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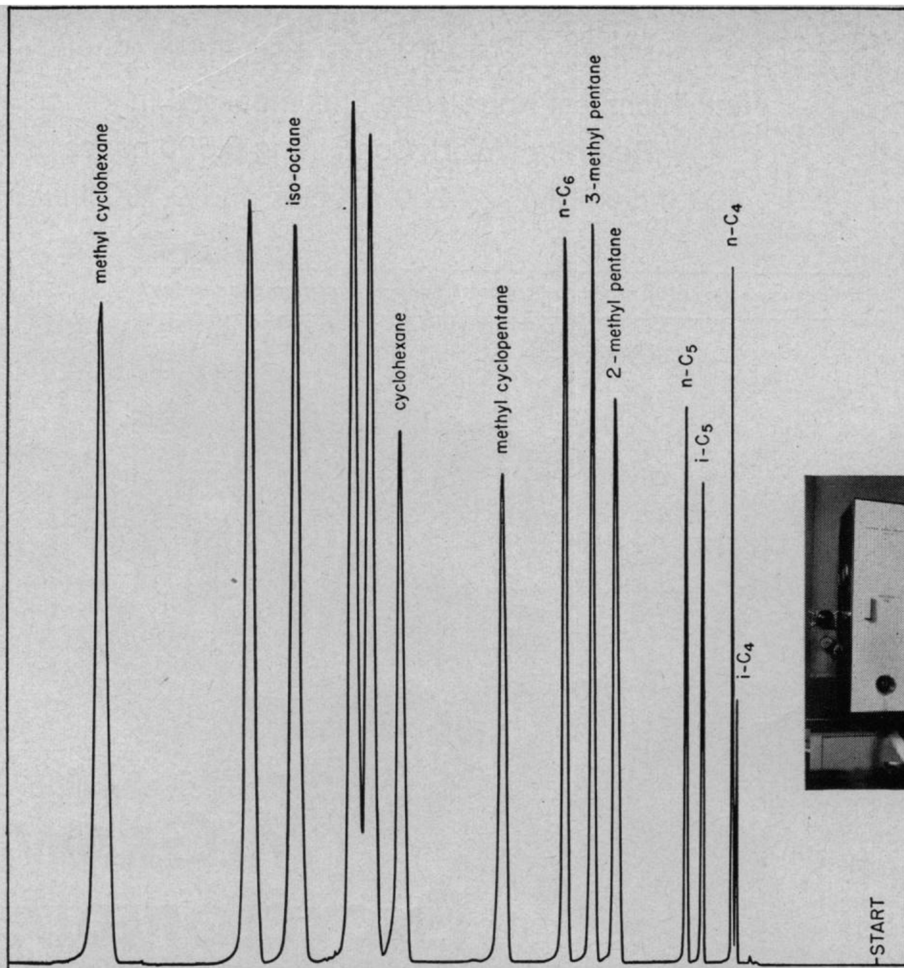

This chromatogram was run on a Model 154-D Vapor Fractometer, utilizing a 150′ squalane "U" Golay* column. Detector was of the flame ionization type. The chromatogram displays high-efficiency separation of a number of C_4-C_6 saturated hydrocarbons.

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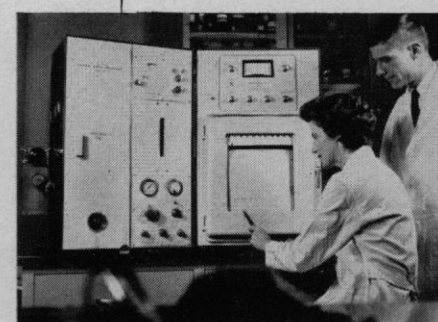

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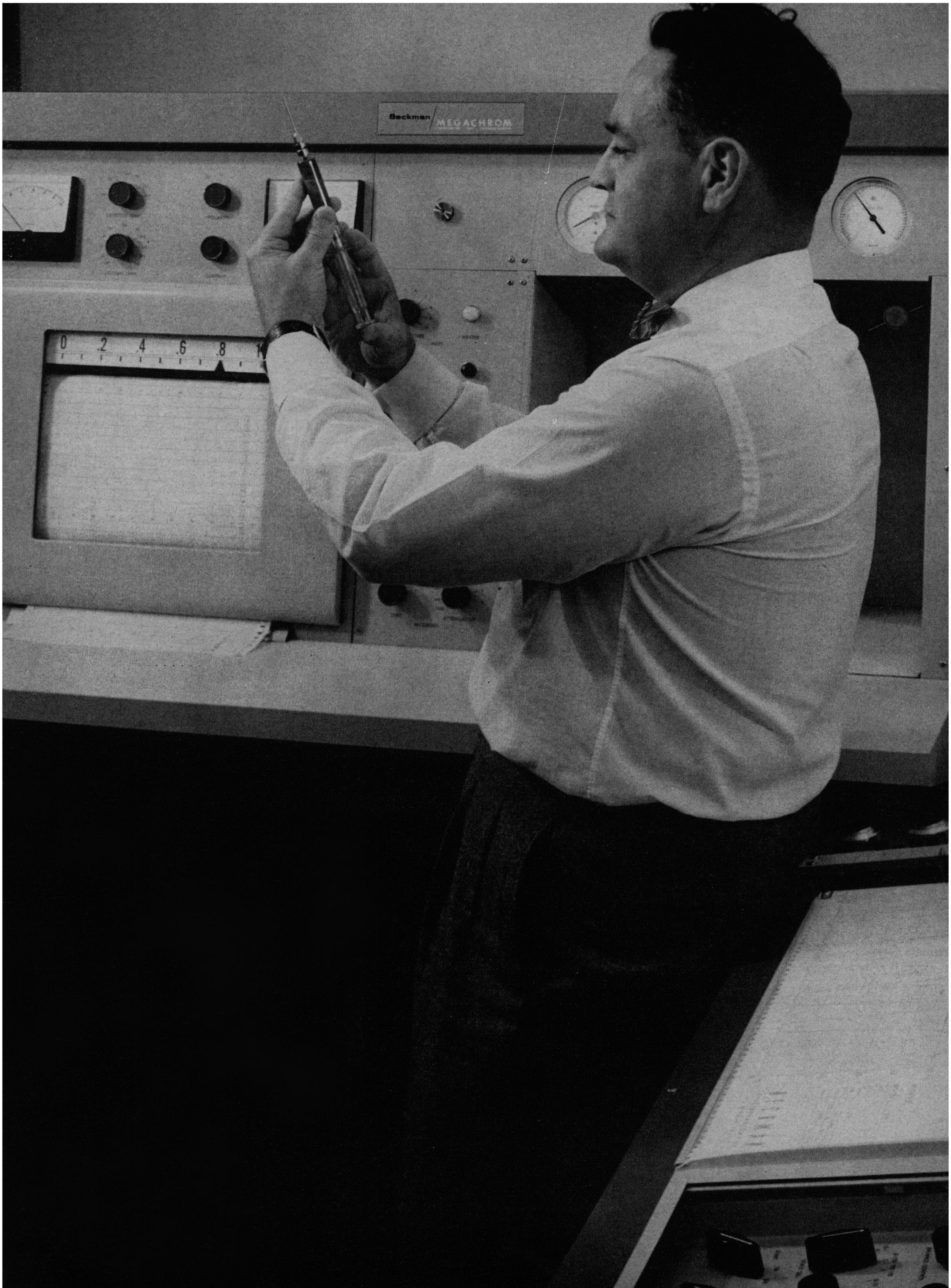
26 August 1960, Volume 132, Number 3426

