over fifty years, one can justifiably applaud. Not bad for a young man; a keen kid.

In 1933 Szilard moved to England, and here his interest in nuclear physics matched the general excitement of the times. In particular, while walking in London he was contemplating a current remark by Rutherford that speculations about large-scale release of nuclear energy were "moonshine," and in waiting for a traffic light to change he was struck with the idea that the newly discovered neutrons might hold the answer rather than the charged particles that Rutherford had in mind. From then on the idea of the neutron chain reaction was in his mind. He started experiments with neutrons using radon-beryllium sources. This was the time of the Szilard-Chalmers effect, the discovery of neutron resonances independently from the work of the group in Italy, and work on the generation of photoneutrons from beryllium. Perhaps nuclear isomerism might be exploited in some way! For five years Szilard sought the chain reaction (sometimes with growing disillusionment), but the break finally came with the discovery of fission in late 1938.

(My own recollection was that Szilard was at one time excited by the *n*,2*n* reaction in beryllium as a chain-reaction mechanism, but that the published mass of beryllium was at that time wrong, and when the correct value was obtained, the potential energy source evaporated.)

After Munich, he had decided to stay in the United States, and the news in January 1939 about fission found Szilard in New York. He immediately started an experiment with Zinn to see if neutrons were emitted in fission, and how many, but he was of course not alone; Fermi, Anderson, and Hanstein started a similar experiment in New York, and Halban, Joliot, and Kowarski did likewise in Paris. All three experiments were completed rapidly (Szilard's letter to the *Physical Review* was in the April 15, 1939, issue). Then the problem was how to start a chain reaction, and Szilard was soon considering graphite, initially because his mind had leapt to the engineering considerations; graphite is a good high-temperature material. His study of graphite led in a few months to Report A-55.

But matters of public policy were even more urgent. With his compatriots Teller and Wigner, Szilard approached Einstein, and the result was the famous letter to Franklin D. Roosevelt in August 1939 (reproduced in the book), accompanied by a short memo by Szilard mentioning uranium procurement, bombs, and the necessity of secrecy. Later, Szilard sent an expanded memo to Lyman J. Briggs, who had been appointed by the President to work with the uranium people. Here he mentions that materials for a large-scale experiment might cost $153,000.

For a physicist, Report A-55 is the high point of the book. In it Szilard concludes that a lattice assembly of uranium spheres in graphite would support a nuclear chain reaction. Fermi had apparently been considering a layered structure to effect the necessary separation of uranium from moderator; Szilard went all the way to the lattice, and furthermore he championed graphite. His rather extensive analysis was based on the fragmentary information of the day as regards cross sections, impurities, resonance capture, and sleepers that did not appear until later, but history has shown that the main structure of the paper was sound.

Report A-55 was sent to the *Physical Review* on February 6, 1940, together with a note requesting that it not be published. It was declassified in 1946, but its first appearance in publication is in this book.

Following the Manhattan Project days in Chicago, Szilard turned to microbiology as a new and exciting frontier. One of his first interests was the invention of the chemostat, a device for the continuous culture of bacteria at a controllable rate. Some research came directly from the chemostat; for example, a given strain of bacteria cultured in it "evolved" in time into a somewhat different strain. His other contributions were varied, but each had a clear central point, like his work in physics.

The collected patents and patent applications that comprise the remainder of the book include two German ones, dated in late 1928 and early 1929, that are related to particle accelerators. The first contains the basic concepts of the radiofrequency linear accelerator, and the second contains the basic concept of the cyclotron. Both of these applications antedate the publication of similar ideas by Wideroe that caught Lawrence's imagination and started him on his cyclotron career. The second patent also includes a circular induction acceleration for electrons — the principle of the betatron, without, however, recognition of the magnetic flux conditions that make a betatron work. To carry the same theme further, a subsequent English patent application (1934) includes a suggestion of how to use a rotating condenser to modulate the frequency of a circular accelerator so as to compensate for lack of synchronism due to the relativistic increase of mass of the accelerated particles: the synchro-cyclotron!

