edited in retrospect

the changing scene

• Since the January issue reached you, a great deal has happened on the Georgia Tech campus. Almost all of this flurry in activity stemmed from one tragic incident, the sudden death on January 23 of Dr. Blake R. Van Leer, the Institute's fifth president.

Appointed as acting president on February 8 by the Board of Regents of the Georgia University System was Dean of Faculties Paul Weber, who under the Tech statutes had headed Tech in the period between the death of Dr. Blake Van Leer and the action by the Regents. Dr. Weber, a former assistant director of the Engineering Experiment Station, has been associate editor of this magazine for over six years. Because of the press of his new duties, he has resigned from his editorial work. But Dr. Weber hasn't completely left the magazine. On page four of this issue he discusses Dr. Van Leer's contributions to research at Georgia Tech. He also mentions the fact that Governor Griffin has made $300,000 available to Georgia Tech at the suggestion of the Board of Regents. The money is for an expansion of Tech's nuclear science program including starting a graduate level program in nuclear science and building the proposed radioisotopes laboratory.

the cautious search

• Selecting a new president for Georgia Tech is the responsibility of the Board of Regents. From all indications they will be cautious and conservative in their search. Tech's faculty and alumni have been asked by the Regents to aid in this task. At the Regent's invitation, a three-man faculty-alumni committee has been set up to consider prospective appointees for the presidency, as well as to co-operate with the Board's educational committee in the selection of Tech's new president. Selected by the faculty as their representative on the committee was Dean of Engineering Jesse W. Mason. President Walter M. Mitchell of the Georgia Tech foundation and President Frederick G. Storey of the Georgia Tech Alumni Association are the alumni members of the committee. Any recommendations for the presidency of Georgia Tech should be forwarded to the Committee secretary, Mr. Roane Beard, executive secretary of the Georgia Tech National Alumni Association.

the research engineer

JULY, 1956
Georgia Tech Engineering Experiment Station

selecting basic research problems — page 4

contents

the cover

Dr. Erling Grovenstein, associate professor of chemistry at Georgia Tech, is the tenth recipient of the annual Sigma Xi research award for the best scientific paper of the year, 1955-56, by a faculty member. Dr. Grovenstein, a 1944 graduate of Georgia Tech, received the award and a $300 stipend at the annual Sigma Xi dinner on June 5. At this dinner, as is the custom, the winner also presented the 1956 Sigma Xi Research Award Lecture, "Some Factors in the Choice of Basic Research Problems in Science." You'll find his thoughts beginning on page 4 of this issue. **Photo by Cecil Allen, Engineering Experiment Station Staff.**

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The Year of The Computers

THE COLLEGE YEAR, 1955-56, could well be called "The Year of the Computers" here at Georgia Tech. In December of 1955, the Rich Electronic Computer Center was officially dedicated. And, in this issue, one of our young research engineers reports the recent acquisition of an analog computer facility by the Engineering Experiment Station. Here on this page, we have the pleasure of announcing that the computer center has added an IBM 650 Magnetic Drum Data Processing Machine and many items of punched card accessory equipment to its facilities.

The IBM 650, a medium-scale, high-speed computer, is fed by punched cards and can be programmed and operated by one person. It utilizes the modern stored-program principle and features an internal memory capacity of 20,000 decimal numbers. It carries out between 400 and 800 arithmetical and logical operations per second.

At Georgia Tech, the 650 will be used to give additional laboratory experience to graduate and undergraduate students in conjunction with our many courses on electronic computer theory and operation (Nine courses in scientific computer theory in the School of Mathematics and seven courses in Data Processing in the School of Industrial Engineering are now offered to Georgia Tech students). The new computer will also be used for basic and applied research by Tech's faculty members, scientists and engineers.

The 650 system was leased to Georgia Tech under the International Business Machine Corporation's educational plan. Under this plan, IBM contributes a portion of the normal lease rate to the college to encourage the teaching of courses in the theory and operation of high-speed digital computers as well as to encourage basic academic research. This new system should complement the UNIVAC Scientific (ERA 1101) that Remington Rand made available to Georgia Tech last fall prior to the opening of the new computer center.

Some factors in the choice of basic research problems in science

by Erling Grovenstein, Jr.
Associate Professor of Chemistry
Georgia Institute of Technology

Artwork—John S. McKenzie

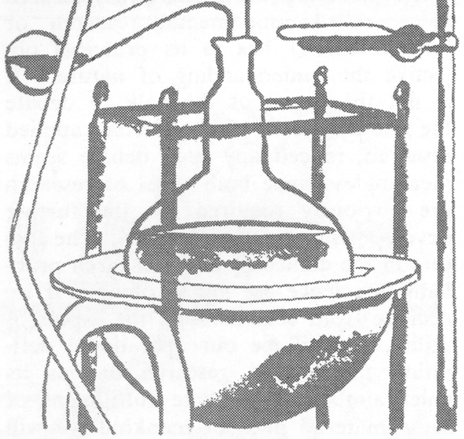
THERE HAS BEEN a remarkable change in attitude of the American public toward science and scientists in the twentieth century. This subject has recently been surveyed by James B. Conant[1]. Some illustrative stories might be of interest. At the time of our entry into World War I, "a representative of the American Chemical Society called on the Secretary of War, Newton Baker, and offered the service of the chemists in the conflict. He was thanked and asked to come back the next day. On so doing, he was told by the Secretary of War that while he appreciated the offer of the chemists, he found that it was unnecessary as he had looked into the matter and found the War Department already had *a* chemist."

Now this attitude should not be thought to apply to chemists only. Conant further states: "In World War I, President Wilson appointed a consulting board to assist the Navy. Thomas Edison was the chairman; his appointment was widely acclaimed by the press—the best brains would now be available for the application of science to naval problems. The solitary physicist on the board owed his appointment to the fact that Edison in choosing his fellow board members had said to the President, 'We might have one mathematical fellow in case we have to calculate something out.'"

This preference of the nineteenth century and early twentieth century public and business man for the inventor rather than the scientist seems rather natural. Was not the inventor the man who had changed our habits and made possible our new comforts? In other words, had not the inventor conquered nature and put it to man's use? Furthermore, the role of the inventor extends back into the dim pages of history. He had discovered the use of fire, made the first stone weapons, extracted metals from ores. His story parallels the development of the practical or empirical arts. The scientist, on the other hand, is a comparative newcomer to history—he is first prominent in the sixteenth century (although he had an ancient prototype in Greek civilization). He is supposed to be concerned with discovering nature's laws. Originally science had little effect upon invention; indeed it was the other way around. Invention had a profound effect upon science. As scientific knowledge accumulated, the scientists were slowly able to make the empirical arts seem less empirical. Thus it was not until about the time of the American Revolution that we had anything like a reasonable explanation of combustion or of the rudiments of metallurgy. In the latter half of the nineteenth century, chiefly in Germany, organic chemists introduced synthetic dyes and drugs to the world. By the twentieth century, science had developed so rapidly that in many of the areas of the practical arts, inventions were being put forth by scientists. As Conant has said, the striking social phenomenon of our times is the scientist turned inventor.