SCIENCE

Cover The amphitheater surrounding Rainbow Bridge, Rainbow Bridge National Monument, Utah. See page 519. Photographed by Russell I. Alley, Salt Lake City, Utah. [U.S. Bureau of Reclamation]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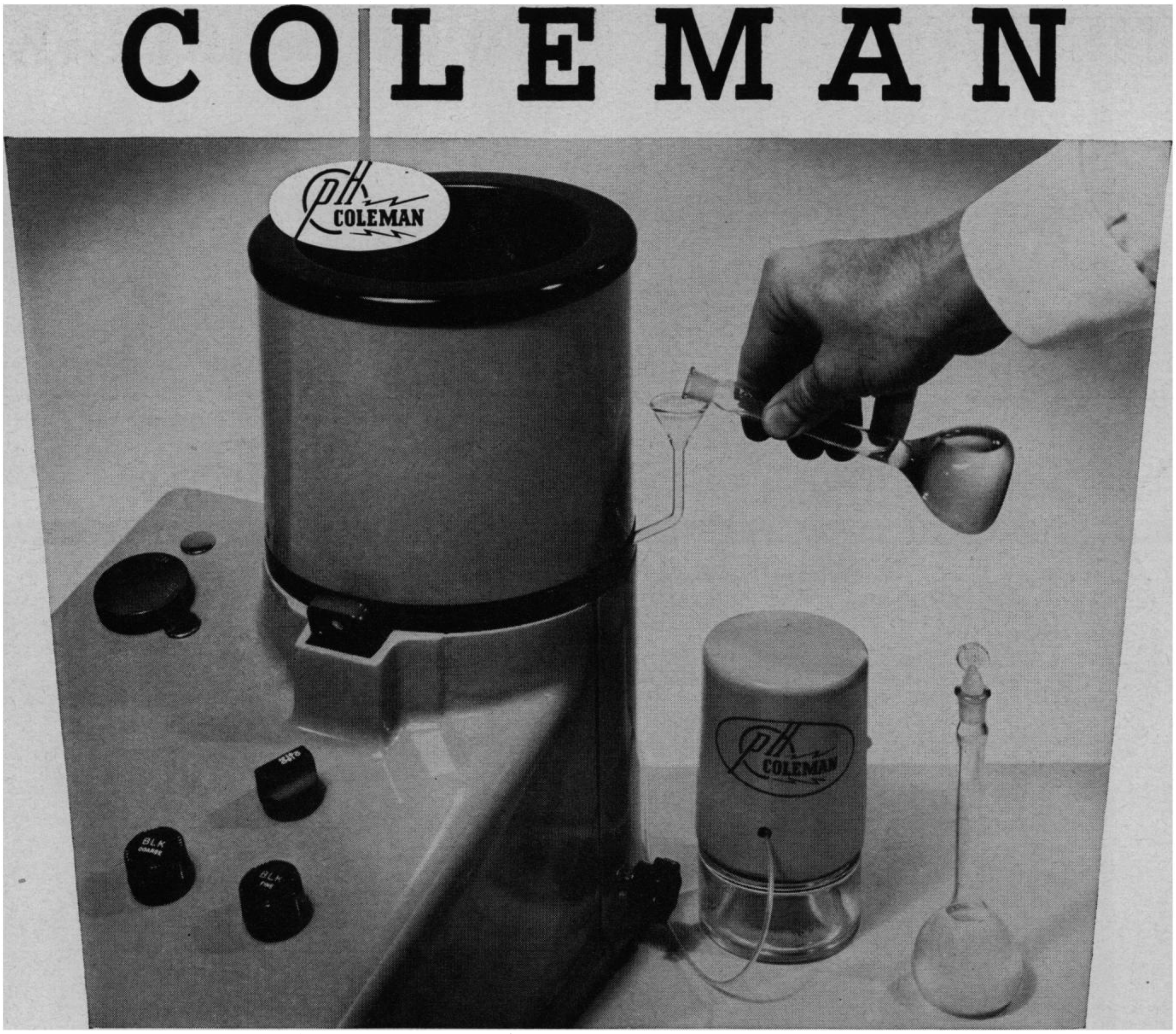
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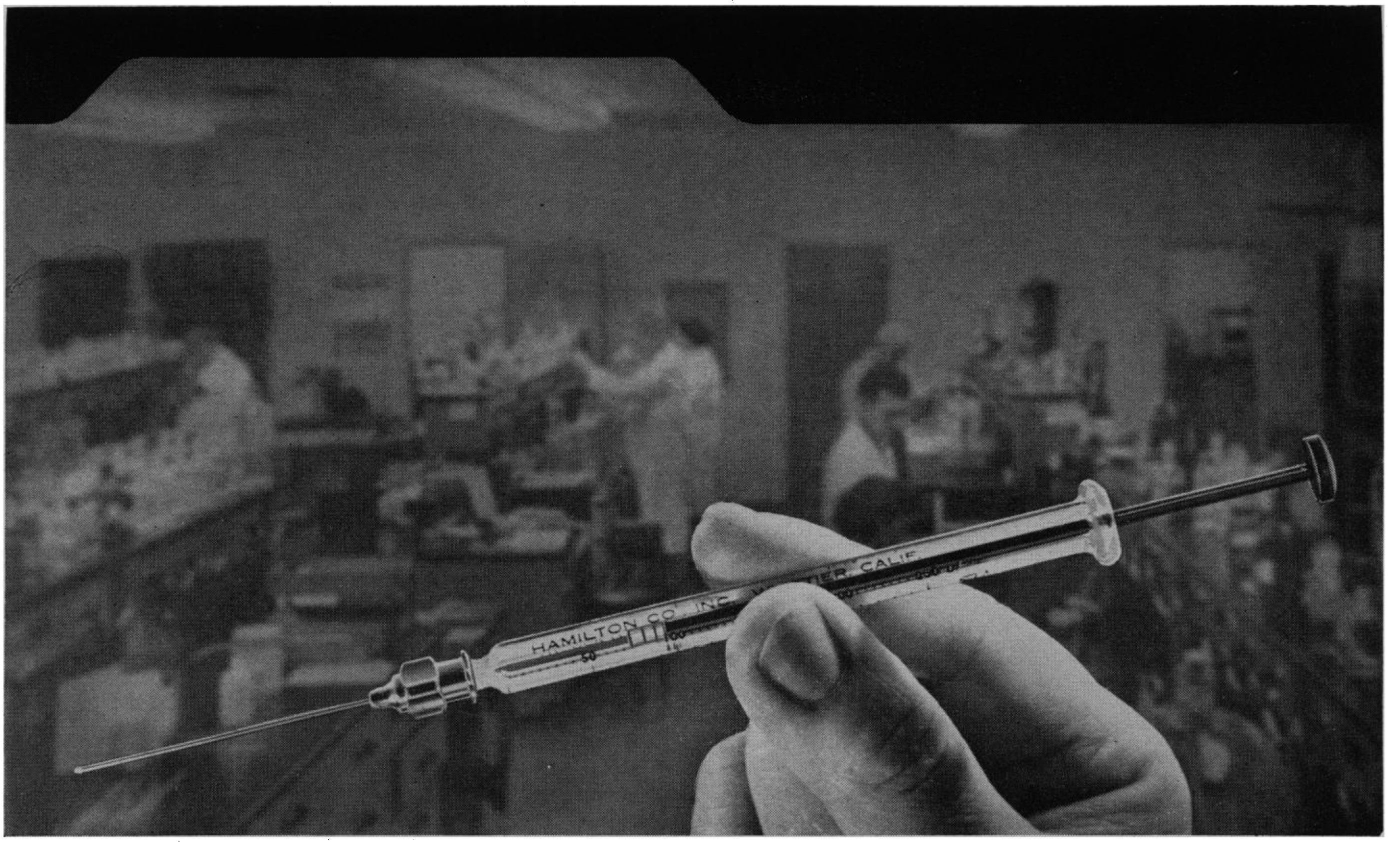