As for the release of nuclear energy, the first patent applications (1934, '35, British) mentioning a neutron chain reaction suggest that the gamma rays produced by neutron capture might be used to produce further neutrons. As stated, a power plant would be attached. The suggestion also appears that deposition of lots of energy into a small volume of "diplogen" (deuterium) might release nuclear energy because of the high temperature; nowadays we call that controlled fusion. The fission chain reaction is dealt with in Szilard's U.S. patent application of 1939; in 1955 the patent was issued jointly to Fermi and Szilard.

It may be pardonable if I conclude with my personal recollections of Szilard. As a bachelor, he lived in the Quadrangle Club at the University of Chicago during the Manhattan Project period. My wife was working nights in a war plant, and to avoid dinner chores we used to go to the Quadrangle Club for the evening meal. Seeing Szilard sitting alone, we formed a habit of joining him. We enjoyed his wit and personal charm, but I still have the impression of a man always looking into the future, about three jumps ahead of everybody else.

Jim White here lines out his love affair with his chosen profession, analytical chemistry. It has come a long way and around many corners in the 23 years since Division Director White recalls picking up his doctorate at Ohio State and joining ORNL. He recounts this evolution from a very personal standpoint, and points with justified pride to his high-achieving division.

PROBLEM SOLVING

the evolution of analytical chemistry

By J. C. WHITE

OMNIA MUTANTUR, nos et mutamur in illis. All things are changing and we are changing with them. This struck me as particularly appropriate to the content of this article, namely, what it is that analytical chemists do at the Oak Ridge National Laboratory and how they go about doing it.

For many at ORNL, analytical chemistry evokes unpleasant memories of the quantitative analysis course in undergraduate college days. Obviously there were a number of us who found it pleasant and interesting enough to major in the field, but very few of the most dedicated among us feel nostalgic about the hours we spent watching the pointer of the balance swing back and forth, counting swings right and left, and dutifully entering their number and value in our notebooks — all for the sake of knowing we were using calibrated weights. Or the equal boredom of calibrating pipets, burets, and volumetric flasks and the horror of breaking the tips after all that onerous work was completed. These days we use calibrated weights and burets of accepted reliability — store-bought, as it were — and single-pan, almost instantaneous weighing. A word of caution is necessary here: complete faith in store-bought equipment is not always a safe practice. A number of years ago, when Clarence Larson was president of the Nuclear Division prior to his becoming a Commissioner, he spent part of his time during the summer working in our laboratory, directing a graduate student in radioanalytical chemistry research. Dr. Larson would call me before coming over from Y-12, so we could have his apparatus and equipment in order and ready for him to operate upon his arrival. One of his most indispensable items was a buret — a factory-calibrated job, of course. Imagine our embarrassment when he noted one day that the numbering on the buret he had been using for some time was off by one digit due to the omission of one figure in the succession of numbers from 1 to 50! A considerable amount of refiguring was required to make sense out of his data, needless to say; it was one of life's hopefully forgettable moments!

These errors are exceptions, however. Our lot these days is simple in that regard, as we accept the validity of basic equipment and accessories, with

J. S. Eldridge holds a model of a lunar rock that K. J. Northcutt has prepared for calibration of the low-level counting system used in the assay of radioactivity found in lunar samples. Exact replicas of rocks are prepared by taking impressions in aluminum foil and hardening them with epoxy. These models are then filled with iron powder containing known amounts of radionuclides and used to calibrate the counting equipment. The Analytical Chemistry Division has as a result a shelf of artificial — but dimensionally exact — moon rocks.

near-perfect results. Chemistry majors don't have to waste most of their lab time in calibration; neither do analytical technicians in the laboratory, so a considerable saving in cost, too, is realized.