This type of scientist we call an applied scientist or engineer. Now the basic factor in the choice of a research problem in applied science is fairly obvious, namely the research should lead to the development of a new material, commodity, tool, weapon, or gadget, in other words, applied science aims at the fulfillment of the material needs of society. On the other hand, basic research (also called fundamental research or pure research) has as its principal objective the understanding of nature. It is not the intent of this talk to debate the relative merits of basic versus applied research; indeed any such debate seems meaningless since both types of research are obviously required for the further development of our civilization. The factors in the choice of basic research problems in science are not so obvious if we exclude from consideration the aspect of utility. It will be our operational definition that if the research has as its chief aim the immediate fulfillment of some material need of mankind we will term the research, *applied*. Now it is surely hoped that basic research will ultimately prove useful in some manner in satisfying material, artistic, spiritual, psychological, or intellectual needs; but man has found that it is useful not to let immediate aspects of utility dominate the choice of all research problems. This is the explanation of the cartoonist's misconception—a picture of a long-haired scientist exclaiming that at last he has made a discovery that should not prove of use to anyone.

WHAT FACTORS other than the aspect of utility can guide the choice of basic research problems? Are they to be chosen by pure caprice? This is an important problem. E. Bright Wilson[2] has noted: "Many scientists owe their greatness not to their skill in solving problems but to their wisdom in choosing them." There is a common fallacy that if you are dealing with scientific matters, judgment of values rarely, if ever, enters in. Facts speak for themselves in science, we are often told. But in the selection of a research problem, there frequently arises the question of what facts are most worthy of being collected. The number of possible facts must be practically infinite. That a choice must be made is incontestable. While the scientist discovers one fact, millions upon millions of facts occur in a cubic millimeter of his body. To put all the facts of nature into



Sigma XI—cont.

science would be to put the whole into the part. As the great French mathematician Poincare[3] has said, "scientists believe there is a hierarchy of facts and that among them a judicious choice may be made." The most interesting facts are those which can serve many times; these are the facts which have a chance of coming up again. We are fortunate to have been born in a world where there are such. A familiar example to the chemist is the approximately one hundred elements which make up the composition of all known substances. In what chaos the chemist would find himself if there were instead one hundred million elements! Biologists would be just as much confused if there were only individuals and no species and if heredity did not make sons like their fathers. The facts which are most often recurring are the simple facts, and indeed these appear to us simple precisely because we are used to them.

C. A. Coulson[4], the mathematical chemist, has pointed out that "... though facts are the raw material of science they do not constitute its glory." Lord Rutherford was accustomed to refer to those scientists who were content to gather facts as "stamp collectors." Surely the unordered collecting of facts is of no more value than the collecting of stamps. Science is primarily concerned with evolving conceptual schemes, grand hypotheses, theories, or laws of nature based upon the known facts.

Alfred North Whitehead[5] has stated: "It is this union of passionate interest in the detailed facts with equal devotion to abstract generalization which forms the novelty in our present society." The Greeks were over-theoretical. He states further that "there can be no living science unless there is a widespread instinctive conviction in the existence of an *Order of Things*, and, in particular of an *Order of Nature*." He emphasizes the word "instinctive" since some scientists claim not to believe in such order. He suggests that this conviction "must come from the medieval insistence on the rationality of God, conceived as with the personal energy of Jehovah and with the rationality of a Greek philosopher."

A PRIMARY FACTOR in the choice of a basic research problem in science, then, is that the research should lead to the development of a new theory, law, or conceptual scheme. The celebrated Viennese philosopher Ernest Mach has said that the part of science is to effect economy of thought just as a machine effects economy of effort. The facts which give a large return are those which take their place in a very general law, because they enable us to foresee a very large number of other facts. Frequently, a new fact will serve to unite elements long since known, but till then scattered and seemingly foreign to each other, and suddenly introduce order in the form of a new theory or generalization where the appear of disorder reigned.

E. Bright Wilson[6] has pointed out that, where possible, it is usually best from the beginning to undertake experiments which are designed to test well-thought-out hypotheses. Experiments for experiment's sake are much less likely to lead anywhere because when an hypothesis arises its test may well require data taken under somewhat different conditions from those used.

TO SUMMARIZE what has been said concerning the choice of fundamental research problems, the problem should lead to the collection of basic facts and to their correlation into theories or conceptual schemes. Conant[7] has said "The history of science demonstrates beyond a doubt that the really revolutionary and signifficent advances come not from empiricism but from new theories." He[8] further points out that the test of a new theory is "not only its success in correlating the then-known facts but much more its success or failure in stimulating further experimentation or observation which in turn is fruitful."

After a theory is well established, after it is beyond reasonable doubt, the facts in full conformity with it are before long without interest to the scientist concerned with basic research, because they no longer teach anything new. It is then the exceptions which become important. Among the exceptions, the ones chosen for study first will be the most accentuated, not only because they are the most striking, but because their study will be the most instructive. New concepts may result from a consideration of difficulties inherent in an old theory.

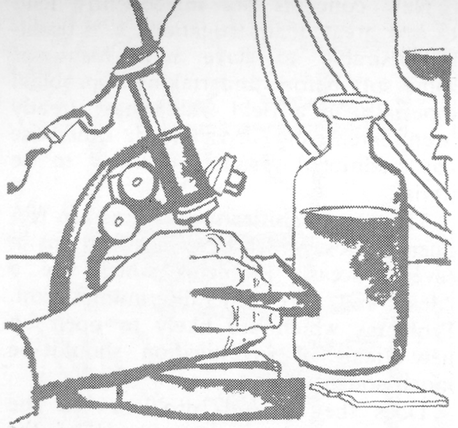
THE INVESTIGATION of a really new area of research is nearly always profitable. Unexpected results can generally be relied upon under these circumstances. In view of the vastness of nature, the exploration should be directed to cover as wide an area as feasible with a later follow-up of any points of special interest. New experimental techniques, thus new instruments in their day such as the telescope, the microscope, the cyclotron, the atomic pile . . . open up immense new fields for investigation. New techniques may evolve gradually as improvements in apparatus and method. When a certain degree of accuracy or convenience is attained, significant new observations may be possible. New degrees of accuracy in measurment often, but by no means always, bring to light unexpected results. New techniques of experimentation may lead to revolutionary results. Far too often, however, projects are undertaken solely as a matter of experimental convenience.

New concepts not infrequently lead to new areas of investigation. It is usually desirable to have new ideas of some sort before undertaking a problem especially in a field which has already been extensively investigated; otherwise any additional results are likely to be trivial.

The most significant problems are frequently those which have ramifications in several areas. Problems which are a "dead end" are generally unimportant. Problems which are likely to open up new fields of investigation should be much sought after.

Does the research problem "fit the times?" If it is too far ahead of the times it will likely not be fruitful. Research techniques may not be well enough developed to make a study of the problem profitable at a given date. The current state of science may not provide a background for understanding the problem.