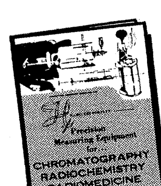
CHROMATOGRAPHY
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SCIENCE

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How Much Research for a Dollar?

That a dollar will not buy as much now as it would in 1950 is clear to every purchaser who can remember 1950 prices. That the \$13 billion the nation will spend on research and development this year will not buy as much equipment or the services of as many scientists and engineers as would the same amount in 1950 is also true, but not so immediately obvious; the figure is so large, the cost elements so varied, and the experience of scientists with figures of this magnitude so meager that one may have to think about the matter to realize that increasing the research and development budget from a little under \$3 billion in 1950 to about \$13 billion in 1960 does not buy a comparable increase in research and development effort.

Ellis A. Johnson and Helen S. Milton of the Johns Hopkins University Operations Research Office have sought to develop a cost-of-research index that will be useful in analyzing research and development trends. They start with the concept of the cost per year of a technical man; "the 'technical man' is the professional scientist or engineer, together with his supporting technical, administrative, and housekeeping staffs, and his machines and equipment, i.e., the man plus the overhead costs."

Johnson and Milton secured cost records from 17 laboratories, varying greatly in size and roughly equally divided among industry, government, university, and private nonprofit institutions. There was variation in the experience of these laboratories, but there was also a quite useful degree of consistency among their records, sufficient to justify the conclusion that when the costs per technical man were applied to the national research and development totals for the past decade, the result demonstrated that for 4½ times as much money as was spent in 1950 we are getting slightly more than twice as much research and development activity.

Using 1950 as a base year in which the cost-of-research index was 100, the authors report that from 1920 to 1940 the index varied only a few points above or below 50. Between 1940 and 1950 it doubled. Since 1950 the increase has averaged 7 percent a year to bring the index to 191 in 1960. The major elements in the increase have been higher salaries and overhead costs. Professional salaries are going up at about 7 percent a year, overhead costs a little more rapidly, and equipment and material costs not quite so rapidly.