Another memory many of us chem majors have is of trying to determine the constituents of a sample, along with their concentrations, of limestone. Usually needed for extra credit, the exercise was a monument to the mismatch of time spent with value received. Countless hours were needed to precipitate and separate a constituent, filter hundreds of milliliters of solution, wash the scarce precipitate endless times to ensure its separation from other components, evaporate liters of solution to a manageable volume — only to start all over again for another constituent. In case you have forgotten, there are a number of elements in limestone! Lurking overhead menacingly like the sword of Damocles was the ever-possible Accident, when you either spilled something or dropped the whole thing. The sword fell more often than it did not, so judicious addition and substraction were required, commonly referred to as "graphite analysis." Today few chemistry departments require limestone analysis — there are a host of easier ways to analyze limestone; no one follows directions in the old textbooks anymore. Nevertheless, the satisfaction of successfully completing a limestone analysis cannot be imagined unless you experienced it yourself. The discipline that one developed is a valuable attribute to the analytical

Analyst Geraldine Olerich "lights up" in preparation for determining the tar and nicotine delivery of experimental cigarettes. The tobacco smoke chemistry and tobacco smoke inhalation bioassay projects, supported by the National Cancer Institute, are directed toward identifying characteristics of a less hazardous cigarette. Chemists regularly generate data on approximately 25 components of cigarette smoke particulates, gas phase, and condensate for eventual correlation with biological data. Chemical research provides novel methods for the more complete characterization of smokes and basic knowledge, in collaboration with Laboratory biologists, on the impact of cigarette smoke inhalation on laboratory animals. Special cigarettes such as marijuana and synthetic smoking materials, and special projects such as the fabrication and evaluation of smoke delivery systems, also receive attention.

chemist, one that can hardly be acquired in these modern days when instrumentation and computerization are taught almost exclusively. In spite of the extent to which modern technology has changed the way we do things, one of the fundamental characteristics of a good analytical chemist is still fastidious carefulness. Few individuals with sloppy lab techniques can succeed in analytical chemistry. With the gradual disappearance in colleges and universities of experimental work whose success depended on this high degree of discipline, perhaps a caution note should be sounded before it is lost altogether.

Probably the most perplexing analytical course of all was the qualitative organic laboratory, where the students were charged with identifying a mixture of unknown organic compounds. The simple tests given in the text and reference books were rarely of help when you applied them to your unknown. Good luck, plus the adroit cultivation of the friendship of the graduate assistant who knew the makeup of the unknown, were as valuable then as the gas chromatograph and mass spectrometer are today.

Contrast the above experience with what goes on in the modern-day analytical chemistry laboratory. Weighing of samples is essentially automatic. Volumetric glassware now comes precision-calibrated at the factory. Separations are done by precipitations about as frequently as young adults get haircuts. We usually try to avoid separations because they take time, and many procedures are designed so that they are unnecessary; but, when they are needed, we have a battery of chromatographic techniques for all types of situations. Analysis of gas mixtures used to be done by the Orsat technique, in which the sample was passed through various absorbents by means of mercury, and the volumes were measured in a buret that was impossible to keep clean. We lost or discarded volumes of the order of 0.1 to 0.5 ml. Now we use a gas chromatograph for this purpose, and the entire sample to be analyzed is about the amount we used to lose in the Orsat.