Is the field one in which known information is published freely? In particular is the field censored for military security reasons? It is difficult for scientists outside the walls of security clearance to work in regions which are highly restricted. To the public at large this might seem a small price to pay for keeping our military scientific secrets from a possible enemy. The difficulty is, however, in part that many top-rate scientists in peacetime do not wish to work for the government especially if their work will not be published. Further there is the danger that secrecy will be a shield for incompetence among those working for the government. Probably the highest cost of secrecy to science lies in our failure to make use of the immense potential of talent at home and abroad among those scientists who do not have access to classified information and who are in many real respects effectively barred from making contributions in classified fields. We must not forget that all of the basic scientific information which led the way to the development of the atomic bomb was imported from Europe. Conant[9] has said: "Secrecy and



Sigma XI—cont.

science are fundamentally antithetic propositions." The development of scientific theories in the past has depended on free discussion of their consequences. While secrecy obviously limits discussion, it is doubtful that secrecy can keep scientific information from an intelligent possible enemy as was brought out by the recent Geneva Atoms for Peace Conference.

Possibly the most important factor in the choice of a research problem is that it should interest the investigator strongly. As E. Bright Wilson[10] has said: "Scientific research, not being a routine process by requiring originality and creative thought is very sensitive to the psychological state of the scientist." An uninterested worker is unlikely to have the necessary new ideas or the drive to complete a research problem. The research problem should, moreover, be within the worker's capabilities. In particular, the worker should possess or be willing to acquire the special capabilities required for carrying out the research.

While many scientists may owe their greatness to their skill in choosing research problems, the significance of a research problem often depends on the peculiar quality of the imagination and creative ability which the investigator brings to its prosecution. The great scientist must be regarded as a creative artist and it is misleading to think of the scientist as a man who merely follows rules of logic and experiment. Einstein has said: "There is no logical way to the discovery of elemental laws. There is only the way of intuition, which is helped by a feeling for the order lying behind the appearances."

We have discussed factors in the choice of a research problem as though there were a number of problems to choose from. Wilson[11] has said: "A research worker in pure science who does not have at all times more problems he would like to solve than he has time and means to investigate . . . probably is in the wrong business."

It is true, in spite of all the foregoing discussion that probably the most important research problems are not chosen but are discovered almost, as it were, by chance. It is noteworthy that such scientific discoveries are made only by highly qualified observers who painstakingly follow up initial chance clues; as Pasteur said, "Chance only favors prepared minds." As example of such discoveries we might cite: the preparation of the first coal-tar dye by William Henry Perkin (who was looking for a synthesis of quinine), the discovery of radioactivity by Antonie Becquerel, the discovery of penicillin by Alexander Fleming. All of these scientists were carrying out more or less "normal" programs of fundamental research when they made the discoveries for which they are now famous. These illustrious discoveries were in no sense "planned" in their initial phases. It is the essence of the unknown that nobody knows what is to be known, much less where to look for the unknown. Yet it is upon the discovery of new fields of inquiry such as these that the future growth and development of science depends. The history of science shows that, by and large, most such fundamental discoveries have been made by "uncommitted" investigators, that is to say, investigators who were free to follow any clue, interesting observation or idea, regardless of what direction the research might lead them. Their investigations were not confined to the narrow limits of some preconceived research program.

As a final factor in the choice of basic research problems in science, we will consider the economic aspect. In the Seventeenth and Eighteenth Centuries most scientific research was conducted by "amateurs," that is to say by men who had their income from other sources or endeavors. Some like Robert Boyle (the son of the Great Earl of Cork, an Englishman who made his fortune by exploiting Ireland) inherited wealth. Boyle, who was his own patron, pointed out that the advancement of experimental philosophy "requires as well a purse as a brain." Others such as Benjamin Franklin and Antonie L. Lavoisier earned their wealth in other professions (Lavoisier was guillotined primarily for his activities as tax collector under the royal regime). In the Nineteenth and Twentieth Centuries, basic research in science has been cultivated primarily in universities and to some extent in research institutes (e.g., the Kaiser Wilhelm Institute in Berlin). There is no indication that the cost of scientific experiments has in any way lessened since Boyle's day (the latter half of the 17th Century). Clearly a pertinent question facing most investigators is whether or not a scientific problem is within attainable financial limits.

Frequently, the solution to this difficulty is to try to convince someone that the research problem is worthwhile. It is at this stage that the difference between applied research and basic research becomes most evident. Nearly always it is easier to get support for research leading to a useful new material, commodity, tool, weapon, or gadget than to support research which aims merely at understanding nature. This is especially true since only some rather vague general principles, such as we have mentioned, which are difficult to apply in practice, can be used to evaluate basic research problems. Joel Hildebrand[12] has said that the judgment of a scientist about problems he does not feel impelled to attack cannot in his opinion be very significant. Surely the judgment of nonscientists is even less significant. Finally, indeed, many of the most valuable basic research problems cannot be formulated because they have not been discovered. It is, therefore, especially hard to get funds to support the uncommitted investigator—the one who might be another Michael Faraday, Louis Pasteur, Antoine Henri Becquerel.

In supporting basic research, the emphasis should be placed upon supporting the right man rather than the right project. The investigator should be encouraged to work upon any problem which seriously interests him. The principle involved here has been formulated by the mathematician H. B. Phillips[13], "When the proper course is known, action can be directed by rule or law. But when the proper course is not known, each individual should be free to go his own way to provide the greatest diversity of action and therefore the greatest probability that somebody will be right." Herein lies our greatest hope in our scientific competition with Soviet Russia.

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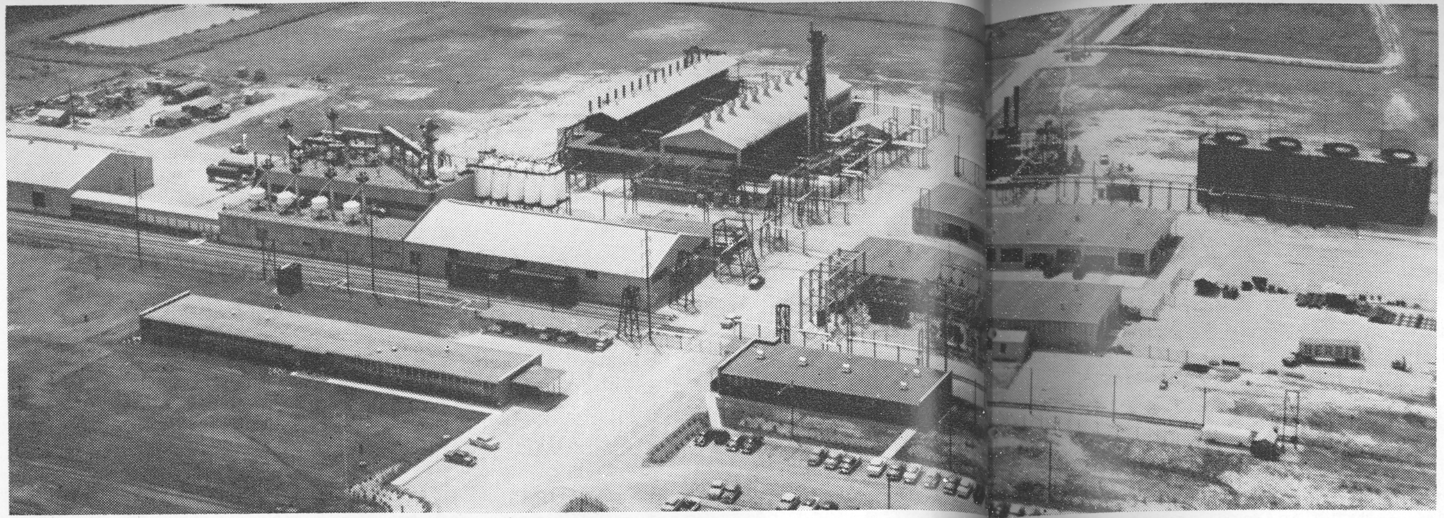