It is always proper to ask whether or not research and development budgets are appropriate to the national needs. Are they increasing too slowly, too rapidly, or at about the right rate? In an ultimate sense this question can be answered only in terms of national needs and the valuation placed upon the work accomplished. Decision on these matters must be in terms of judgment, but a generally accepted cost-of-research index should assist us to reach better informed judgments. For example, Johnson and Milton write, "In military technology . . . there are some critical areas in which the absolute dollar support has increased but in which less actual technical effort is being expended than in 1950." Is this what we want? Surely there are times when a decrease in effort is justified, just as there are times when an increase is mandatory, but we should know what we are doing, and not be fooled by the fact that we are spending more dollars to buy less research. When we are spending \$13 billion a year and the amount increases year after year, we need a cost-of-research index just as, for other purposes, we need a cost-of-living index.—D.W.

Letters

Teacher Training

Having been involved as a member of the curriculum committee of the State University of New York College of Education at Cortland in a recent revision of our curriculum for the preparation of elementary school teachers, I was interested in the report by John Mayor of the proposals coming from a meeting of representatives of the AAAS and various certification groups from several states [*Science* **131**, 1779 (1960)]. May I suggest that it is an admirable aim to require four courses in mathematics for all prospective elementary teachers, but that this is unrealistic as long as students come to college so poorly prepared in even basic arithmetic, and unreasonable unless the AAAS and other groups concerned in this matter are willing to give wholehearted support to the upgrading of teachers' salaries and professional standing. If our students came to college able to handle the mother tongue and having learned their lessons in basic science, social studies, and so forth, it might be possible to eliminate what is really remedial work in the colleges and thus gain time for teaching these recommended and truly collegiate courses.

It is clear in my mind that we cannot hope to adequately prepare a person to teach in the elementary school in the usual four years of college. If scientists and engineers are born and nurtured in the elementary schools of our land, then elementary school teachers must be ready to play the vital role assigned them. If this requires four courses in mathematics, then it also requires eight courses in science, to say nothing of foreign language, now almost forgotten in the preparation of elementary teachers. Then must come English, speech, music, art, sociology, history, psychology, philosophy, health, political science, and the necessary pedagogical courses. Physical education must not be neglected, and every teacher needs apprentice teaching experience. No college program should eliminate the possibility of electives. What this all adds up to is at least five years of college work, if not more. When this is balanced against potential pay and prestige, is it any wonder our best prospects turn their backs on teaching?

In our curriculum study here each department brought its recommendations for those courses considered essential in the training of elementary school teachers. From this immense total was carved a compromise which would total the 132 semester hours required for graduation. No one is happy with the result, but compromise is the best that can be done. As a liberal-arts college graduate sympathetic to the recent cries against "too much professional education in our teachers colleges," may I say that I believe the 21 hours (equal to seven courses) in pedagogy, including 3 hours in general psychology and 6 hours in human growth and development, which are included in our curriculum are an absolute minimum. The only way that an adequate job can be done is to lengthen the course of study. The recent five-year programs started at some engineering colleges are attempts to cope with the same problem. These are things of which scientists cooperating in these studies must be aware.

JOHN A. GUSTAFSON
State University College of Education, Cortland, New York

The Scientist's Image of Himself

Gerald Holton rightfully points out [*Science* **131**, 1187 (22 Apr. 1960)] the schism that exists between scientific knowledge and other currents of intellectual thought. Although he suggests that scientists themselves contribute to this schism, his major emphasis is on the image that the public has of science, and of the scientist. I think that the images scientists hold of themselves, and particularly the changes that are evident in their self-representations, also play a very significant role.

I had an opportunity to investigate the self-images of 40 research scientists, men in natural-science fields at academic installations, as one part of a larger psychological study of persons who have selected research science as their vocation. The findings brought out that in some aspects of their identification scientists are caught up in some of the sterotypes about men of science that exist in the public mind. For example, they see themselves as intellectuals, as discoverers of new worlds—worlds which they not only create but in which they then proceed to live. Their work is propelled primarily by pressing "inner" drives; thus, the majority scorn "impure" motivations, such as the desire for recognition, exhibitionism, personal aggrandizement, pragmatic reward, unless these are inescapable concomitants of devotion to the search for truth. Happiness and fulfillment rest primarily in satisfactions at work, with routine, drudgery, administrative duties played down as interferences. In fact, rigor, persistence, and discipline have become institutionalized in their morality code as values in themselves, and the "gentleman scientist" is looked down upon as a laggard who is bound to be unproductive.

It is obvious that such facets of identification emphasize how isolated the researcher is, and how removed from the general cultural pattern. However, he shares many of the elements that would establish bonds between men of science and other intellectuals. Where the break with the intellectual comes is in some of the aspects of identification which are in the process of change, and where some of these most highly stereotyped conceptions about scientists are giving away.

Change in self-image seems to have been most stimulated by the differences that have emerged in the way science is pursued, and the attitudes and values that have accompanied these differences. I shall mention only a few to show the trends of change. For one, the researcher is now shying away from identification with the "great, but maladjusted" or "eccentric" scientist. Reverence for forefathers whose outstanding minds were sometimes housed in odd personalities still exist, yet today's scientists consciously dissociate themselves from peculiar and difficult associates or students, knowing full well that they may be shunting off some very creative workers from their own laboratories. They prefer to depend for their progress on a well-organized, smooth-running, large-scale operation, whose stability demands a minimum of interpersonal relations, especially disturbed ones. Even colleagues who "play expert" in nonscientific fields and attempt to generalize scientific knowledge in relation to cultural problems are frowned upon, and often their scientific work is looked upon retrospectively with suspicion.

Another change comes in the new interest in "putting breakthroughs across." While these men still stress that the main motivation of scientists is the gaining of understanding and knowledge without concern for its immediate application, they feel that the fruits of their searches can be more readily put to good use if they adopt what I have called the skills of "sciencemanship." Some think that success in science and the manipulation of that success are a natural sequence, if one realistically acknowledges that the same motivations which are found in other workers are also present in scientists—such as jealousy, competition, the desire to please a superior.