The gas chromatograph is probably the most useful instrument in the analytical laboratory today. I have mentioned how the chromatograph has changed gas analysis, but its use in organic analysis is undoubtedly the most significant. It is not an overstatement to say that gas chromatography is indispensable in the operation of most organic chemical process plants. Entire production lines are controlled by these instruments. More relevant to this article is the impact of gas chromatography on food additive, pesticide, and drug analysis. This could be the topic of an entire book, but suffice it to say it is due entirely to the gas chromatograph that we can determine parts per billion of these compounds, with the net result that their use has become controversial and their proponents and opponents contentious. Not many years ago we were unable to determine DDT much below 1000 ppm; now we determine a few parts per billion. The same story applies for most of these compounds. The question now is, Just what does this low concentration really mean in its effect on man? The switch is that today the analytical methodology is far ahead of its application and use. A great deal of this change is a direct result of the developments from the atomic energy program, which introduced myriad difficult analysis problems that taxed the common sense and ingenuity of many of us in this business. I remember sharply my fledgling days at Y-12 when the ether fire in the hood was a daily occurrence. Ethyl ether was used to separate uranyl nitrate. The process was not only slow and deadening on the senses, but dangerous and spectacular. Less flammable and more efficient reagents for this separation evolved inevitably. I wonder if we could have achieved any success at all in those days if we had had to operate under today's safety practices. Processing of the uranium and plutonium fuels required a whole new area of analytical chemistry — the analysis of fission products and handling of radioactive solutions. Microanalysis took on a new look. Lead bricks were as common as pipets, and eventually master-slave manipulators and their taxing of our dexterity and depth vision became equally common. All this had a profound effect on what the analytical chemist did and how he should be trained. Gross alpha, beta, and gamma analyses were the staple work of many laboratories. The single-channel analyzer came into being, followed by the multichannel — first 20 channels, then 200, then 400, then 4000 and the PDP-15 computer and its magical resolution of complex radionuclide mixtures.

The reactor programs at ORNL not only introduced a new analytical chemistry but also a new concept in the role that the analyst played. My first industrial experience was in a steel mill laboratory, where we had to produce accurate numbers in a great hurry or risk the wrath and scorn of the melter. We were a partner, albeit a minor one, in the production of heats of stainless steel. The homogeneous and molten-salt reactor projects demanded the combined talents of engineers, metallurgists, physicists, and chemists of many persuasions. These projects spawned the formation of the interdisciplinary teams that have characterized the Laboratory programs for many years. Dave Novelli wrote an eloquent article in the Fall '68 *Review* telling how these talents were brought into play in the successful separation and purification of transfer ribonucleic acids.

And so I come to today's analytical chemistry as practiced at ORNL. Analytical chemistry is by definition an altruistic endeavor. We stand ready to do things for someone else to ease or upgrade his task and thus achieve a common goal. Our organization is centered around this theme. Everything we do is truly problem solving: this thesis is central to the training of analytical chemists in universities today. We perform countless analyses of widely differing types for the programs at ORNL, devote a great deal of time to developing and modifying methods to tailor them to the problem, and lastly and most significantly we do research in analytical chemistry to make these solutions of difficult problems possible. We have developed, evaluated, and put into practice many instruments that have made possible analyses hardly considered feasible in the past. Today we combine sophisticated instrumentation with minicomputers so that we can, for example, analyze 200 samples of aqueous solutions per hour for certain toxic elements by means of atomic absorption. We determine complex organic compounds with the mass spectrometer and the computer. We use the spark-source mass spectrometer to analyze for virtually all the elements in the periodic table in a single sample.

H. A. Laitinen, professor of chemistry at the University of Illinois and editor of *Analytical Chemistry*, recently wrote an editorial in which he defined the role of the analytical chemist in science. He said that analytical chemistry should not be defined in terms of its function of yesterday as though there had been no changes in the intervening period. "The need for sophisticated and detailed information about the composition of matter," wrote Laitenin, "ensures the necessity for a viable science of analytical chemistry as a research field for the foreseeable future." I concur heartily and add that the makeup of the analytical team has changed and will change more. Physicists, biologists, and engineers play important roles along with chemists today, but it's still problem solving and that's what analytical chemistry is all about.

Gerald Goldstein is testing the MAN Program—developed GeMSAEC for its ability to analyze drinking water and the air we breathe for trace pollutants such as zinc, selenium, copper, cadmium, etc., which are of interest at the parts-per-million level. This program of methods development is relatively new, and is being carried out under the auspices of the NSF-RANN Environmental Analysis for Trace Contaminants (EATC) study. The GeMSAEC shows great promise for this purpose, being extremely versatile; for example, it can move readily from one kind of quantitative analysis to another: spectrophotometric, reaction rate, and luminescence are a few.

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