The Spencer Chemical Company's forty-five million pound a year polyethylene plant in Orange, Texas. Building of this plant was a direct result of the company's early market research in possible uses of a gas containing a high percentage of ethane polyethylene.

by Robert J. Kyle
Head, Technical Information Services

Management decisions based on analysis of facts, not on hunches or intuition, call for the latest in

INDUSTRIAL MARKET RESEARCH

INDUSTRY MUST OWN A MARKET, or it has no advantage in owning a production plant.

In order to win a market, an industry must offer the desired product to the proper people at the right time. Industrial market research offers the only feasible technique for evaluating these factors. It provides a tool for determining the proper balance between quality and price and for answering many other perplexing industrial problems. In short, it offers industry, large and small, a major device for increasing earnings on the companies' investments.

Most of our economists agree that our prospects for continued prosperity are good. As our population increases to 220 million or more in the next 20 years, industry will have greatly expanded markets, even if our standard of living should not continue to improve. Our productive capacity may even have difficulty keeping pace. Nevertheless, the competition will continue to be keen in most fields. Furthermore, the increasing cost of production and scarcity of accomplished manpower make correct management decisions more and more important.

Businessmen who do not have market research facilities available must evaluate their proposals in terms of past experience and estimate data for the future. Over much of the world this approach is still the very best that can be done, and even in the United States relatively few companies use truly systematic programs for evaluating their future.

But gradually, the old philosophies are changing. It has become increasingly important that management's decisions be sound ones: decisions not influenced by hunches or "intuitions," decisions based on the analysis of facts. Market research, the study and evaluation of markets and the many factors which control them, is intended to aid management in evaluating the marketability of its product. Thus, the organization which makes full use of industrial market research can plan with a realistic insight into the future.

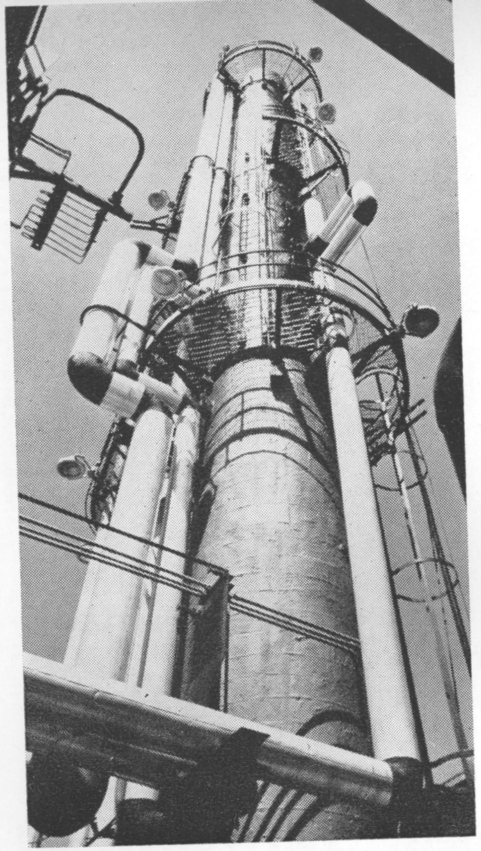
Market research can profitably be used in almost every phase of industrial operation. The following questions indicate the problems with which market research can help industry:

1. What products can be sold? 2. What quantity and quality will be needed? 3. How does price influence the sales of the product? 4. What competition will be met? 5. Where will the markets be? 6. How long will the market be open? 7. What additional uses can be found for the existing products? 8. How can the user best be reached? 9 What are the good and the bad features of the proposed product? 10. What inventory should be maintained? 11. What should the sales quotas be? 12. How much should be spent on advertising?

SALES

There are several more-or-less independent ways in which market research assists a company's sales program.

Practically all market research groups do a considerable amount of forecasting of expected sales. Forecasts are based on the overall trends of sales for the company's products as shown by past history. In some cases, the trends must be established from a basic understanding of the way in which the product will be used and from an estimation of the demands for the new product. Suitable allowances are made for technological and economic changes which can be anticipated and for known changes which will occur in the competition. In addition to the modified trends, many market research groups attempt to predict cycle fluctuations in the demand for the company's product. Such cyclic changes are



One of the ethylene purification towers at Spencer's Orange, Texas, polyethylene plant.

influenced by the season of the year, cyclical variation in the overall economic status of the country, and many other factors.

These forecasts are used for establishing realistic sales goals which are an integral part of most companies' sales programs. They are also used to evaluate the effectiveness of the sales program. For many industries, sufficient data is published to permit an appraisal of any company's sales in various geographical areas with the total sales of similar products in these areas. Whether the data is taken from published statistics (usually from Government or trade-association sources) or from sales forecasts, it is pos-

sales emphasis is necessary.

The market research group is often called upon to find new markets for old products when a company has excess productive capacity, or it desires to increase its manufacturing level.

At times, when a company begins work with a new product or when distribution is being undertaken in a new area, it is necessary to appraise the various possible distribution systems. In some cases, the appraisal of the distribution system also involves a study of the pricing structure followed by competitors.

The evaluation of sales effectiveness may be carried out by direct contact with customers, potential customers, distributors, installation men or others who are familiar with the company's product. Surveys of this type may become very detailed as illustrated by the following example:

As part of a development program (not a sales project) the Maytag Company decided to determine motivations for the purchase of automatic washing machines. The psychologist who was called in to do this special job reported that housewives tend to have a feeling of guilt regarding the many labor-saving devices they have. Usually the purchase of an automatic washer is urged by the husband. Women, who are usually proud of their role as housewives and conscious of their duties, and particularly of their obligations to be efficient and budget-minded home-makers, feel that automatic washers are an extravagance. They feel that wringer-type machines wash just as well. Consequently, many women who could afford washers take the attitude, "Well with all of the other labor and time-saving devices, I'm just a little afraid my husband thinks I've got it too easy the way it is." Because of housewives' feeling of extravagance in purchasing automatic washers, some "face-saving" economy measure is desirable. Apparently either a water-level control for partial loads or a suds-saver serves this psychological need.

ent from most of the market research discussed in this paper in that it is an evaluation of consumer attitude. But, the example isn't even necessary to see that market research is a valuable tool for the sales manager to use in planning an effective program. In fact, in a highly competitive industry, the sales department with the best fact-finding operation will most likely be the one to get the most business.