In general, the new model of science is a corporate one. With this, and its spelling out of all the multiple subparts, budgets, and personnel and administrative matters, comes an increasingly tight definition of what each scientist specifically does at work. This is an important change in the scientist's view of his role, which has characteristically been in part ambiguous and indefinite, hinting at the possibility of multiple choices. Many scientists have clung to this ambiguity because it seemed to them to mirror some of their attitudes toward the intellectual problems with which they grappled—their dissatisfactions with the obvious and with conventional ways of seeing reality.

It seems to me that these changes in self-image imply that the scientist may be moving outside the intellectual pale and that he thus may remain quite unconcerned with the cultural schism that Holton points out. The "impotence of the modern intellectual" has a counterpart in the impotence of the modern scientist. If the intellectual is moving away from the scientist and the scientist is, at the same time, moving away from the intellectual, the summation would indicate that the two may be drawing apart at an alarming rate.

BERNICE T. EIDUSON
Reiss-Davis Clinic for Child Guidance, Los Angeles, California

Bernice Eiduson's letter is a useful amplification of some points on which I only touched in my article. Because the latter was largely on the image of science and of the scientist in the mind of the literary intellectual, it did not attempt to develop also the interesting problem of the scientist's own image of his work and profession.

There are, I think, at least three good reasons why one should be careful to discuss these problems separately. I cannot develop them here, but I can mention them briefly.

The first point is simply a practical one. Imaginative remedial action is quickly needed on both fronts, but urgency for action is all too often destroyed if each "side" is allowed to make the comfortable mistake of cancelling its obligation by detecting equal needs of reform on the other "side."

Secondly, the two problems are not compatible in an important sense: It is, as Dr. Eiduson implies, bad enough if scientists have an erroneous conception of the nature of science; but it is even worse if this is the case of intellectuals outside science, for we have always depended on the latter to fit science into the total pattern of knowledge. It is not the Newtons who give us Newtonian syntheses but the Voltaires.

Lastly, even at the risk of being widely misunderstood, I feel I must point out that the two problems are unsymmetrical: despite Dr. Eiduson's uncomfortable findings on 40 scientists, the general policy and the tone of scientific work in this country are still being set by men and women who give us far more and far better statesmanship than we might have any right to hope for. Just as in my article I was concerned with the image of science in the minds of the intellectual leaders to whom the public looks for guidance, I find it significant that among the leading scientific policy makers we still have the quality, insight, and range of concerns of such men as Rabi, Szillard, Oppenheimer, Bronk, Wiesner, Stratton, Kistiakowski, Beadle, Harrison Brown, and Glen Seaborg, to name only a few at random.

Perhaps, however, this hopeful aspect of present scientific leadership is only transitory. These men received their training in the days before the "new science." If the concerns of the new generation of scientists fit those expressed in Dr. Eiduson's sample, one may indeed wonder what will happen when the men and women she studied take the place of our present spokesmen for science.

GERALD HOLTON
Department of Physics,
Harvard University

Computers and Game-Playing

Since *Science* is intended not only for scientists in general but also for intelligent and interested laymen, it seems unfortunate that Norbert Wiener [*Science* **131**, 1355 (1960)] should have permitted himself to use the jargon of computer specialists without any explanation of the special meanings which accompany this jargon. Wiener discusses checker-playing machines, chess-playing machines, and learning machines, and this may give the impression that these are actual physical embodiments of such abilities. Actually, no such machines exist. Wiener has here followed the practice of discussing a program on a general-purpose computer as though it represented a special-purpose machine which would operate in the manner set forth in the program.

A program is a set of numbers in a certain order, and without human interpretation it remains only that. Undoubtedly a machine could be built which could move checkers and chessmen, but it could only operate on standard-size pieces and could not recognize as chessmen the innumerable pieces of different design which the human player recognizes and moves around quite simply.

It is also possible to play abstract chess, like games in a book, without moving pieces physically; but there are analog relationships in real chess—for example, the emptiness of a line, which is the requirement for movement or castling—which cannot be directly handled by any digital machine. These analog relationships can be approximated digitally by remembering and recalculating the moves of all other pieces in order to determine whether a given line is empty and, hence, that a certain move is possible. But such a set of calculations is not identical to the visual recognition that the space between two pieces is empty. A large part of the enjoyment of chess—for example, its relationship to war games—derives from its deployment or topological character, which a machine cannot handle except by elimination. Wiener recognizes this problem by noting that different programs would be required for opening, middle, and end games. If *game* is used in the usual sense—that is, as it was used before the word was redefined by computer enthusiasts with nothing more serious to do—it is possible to state categorically that machines cannot *play* games. They cannot *play* chess any more than they can play football.

In Wiener's earlier work, *Cybernetics*, he was able to make a case for learning machines only by equating learning with conditioned reflexes. As a matter of fact, the doctrine of conditioned reflexes as an explanation of human habits and human learning is certainly questionable, if not false. Sherrington can be accepted as an authority in stating that although the behavior of living organisms can be modified by subjecting them to certain patterns of experience, reflexes in the physiological sense of that term are not conditionable or modifiable. In short, the expression *conditioned reflex* is a contradiction, because physiologists distinguish reflex activity from other types of nervous activity on the basis of the fact that reflexes cannot be conditioned. There is nothing more strange or mysterious in this fact than there is in the denial of the Lamarckian doctrine of the inheritance of acquired characteristics. Sherrington explicitly and categorically distinguishes reflex behavior from habitual behavior on the grounds that habitual behavior is acquired and modifiable, whereas reflex behavior is not.

Having described the feedback operations of computers in *Cybernetics*, Wiener goes on to say: "I wish to emphasize that I do not say that the process of the conditioned reflex operates according to the mechanism I have given; I merely say that it *could* so operate. If, however, we assume this or any similar mechanism, there are a good many things we can say concerning it. One is that this mechanism is capable of learning. It has already been recognized that the conditioned reflex is a learning mechanism. . . . There is nothing in the nature of the computing machine which forbids it to show conditioned reflexes."

When reputable scientists begin to accept explanations merely on the basis that they *could* be true and that nothing *forbids* their being true, science becomes indistinguishable from superstition.

One final comment should be made about the danger of heuristic arguments about heuristics in science. Wiener objects to von Neumann's theory of games, which depends upon stating the complete formal rules of a game, and suggests that we substitute tentative play modified by experience. He justifies this suggestion by pointing out that this is the way human beings play chess, in particular, or run their affairs, in general. He points out that certainly Napoleon won his victories by modifying his strategies in terms of the different abilities and responses of his opponents. He seems not to recognize that this strategy also led Napoleon to Russia and Elba.

MORTIMER TAUBE
Documentation Incorporated, Washington, D.C.

Science and Human Affairs

The AAAS Committee on Science in the Promotion of Human Welfare should be congratulated on its statement [*Science* **132**, 68 (8 July 1960)]. Most of us will agree that the problem discussed is real and urgent. No one should oppose the suggested preparation and dissemination of reports for the general public.

The "development of liaison between scientists and the public on a local basis," however, raises some prickly problems and should stir up a continuing debate. The scientist who tries to take part in political and economic activities as a scientist, instead of merely as a citizen, comes face to face with the fact that all important decisions in business, politics, and war are made on the basis of inadequate information and unproved theory; in an embarrassingly large number of cases, the scientist can only point out the depth and breadth of human ignorance—and shut up. But in many cases the border line between personal prejudice and scientific theory is pretty dim, and the man who sets out to explain the facts about radioactive fallout or increasing birth rates is apt to find himself defending a political philosophy or religious dogma.

Unfortunately, it is a general rule that men tend to be radicals in fields which they know well and conservatives in areas where their knowledge is superficial. Physical scientists and engineers are usually classed as reactionaries by the more original theorists of politics and economics. It would be unfortunate if our efforts to serve humanity merely resulted in identifying science with the most archaic,

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unimaginative, and sterile approaches to social problems. (Note: I am a physical scientist.)

We can avoid such traps if—and only if—we emphasize the fact that the real contribution which science can make to problems of human relationships is *not* "facts" or "answers." The real contribution is an attitude and a set of values. Science did not gain its present power and influence simply by being an orderly accumulation of data; a large telephone directory may contain a million facts, in perfect order, and an infinite number of telephone directories would contain an infinity of factual information—and be of no value whatever for solving our major problems. Science, as we now know it and value it, exists because there have been men with a passion for ideas, whose first loyalty was to intellectual honesty—men who valued an increase in understanding more than they valued the comfort and safety of accepted authority. They have created a tradition of objectivity, tolerance, free speculation, patient analysis, and open communication. They have fought, when necessary, for the right to consider alternatives to established dogma and the freedom to discuss ideas, including the ideas of other men in other countries.

There is little doubt that such an attitude, if widely understood and generally accepted, could be a major step toward the better ordering of human affairs. I recognize the fact that there are many who will argue that the application of the scientific attitude to social relationships is impossible—that the human animal simply has to have some outlet for the irrational and psychopathic forces which are within us all. It is obvious that we all seek someone to blame for our frustrations and inadequacies. There is something irresistibly attractive in the thought that we can please God and solve all problems by simply burning heretics and despoiling unbelievers. War, politics, and religion have traditionally satisfied these needs, and it may be argued that they always will. We can only say that, if this is so, "always" may be a very short period of time. But we have good reason to hope that there are unexplored potentialities in human psychology. Books like Ruth Benedict's *Patterns of Culture* tell us that there are alternatives to most of the sources of satisfaction which we take for granted, and that enduring societies can be based on these alternatives. It is not absurd to try to create a world in which scientific discovery will give as much emotional satisfaction—and bring as much prestige—as killing large numbers of strangers, or one in which intellectual integrity will be as powerful as fanaticism.

To explain, apply, and defend our scientific values is neither easy nor safe, however—especially when we try to apply them to controversial policies. The committee report ably, but too briefly, points out the pressures which are being applied to make science an instrument of national policy, and to make it sacrifice its best traditions in the hope of transforming it into a dependable military weapon. The committee might well have added some comment on the informal social pressures, which can be equally strong and equally deadly. The fact is that most laymen regard the application of scientific objectivity, tolerance, and logic to problems which touch on religion, patriotism, politics, or sex as blasphemous, subversive, and immoral. And a surprising number of scientists feel the same way. Few of us are any more judicious or restrained, outside our laboratories, than the average layman. On the whole, we cooperate enthusiastically with the efforts which our nonscientific friends are making to run the world by tradition, prejudice, and simple inertia.

A program somewhat along the lines recommended by the committee is probably essential if science, as we know it, is to survive. Otherwise, we may expect an ever-increasing blanket of secrecy to cover the progressive degradation of science to the status of a weapon for cold war or hot, and ever-increasing suspicion, hostility, and misunderstanding to divide scientists from nonscientists. Such a program, though, will call for more courage, more action, and more conscious resistance to mass hysteria than most of us have been willing to contribute to date. During the recent years which were dominated by Joe McCarthy, the real point at issue was not the guilt or innocence of his victims. The real question was whether the American citizen could be punished for opinions as well as for acts—whether our traditional freedom of thought, freedom of association, freedom of movement, and freedom of speech were absolute or subject to the whim of any official who claimed to act in the national interest. The question was obviously of vital importance to scientists, yet scientific and technical societies took remarkably little part in the debate, and scientists as individuals applauded the witch-hunt about as often as they condemned it. We shall have to do better in the future if science is to have either the respect or the understanding of future generations.