Production

As a guide to manufacturing, the forecast for the sales department often supplies facts indicating seasonal variations and general trends which aid in scheduling production and controlling inventory. This type of assistance can be exemplified by supposing that a company is in a period of slow business. The sales department would normally try every possible means of obtaining additional orders. However, if market research data predicted that several months in the future, sales for the given product would increase sharply, the production department could increase the manufacturing rate in order to build up a good inventory, and, in the meantime, the sales department could concentrate on products which were momentarily more profitable.

Product Development

Perhaps the greatest pay-off of market research in industry is in product development. The evaluation of a new product's potential is one of the most difficult and important operations in any new or expanding business. An excellent example of the use of market research in development programs has been given by the Spencer Chemical Company of Kansas City, Missouri. In 1951, this company undertook a general economic appraisal of possible uses of a gas containing a high percentage of ethane. They discovered that the most attractive product was polyethylene. However, the Imperial Chemical Industries of England held patents covering the commercial production of polyethylene and had

Union Carbide and Carbon. Hence, Spencer turned their activities to the production of ethylene glycol. However, in 1952, an antitrust ruling required Imperial to license their patents to other American companies. Because of the preliminary survey, Spencer was able to open negotiations immediately with Imperial. It was the first of five U. S. companies to obtain a license. Because of the anticipated competition from the other four newly-licensed companies, Spencer purchased a technical and market study which had just been produced by a consulting firm. They used this study as the nucleus of a more detailed program conducted by their own market research group. Their team spent four months in research and made nearly 900 field interviews. They prepared a report which covered the product, the process details, the relationships of its properties to its uses, the names of process equipment manufacturers, methods of research and development, and market estimates. The survey gave Spencer Chemical Company the answer to questions about the most effective sales program and the location of the potential market. These studies led ultimately to a $14 million plant in Orange, Texas, with an annual capacity of 45 million pounds of polyethylene. The first commercial runs were made early in March, 1955.

Frequently, a manufacturing enterprise is requested to supply some special product for an individual company. To refuse such requests often might mean disregarding a highly profitable undertaking, but to accept them without a detailed appraisal is like asking for trouble. Thus, market research groups are frequently given the task of evaluating the potential of some customer's market in order to predict the stability and magnitude of future sales.

Research

Every applied research project should be preceded by a market research evaluation. Often, only a superficial study

Continued on next page

fully there would be no commercial value to the product. However, if superficial study shows promise, it is practical either to expand the market research to get more exact information or to initiate the research project.

It should be emphasized that there is a timeliness to market research reports as they affect both research and product development. It is quite possible for a company to be forced to abandon an expensive program with a promising product because the research and development work cannot be made to move rapidly enough. A favorable market research report today may be completely reversed in one or two years, or less.

Management Decision

As mentioned earlier, the data from market research reports are necessary for effective long-range planning. The long-range planning involves the forecast of business conditions in general and the forecast of conditions for the specific product of the company. After the forecasts are made, the specific objectives and means for carrying them out must be established. Finally, it is necessary to maintain a continuous review of all pertinent factors in order to readjust the objectives as conditions change.

The market research data provide an indication of the effectiveness of the advertising program, the fraction of the total market which is held by the company, the estimated sales which are to be expected in the future and related data which gives management a criteria for distribution of the available funds among the various departments.

The scheduling of plant expansion and product modification can also be based to a major extent upon market research data. For example, when manufacturers of flourescent tubes shifted from beryllium salts, which are toxic to human beings, to other phosphors, Mallinckrodt Chemical Works was in a position, within 60 days, to prepare samples and build a several-hundred-thousand-dollar-plant to supply the new chemicals. The com-

rapid switch in product to the fact that they had maintained a continuing review of this market and had the right information at the right time.

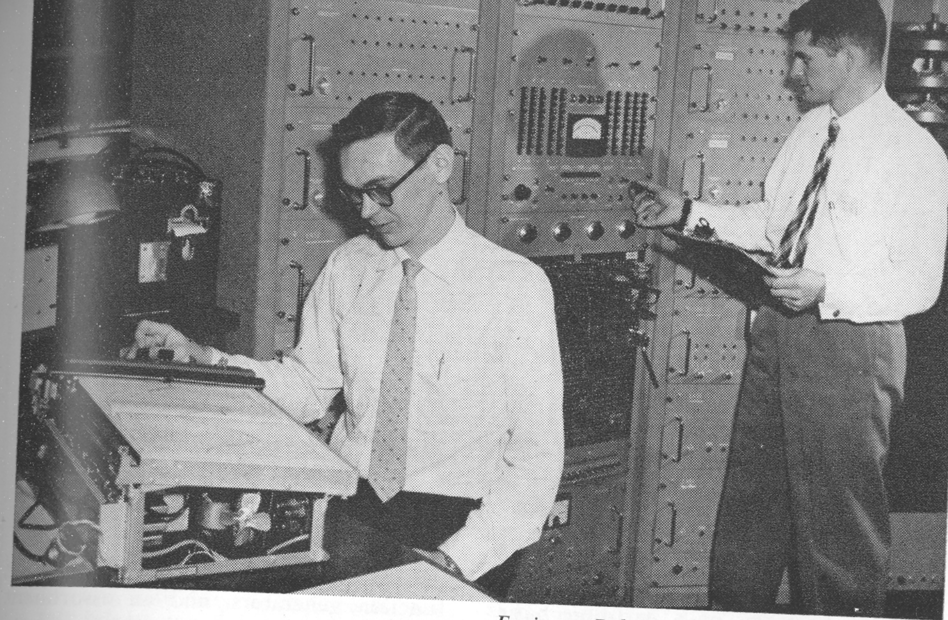
As companies expand it is often desirable for them to build entirely new production facilities. A preferable location for the new plant is determined by the availability of raw materials, labor, transportation, markets, and other factors. In many companies, the market research group is relied upon to supply the data necessary for these decisions.

Market Research Facilities

Most large companies maintain their own market research facilities. However, even these companies frequently find it desirable to pass on excessive workloads or specialized assignments to outside firms which specialize in offering such services. In general, small companies have too little need for market research facilities to justify a full-scale organization. Such companies usually rely on independent market research groups.

Market research services are available through private consultants and many institutions. At the present time, the Georgia Tech Engineering Experiment Station is expanding its staff and facilities for market research through its newly established Industrial Development Branch.