William Palmer Taylor
416 Ross Avenue,
Hamilton, Ohio

The suggested approach of the AAAS Committee on Science in the Promotion of Human Welfare is a fine beginning for restoring balance in the relationships between the scientist and the general public. As the committee comments, the problem is an acute one, becoming more so, and some sort of crash program is needed in these frightening times. In this light, I think the four-point program proposed—(i) stimulation of discussion among scientists, (ii) gathering of facts relevant to an issue, (iii) dissemination of reports to the public, and (iv) development of local liaison—is a good one.

However, with regard to points (iii) and (iv), I think an approach on a more vertical front than that proposed is required. The adult public as it now exists has little if any background in science and scientific thought. It has had schooling in which the study of science was largely avoided. This is true even of the political and economic leaders of our society. Many of those with college educations have had business or liberal arts training in which the science requirement has been at a minimum. Those with high school educations have had even less science.

There is therefore a poor foundation on which to build a structure of scientific awareness in our lay population. A man who has had no physics is not likely to grasp fully the implications of the nuclear revolution. A man with no biology or mathematics cannot really understand the problems of the population explosion. Although I do think we should attempt to reach the adult public according to the proposals advanced by the committee, I also think we have considerably more to gain by an expanded program directed toward our student population.

There is much talk of increased science teaching in our schools. However, it must be remembered that these curricula are still being run by the same administrators who were in charge before and who were dedicated to making school as easy as possible. They are the least well educated people in our educational system and are hardly qualified to administer a meatier program without guidance and support from scientists themselves. It appears to me that those schools which had strong science curricula before have them still and that those which had weak programs still have weak ones.

This is not to say we should educate all to be scientists. I am saying we can do a lot more to see that our students are taught enough science and scientific method of thought to enable them to consider the problems created by scientific advance and have some hope of solving them intelligently.

To be specific, I think we should (i) participate more, as individual scientists and in groups, in the educational affairs of our communities; (ii) create committees, if they do not already exist, to consult with state and national educational organizations and with state education commissions to impress upon them the importance of expanded science teaching in the schools; the committees should be prepared to present suggested curricula, textbooks, and teaching methods to these organizations. (iii) these activities should be conducted on a broad scientific front rather than by individual groups representing biology or chemistry or mathematics.

We are forced now to take almost defensive measures as scientists against a relatively ignorant and sometimes hostile public. By making greater efforts on behalf of our students, we should be able to look forward to a time when the general public will be aware of the peculiar methods and needs of science and of the problems created by scientific advance; will understand the difference between science and technology; will not exert the pressures which undermine scientific integrity; and will not have to be forced-fed a diet of science in order to gain some dim understanding of the tremendous changes occurring in the world.

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Meetings

The Pancreas

A symposium on the pancreas, sponsored by the Mallinckrodt Foundation, was held from 23 to 25 May at Endicott House, Dedham, Mass. The discussion covered several phases—first, the pancreas as a whole with its anomalies, its injuries and infections and its tumors, both benign and malignant. The comments and conclusions may be summarized as follows. Most of the clinical tests for pancreatic function are relatively inaccurate. Pancreatic disease, once initiated, tends to be progressive. The volume of secretion from the pancreas is surprisingly large, amounting to several hundred cubic centimeters per day. There is no known disorder due to overfunction of the pancreas as an exocrine organ, although probably the reason alcohol makes pancreatitis worse is that it increases the amount of secretion produced by the still functioning portions of the pancreas.

Mucus is an important constituent of the pancreatic secretion, and disturbance in mucus secretion as part of a generalized disease process leads to the development of cystic fibrosis. The basic unit for exocrine function seems to be protein particles associated with the microsomal portion of the cell, which are concerned with making the enzymes. The key units probably come from the endoplasmic reticulum of the acinar cells. There is evidence that some type of feedback mechanism exists whereby the output of the proper enzyme is maintained to meet dietary needs. The particles of enzyme apparently require magnesium ions to maintain structural integrity. The enzyme particles form in the Golgi region, grow larger, and take up additional liponucleoprotein to become zymogen granules. The formed zymogen granule is from 0.5 to 1.5 micron in diameter. The nucleus seems to have a gene-governing function but does not participate directly in the process of enzyme formation. The zymogen granules pass by lacunar spaces through the cell membrane. The reaction of the fluid within the ducts is quite alkaline. It is possible that the bicarbonate which helps to maintain this alkalinity is secreted by the centroacinar cells.

Among other diseases that affect the pancreas as a whole are hemosiderosis and hemochromatosis; these are difficult to distinguish from one another and perhaps are the same disease process basically. The effects of the disturbed iron metabolism are apparent in the pancreas as well as in other organs of the body, and there is damage to practically all the cells of the islands indiscriminately, except that the alpha cells seem to be relatively free from iron pigment. When the beta cells are sufficiently involved in hemochromatosis, diabetes results.

Decrease in pancreatic exocrine function is largely due to inflammatory or obstructive processes. Obstruction is probably the most important in bringing about loss of exocrine function, and obstruction always leads to infection, which, in turn, tends to destroy additional pancreatic tissue. In general, the islands of Langerhans resist inflammatory and neoplastic processes. Administration of crude extracts of the pancreas in general constitutes satisfactory substitute therapy in the event of loss of exocrine function. While ectopic pancreatic tissue may take over to some extent for a damaged pancreas, the occurrence is rare and the mass is usually small. Usually ectopic pancreas carries ductal, acinar, and insular elements. Under certain conditions new formation of islands may occur, even in the diabetic patient. This has been observed both in man and in experimental animals. The new formation comes chiefly from the epithelium of the ducts. Under normal circumstances, cellular turnover in the islands is extraordinarily slow. With acute damage, as in alloxan poisoning and acute toxemic diseases, the beta cells may be particularly damaged

and regeneration may occur promptly.