Market research had its origin in Gallup-poll-type political surveys in the early 1800's. These surveys were eventually broadened to include an appraisal of various advertising media through surveys of magazine readers, etc. The real start of market research began in the period between 1910 and 1920, and most of the development of present techniques has occurred since 1930. Today it represents a science which often calls for advanced techniques in sampling, data collection, and mathematical interpretation. Along with these advances in techniques the subject has graduated from the category of a curiosity and luxury to that of a real necessity for any progressive expanding business.



Engineers Bob Johnson, front at plotting board, and M. David Prince, operate the newest station research tool, the analog computer.

THE ANALOG COMPUTER LABORATORY

A versatile new research facility

by M. David Prince, research engineer and T. K. Wright, research assistant

THE NEED FOR ANALOG computing equipment as part of the research facilities of the Georgia Tech Engineering Experiment Station became apparent during the summer of 1954. This need was further emphasized during the following year when one of our large research projects had to purchase six months of computer time from a government-owned analog center in New York City, while another project had to use a military computer at Rome, New York. All indications were that the need would continue in existing programs and probably increase with expected new research. This need, coupled with the many educational advantages that would accrue to Georgia Tech through the addition of this facility to complement its large digital computer center, made the acquisition of analog computing equipment imperative.

During the late summer of 1955, funds were allocated for this new facility. Simultaneously, a study of commercial equipment was carried out, and the most suitable combination of computing elements was selected for the laboratory. The equipment, purchased from the Berkeley Division of Beckman Instruments, Inc., was delivered here in January of 1956. The laboratory was immediately established in the Defense Branch of the Station's Physical Sciences Division where the principal projects requiring its use were located. At the present time the laboratory's operating, programming, and maintenance staff consists of five full-time engineers and

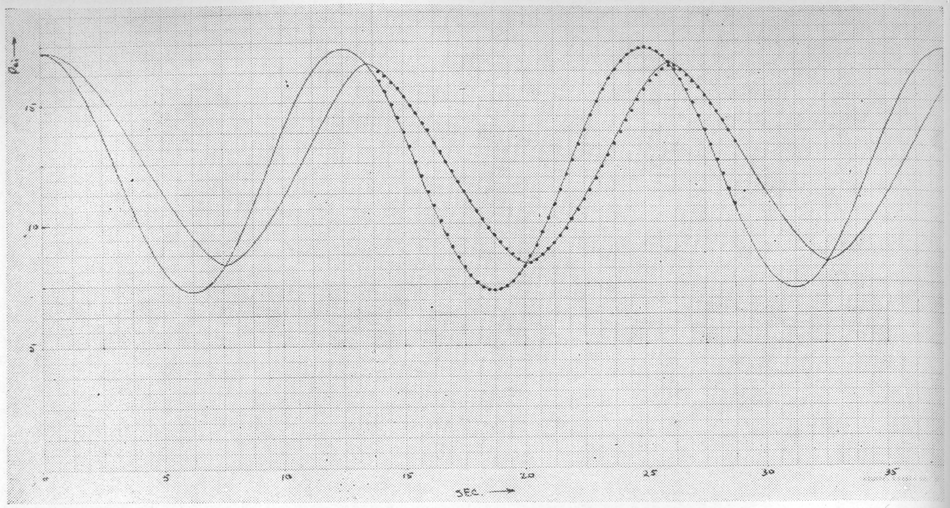


FIGURE 1—COMPARISON BETWEEN ANALOG AND DIGITAL SOLUTION OF NON-LINEAR EQUATION

assistants, all of whom are pursuing their graduate programs on a part-time basis. Three of these men graduated from Georgia Tech and joined the Station staff this year.

Digital and Analog Computers

In considering the relative roles of digital and analog machines for engineering computation, we see that digital computers are employed for two principal reasons: first, accuracy is attainable to any degree that might reasonably be desired; and second, they are adaptable to almost all types of numerical problems, including those of an accounting and business nature.

On the other hand, analog computers are somewhat restricted in accuracy. Although the accuracy of each component is 0.1 percent, errors of about one percent will accumulate in solutions of moderate difficulty (using about 100 computing elements). However, this precision is usually adequate, as demonstrated by the comparison between digital and analog results shown in Figure 1. In this figure, a solution of the type shown in Figure 2 was plotted and compared with the precise point-by-point solution computed by the Rich Electronic Computer Center on the ERA 1101. The analog solution of this problem required 28 amplifiers, 2 multipliers, 2 function generators, and an assortment of minor elements.

Unlike digital machines, analog computers cannot be used for payroll, inventory and other accounting-type computation, but are best suited for physical problems in which the quantities are continuous in nature. However, for this type of problem, they may be ten or even a hundred times as fast as large digital computers.

Operations and Computing Components

An analog system is sometimes called a simulator, sometimes called a differential analyzer, and sometimes an analog computer. It is all of these, since modern general-purpose analog equipment is readily employed for physical simulation (in "real time") in which parts of a missile system may actually be wired into the machine, or for use as a differential analyzer to solve differential equations. However, its use is not restricted to these applications—it may also be employed as a more general computer for solving linear algebraic equations, double integrals, geometric problems, and many other mathematical types.

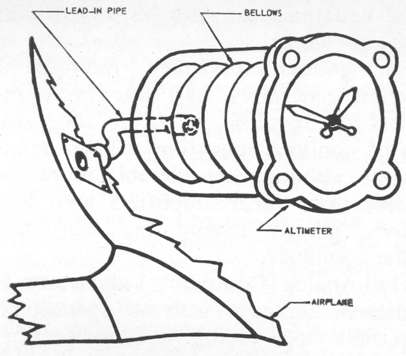
The simplest analog operations are those of multiplying by a constant, adding and integrating. These basic op-

A TYPICAL ANALOG COMPUTER PROBLEM

A jet pilot is flying at 40,000 feet near supersonic speed. He sights an enemy aircraft below and dives in pursuit. Passing through a cloud layer, he directs his attention to the altimeter so that he can level off under the cloud ceiling at about 2,000 feet. When the instrument shows an altitude of 10,000 feet he begins to pull out of the dive and level off . . . but seconds later there is a blinding crash as he plows into the ground. The aircraft is demolished and the pilot is killed. . . . What caused the crash? Was it a human error in instrument reading? Did the altimeter needle stick and fail to indicate the change in altitude? Was some new problem, not considered prior to today's high-speed aircraft, encountered?

One new problem, now receiving attention, may explain the crash. It has been found that a serious error in altimeter indication is caused when an aircraft dives at high speed. Investigation of this error, due to a time delay in the altimeter pressure bellows, was the first task undertaken by the Analog Computer Laboratory of the Engineering Experiment Station.

A total of 130 solutions was computed for a range of pipe sizes, pressure amplitudes, and pressure frequency variations. (This work arose through a contract between Sandia Corporation and Dr. A. L. Ducoffe of Georgia Tech's School of Aeronautical Engineering. *A generalization of this problem is the basis of a Master's Thesis by Mr. George Simitses, A. E. graduate student.*) Typical results are seen in Figure 2, which shows the pressure inside the altimeter bellows due to the aircraft diving and climbing periodically (a sine-wave variation of outside pressure). The left-most curve represents the pressure outside the aircraft. And the other curves, lagging behind the forcing function, apply for various sizes of the pipe which joins the pressure chamber to the atmosphere. This time-lag error is caused by the rather small diameter of the pipe, which delays the air rushing into the bellows chamber. For the smallest pipe size, the arrows on the figure indicate a difference in pressure of 1.6 pounds/sq. inch between the atmospheric pressure and the bellows pressure. Since a change of one pound/sq. inch corresponds to an altitude change of about 2,000 feet, the importance of this error readily becomes apparent.



A cutaway sketch of a barometric altimeter showing the bellows and lead-in pipe.

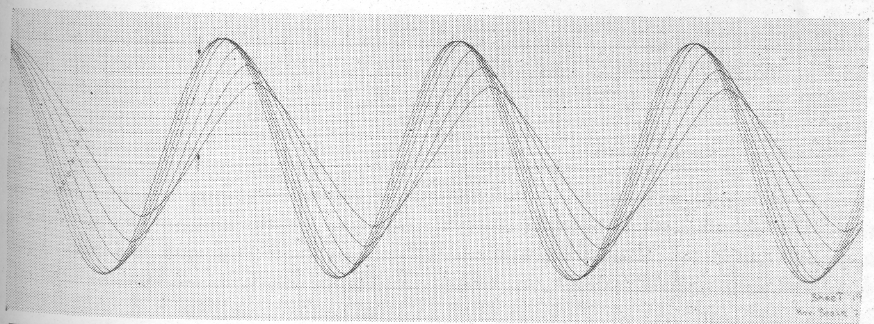


Figure 2—apparent altimeter pressures for different lead-in pipe sizes. The sine-wave variation of outside pressure is simulated by the curve of largest amplitude.

[...] differential equations with constant coefficients, the type so commonly encountered in electrical and mechanical engineering. Simultaneous linear differential equations can also be solved with these operations.

For greater versatility, multipliers, function generators, and sine-cosine resolvers are required to permit the solution of nonlinear equations. These components, although considerably more expensive than linear amplifiers and integrators, greatly extend the usefulness of the computer.

The Analog Computer Laboratory is at present equipped with 40 operational amplifiers for adding and integrating, 34 potentiometers for multiplying by constants, two electronic multipliers, and two electronic function generators. Four interchangable patchboards are available so that several problem set-ups can be retained and each placed on the machine as scheduling time permits. Also, three servo-resolvers of advanced design are scheduled for delivery in September as part of the original equipment order. In addition, two more multipliers, two function generators and three servo resolver-multipliers are on trial loan from the manufacturer. This computer is supplemented by a 10"x15" X-Y plotting board which produces curves similar to those shown in Figure 2. The necessary control cabinets, power supplies, air conditioning equipment, and a line voltage regulator complete the Laboratory.

Applications of Analog Computers

The example shown on page 17, involving a physical situation in which engineering accuracies are adequate, is typical of the problems especially suitable for solution on an analog computer. It illustrates the usefulness of plotted solutions, and demonstrates the simplicity with which changes of the system design can be investigated. This capability is one of the prime advantages of the analog computer because it provides the engineer with a "feel" for his problem that can be obtained in perhaps no other way. Each intermediate variable can be plotted or observed on a meter, thus providing an unusual insight into the problem solution. Parameters can be varied by the turn of a knob to demonstrate their effect on the final results. Furthermore, the language of the analog computer is the language of the electrical engineer—"voltage," "transient response," "step function," "feedback," etc., so that an engineer with a good background in the mathematics of his field can quickly become conversant with its use. For these reasons, analog computers are used extensively to study engineering problems such as aircraft and mechanical vibration, aircraft flight simulation, trajectory studies, aerodynamic, fluid flow and heat flow problems, servo-mechanism design, and chemical process controls.

Plans for the Future

Since the Laboratory is newly established it is still in a state of expansion. Need has been evidenced for ten more amplifiers, four additional electronic function generators, four additional electronic multipliers, and some special test equipment. When these needs are filled, the Laboratory will be one of the most modern in the South, capable of solving an impressive variety of problems.

In preparing for the future, we are looking forward to a balanced three-point program. The first responsibility is to supply an analog computing service for the Station projects, for the academic departments, and when possible, for outside industry. Second, we should undertake a program of applications research, in which we study new and better ways to apply our computer to practical problems. (The Laboratory already has one commercial project to pursue this objective.) Finally, a program of equipment design and development is required so that we might improve the existing equipment and develop new components. In this way we may give expression to the creative ability of the young engineers who will staff the Laboratory. Through this parallel three-part program the maximum benefit will be provided to Georgia Tech, and to the Station and its staff members.

Ingols, Robert S., and George M. Jacobs, "Variations in the Chemical Character of Small Streams." Reprinted from the Bulletin of the Georgia Academy of Science, Vol. XIII, pp. 94-100. Reprint 95. Twenty-five cents.

Because the U. S. Geological Survey has shown extreme interest in the quantity of water produced by a large number of streams during a very dry spell, the Engineering Experiment Station has been conducting a survey of two small streams in the Yellow River Basin. This paper discusses the study of the quality of these streams and notes the variations which occur on a diurnal basis. Because of the large draft of transporation, the water in a flowing stream may be reduced to a trickle at the period of maximum activity; whereas, the same stream may have a three- or four-fold increase in flow at night causing variations in chemical character. It is the purpose of the paper to indicate these variations and show the basis for a continuing study which may determine the cause of the variations of the quality of the water.

Witt, Samuel N. Jr., "Transistorizing Meacham-Bridge Oscillators." Reprinted from Electronics, March, 1956. Reprint 97. Gratis.

Circuits have been designed using both point-contact and junction transistors. Operating at one megacycle, oscillators provide good long and short-time frequency stability. They are stable with respect to temperature and supply-voltage changes.

Orr, Clyde, Jr., Mendal T. Gordon and Margaret Kordecki, "The Density and Size of Air-borne Serratia Marcescens." Reprinted from the Journal of Bacteriology, Feb., 1956. Reprint 100. Gratis.

The density and radius of air-borne *Serratia* marcescens were determined as a function of relative humidity using Millikan's technique with which the value of the electron charge was established. Cell radius was found to decrease with decreasing relative humidity. Cell density increased as the relative humidity decreased until about 40 per cent relative humidity was reached. At lower relative humidities the apparent cell density also decreased.

Tooke, Raymond, Jr., "Physical Studies of Paint Systems Applied to Southern Yellow Pine." Reprinted from Official Digest, Vol. 22, No. 370, November 1955. Reprint 99. Twenty-five cents.

This paper received the American Paint Journal award as the best constituent club paper at the 1955 annual convention of the Federation of Paint and Varnish Production Clubs.