The insular tissue makes up about 2 percent of pancreatic mass in normal tissue and may make up as much as 3 percent in obese persons. Insulin plays a role in stimulating pinocytosis in certain fat cells. The ratio of alpha to beta cells is approximately 1 to 3 in man. In the diabetic patient, the average weight of the island tends to be about one-third that of the normal island. If there are not qualitative evidences of damage to the island in diabetes mellitus, there is almost always a quantitative reduction. The first evidence of disease in the islands is hydropic or glycogenic vacuolization, which may progress to fibrosis or hyaline deposition. The pancreas of the young diabetic contains much less insulin than does that of a person who becomes diabetic later in life. Interestingly enough, the insulin requirement of the totally depancreatized individual or animal is somewhat less than that of the diabetic.

Insulin is clearly related to the beta cell, glucagon to the alpha cell. Insulin may properly be considered a growth hormone as well as a regulator of carbohydrate metabolism. Insulin tends to build glycogen and reduce fat. Unless special precautions are taken, insulin contains glucagon. Glucagon increases the metabolic rate and causes ketosis even before glycosuria develops. Glucagon may induce diabetes in some animals. After prolonged administration of glucagon to the experimental animal the alpha cells decrease in number.

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Forthcoming Events

September

20–23. Conf. on Pure Food Laws, London, England. (Secretariat, Pure Food Centenary 1960, 14 Belgrave Sq., London S.W.1)

20–24. Aeronautics, 4th European cong., Cologne, Germany. (Wissenschaftliche Gesellschaft für Luftfahrt, Eberplatz 2, Cologne)

20–7. International Atomic Energy Agency, 4th general conf., Vienna, Austria. (IAEA, 11 Kärntner Ring, Vienna 1, Austria)

21–22. Industrial Electronics, 9th annual symp., Cleveland, Ohio. (G. E. Hindley, Reliance Electric & Engineering Co., 24701 Euclid Ave., Cleveland 17)

21–23. National Power Conf., Philadelphia, Pa. (A. B. Conlin, Jr., ASME, 29 W. 39 St., New York 18)

22. Society of Plastics Engineers, Binghamton, N.Y. (T. A. Bissell, SPE, 65 Prospect St., Stamford, Conn.)

22–23. High Temperature Resistance and Thermal Degradation of Polymers, symp., London, England. (Symposium Subcommittee, Plastics and Polymer Group, Soc. of Chemical Industry, 14, Belgrave Sq., London, S.W.1)

22–26. Cancer Cytology, intern. conf., Madrid, Spain. (Miss E. L. Hughes, Pan American Cancer Cytology Soc., P.O. Box 633, Coral Gables, Fla.)

23–25. Inter-Society Cytology Council, annual, Chicago, Ill. (P. A. Younge, ISCC, 1101 Beacon St., Brookline 46, Mass.)

24–2. American Soc. of Clinical Pathologists, Chicago, Ill. (A. H. Dearing, 2115 Prudential Plaza, Chicago, Ill.)

25–28. American Inst. of Chemical Engineers, Tulsa, Okla. (F. J. Van Antwerpen, AICE, 25 W. 45 St., New York 36)

25–30. North American Assoc. of Alcoholism Programs, 11th annual conf., Banff, Alberta, Canada. (J. G. Strachan, Alcoholism Foundation of Alberta, 9910 103rd St., Edmonton, Alberta, Canada)

26–28. American Inst. of Electrical Engineers, petroleum industry conf., Oklahoma City, Okla. (R. S. Gardner, AIEE, 33 W. 39 St., New York 18)

26–28. American Soc. of Mechanical Engineers, petroleum mechanical engineering conf., New Orleans, La. (A. B. Conlin, Jr., ASME, 29 W. 39 St., New York 18)

26–28. Electronic Industries Assoc., natl. conv., Pittsburgh, Pa. (V. M. Graham, EIA, 11 W. 42 St., New York 36)

26–28. Standards Engineers Soc., annual, Pittsburgh, Pa. (J. A. Caffiaux, SES, 11 W. 42 St., New York 36)

26–30. Instrument Soc. of America, New York, N.Y. (W. H. Kushnick, ISA, 313 Sixth Ave., Pittsburgh 22, Pa.)

26–1. Natural Rubber Research Conf., Kuala Lumpur, Malaya. (Rubber Research Inst. of Malaya, P.O. Box 150, Kuala Lumpur)

27–29. Relative Humidity and Paper Test Methods, symp., Grand Rapids, Mich. (Technical Assoc. of the Pulp and Paper Industry, 155 E. 44 St., New York 17)

27–30. American Rocket Soc.—Power Systems Conf., Santa Monica, Calif. (R. L. Hohl, ARS, 500 Fifth Ave., New York 36)

27–30. American Roentgen Ray Soc., Atlantic City, N.J. (N. Jones, 20 N. Wacker Dr., Chicago 6, Ill.)

27–30. Association of Iron and Steel Engineers, annual conv., Cleveland, Ohio. (T. J. Ess, AISE, 1010 Empire Bldg., Pittsburgh, 22, Pa.)

27–30. Medical Photography, 1st intern. cong., Cologne, Germany. (Deutsche Gesellschaft für Photographie e.V., Köln, Nurmarkt 49, Germany)

28–1. International Soc. of Audiology, 5th cong., Bonn., Germany. (Prof. Langenbeck, Baumschuallee 29, Bonn)

28–5. Pan-Pacific Surgical Assoc., 8th cong., Honolulu, Hawaii. (F. J. Pinkerton, 230 Alexander Young Bldg., Honolulu 13)

30–1. American College of Radiology, Atlantic City, N.J. (F. H. Squire, Presbyterian-St. Luke's Hospital, 1753 W. Congress St., Chicago 12, Ill.)

30–1. American Medical Writer's Assoc., Chicago, Ill. (H. Swanberg, 510 Maine St., Quincy, Ill.)

October

1–2. Enzymes in the Manufacture, Storage and Distribution of Food, symp., London, England. (Society of Chemical Industry, 14 Belgrave Sq., London (S.W.1)