The exterior-house-paint exposure studies described in this paper represent a second series of investigations on this subject conducted at the Georgia Tech Engineering Experiment Station. The Station, as well as southern paint manufacturers, has been interested in the painting of southern yellow pine because this wood is one of the most difficult building materials to paint satisfactorily, and because it is extensively used for construction in the South. Emphasis has been directed toward a study of two-coat paint systems since this procedure has become predominant in current exterior-house-painting practice. The initial investigation reported in 1947 was directed toward a study of primers. The present work is intended to encompass the original findings with respect to primers into a comparative evaluation of a number of well-known primer and top-coat formulations together with selected variations.

Belser, Richard B., and James W. Johnson, "A Versatile High-temperature Infrared Oven." Reprinted from Ceramic Age, December 1955. Reprint 101. Twenty-five cents.

An economical, high-temperature, infrared oven was constructed from a one-liter beaker, lined with a reflecting material and a single 250-watt, infrared bulb. By operation of the oven, with the beaker inverted over the lamp, the placing of samples to be heated is facilitated. The oven may be powered by a photoflood lamp, an ultraviolet sun lamp. or one of the newer 1500-watt infrared lamps with a Vycor face. By using a lamp with a hole drilled in its face, the oven may be operated within a vacuum chamber. The heating efficiency is greater within the vacuum. A temperature of 750°C was reached within the oven with 135 volts across the lamp's filament. The oven has proven itself to be a useful laboratory tool by three year's active use.

These and other technical publications may be obtained, and the complete publications list requested, by writing Publications Services, Engineering Experiment Station, Georgia Institute of Technology, Atlanta 13, Georgia.

edited in retrospect

the changing scene

• Beginning with the fall term of 1956, Georgia Tech's expanded nuclear science program—first outlined for you in the January issue of this magazine—will officially get underway with the registration of the first students in the graduate program. The final green light for this program—approval for the granting of two new graduate degrees—was given by the Board of Regents, governing body of Georgia's University System, at their May meeting. The new degree designations are the Master of Science in Nuclear Engineering and the Master of Science in Nuclear Science. Applications for this program plus full information on the prerequisites and course content are now available from the Dean of Georgia Tech's Graduate Division on the campus.

Also scheduled for this fall is the beginning of construction on the radioisotopes laboratory building on the campus. This building and the graduate program were made possible through Governor Griffin's special allocation of $300,000 to start Tech on the road to becoming a major center of nuclear education and research. The new building will house a neutron physics laboratory (including a subcritical atomic pile) for use in the graduate program. It will also contain a classroom for demonstrating the uses of radioisotopes in special courses for industrial, medical and other groups from throughout the State and the South. The building will furnish Georgia Tech staff members with much-needed space and equipment to carry out an extensive program of basic and applied research in radioisotopes.

the Governor's Program

• Present indications are that Governor Griffin—well aware of the great benefits that industry, medicine, agriculture and the people of the State would derive from a research reactor located in Atlanta—will ask the 1957 Legislature for funds to build Georgia Tech a reactor. If this request were granted, the program of needs set down by the Nuclear Science Committee early this year would be satisfied. The interest and cooperation shown by the Governor and the Board of Regents is extremely gratifying to all who have worked so hard to get this program started.

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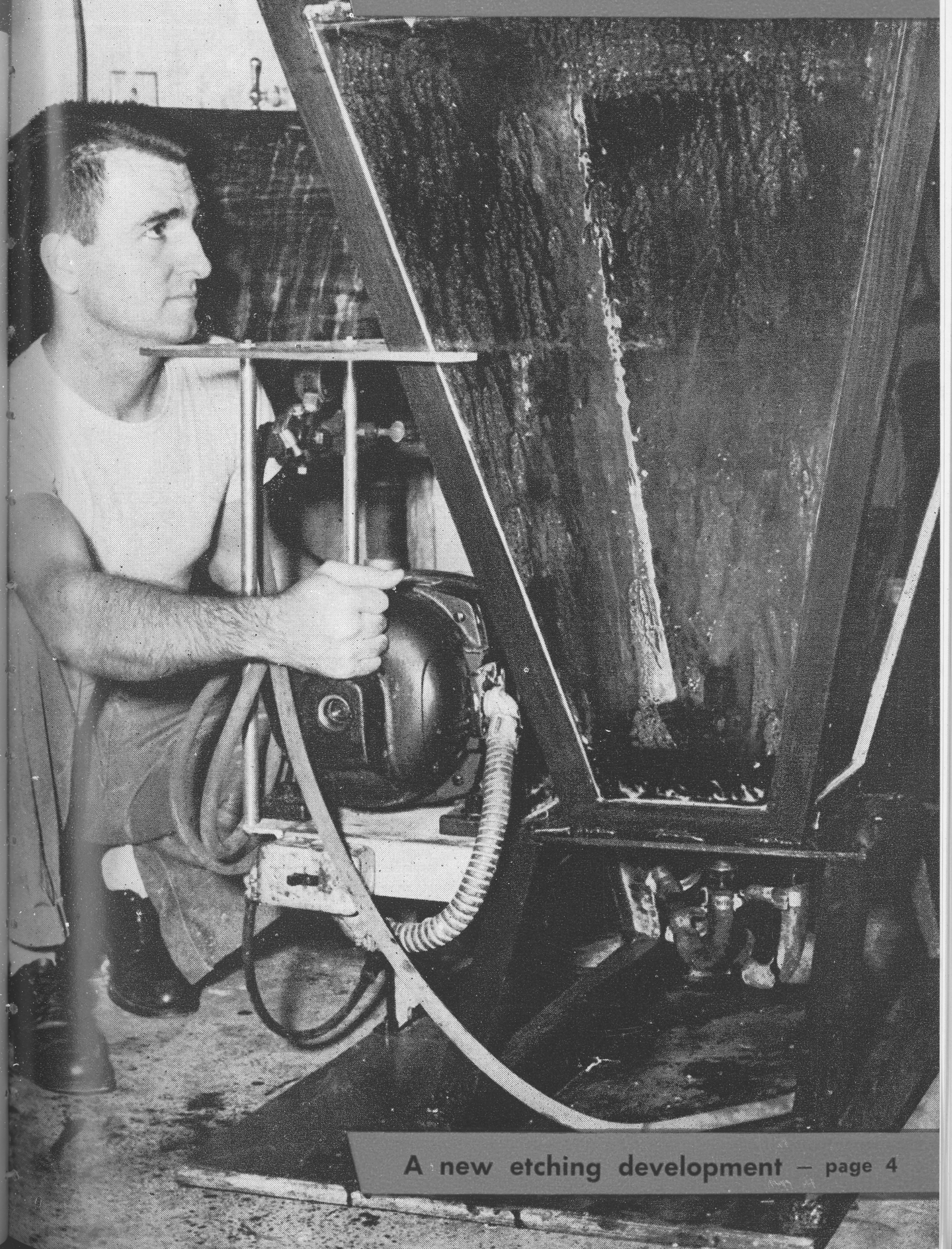
Georgia Tech Engineering Experiment Station



A new etching development – page 4