tion with the living tubercle bacillus. The *phosphatide fraction* introduced into the peritoneum produces a veritable tumor of monocytes, the one variety of mesoblast cells that constitutes the tubercle. This function has later been shown to be possessed, after a finer fractionation, by the *saturated fatty acid* of this phosphatide portion of the tubercle bacillus, H37.

The proportions in which these fractions are obtained are indicated in the several papers referred to, but their significance has a direct bearing on another feature of the same combined study being carried out by Dr. DuBois (not yet published) and his associates at the laboratory of the Russell Sage Foundation of Cornell University Medical School in which it is found that phosphorus is an element of the most vital importance in the whole tubercle process. How far this will lead us in lipin metabolism, carbohydrate metabolism, living cell function and primary life functions no one to-day can predict.

In this program of research there are now cooperating three government divisions, eight universities, four endowed laboratories, two manufacturing chemical plants, seven volunteer health bodies and two semi-governmental bodies.

Scientific research has always gained and suffered from the belief that isolation and untrammeled labor is the birthright of its devotees. This came from a day when fear and accusations of witchcraft followed the pursuit of knowledge and from a time when the well-equipped brain was rare. To-day conditions are vastly different. Our equipment of real students is very large, but in the United States our physical equipment of laboratories and apparatus is far greater than our capacity to use them well. Then there is now a sort of hysterical worship of research, and much passes for research that is only mimicry. Still, there is no nation with our potential for the purest type of research, if we only realize our opportunity.

For those interested in research in its truest sense I can speak with some experience. I have yet to meet one, no matter how abstract his problem, who is not happy to give of his time and his knowledge for the advancement of human welfare if he can see the direct application of his special knowledge in the solution of a problem of general welfare and importance to mankind. Not only that, he is also the most honest member of society in his cooperation and seldom loses sight of his high purpose in his selfish desires. They all seem imbued with the thought of Lord Kelvin, who said "There can not be a greater mistake than looking superciliously upon practical applications of science. The life and soul of science is its practical application."

Therefore, I wish to state again that the carefully defined *problem* is the essential thing, and its analysis and apportionment to the proper student is the road to success. There is little need for new institutes of research with their need of robbing other institutes to man them. It is better to apportion the task to a man where he is and to strengthen him in the environment of his growth. Problems must be viewed in their relation to the nation and to the welfare of man and the more widespread our centers of study the more powerful our reserves, for there are always the oncoming students from which we must draw our future strength.

From the example I have given you of research in tuberculosis you will appreciate that the method of systematic study of pure bacteria on synthetic media, with chemist and biologist working in conjunction, is applicable to many of our disease conditions in plant and animal; but far wider is its application to problems of nutrition and bacterial processes in industry and to the problems of life itself. From a study of the simple units of life we shall probably grow into a knowledge of the congregate groups of life undreamed-of before. This is the more likely since there is no living plant or animal that exists free from myriads of these unicellular organisms performing probably the most necessary functions for existence.

Finally, the greatest difficulty comes, as I have noted above, in the correlation and final synthesis of the knowledge gained. One always has to remember the remark of Jean Jacques Rousseau, "I know only that truth is in the things and not in the mind which judges them; and that the less I put my mind in my judgments about them the more sure am I to come near the truth," and so careful record, rechecks by repetition of the things we record, are necessary before we commence synthesis, lest mixing an impure observation we pollute our conclusion. Time alone can tell the success that will follow our efforts, but already the results have abundantly justified the method. Our endeavor should always be

> To search through all
> And reach the law within the law.
>
> —*Tennyson.*

WILLIAM CHARLES WHITE
U. S. PUBLIC HEALTH SERVICE

THE MECHANICS OF MATERIALS—A CONTRIBUTION FROM APPLIED SCIENCE TO PURE SCIENCE

DURING recent years we engineers have been frequently reminded of our debt to "pure" science and of the necessity of the development of pure science if applied science is to continue its progress. These reminders have doubtless been good for us and we have come to realize (what we have always admitted) that the workers in applied science must keep in touch with the developments in pure science, because the advanced theory of to-day may be written into the handbook of to-morrow as an accepted working formula.

In this salutary mental attitude of recognizing our dependence on pure science let us not forget that while pure science furnishes us with generalizations of observed phenomena into concise, summarized statements which we call laws and also gives us root ideas for many of the practical appliances and processes of applied science, yet it is equally true that the study of practical problems has been the source of not a few of the root ideas of pure science.

This is true to a very striking degree for the science which deals with the strength and elasticity of solids—the mechanics of materials. The writer wishes to point out some of the contributions from applied science to pure science in this particular field and to comment on them briefly.

Our sculptors and our architects look back to the days of ancient Greece as the golden age of art and of architecture. In those days, they say, men thought great thoughts, dreamed great dreams, and crystallized their thoughts and dreams into marble and stone. The applied scientist looking back at the same age sees much to admire and recognizes several intellectual giants in his field, with the rugged features of Archimedes towering above all the others, but the applied scientist sees also how in those days the development of scientific thought and especially the development of scientific technique was limited and hindered by a spirit of intellectual aloofness which caused the man of thought to look down on the world of tools and labor.

We find this spirit playing no small part in shaping the life and writings of Archimedes, whom we hail as the father of all engineers—a title which probably makes his ghost highly indignant. This influence is discussed by Plutarch in his comments on Archimedes in the life of Marcellus. The following paragraphs, from Bernard Perrin's translation, are significant:

> For the art of mechanics, now so celebrated and admired, was first originated by Eudoxus and Archytas, who embellished geometry with its subtleties, and gave to problems incapable of proof by word and diagram a support derived from mechanical illustrations that were patent to the senses. . . . But Plato was incensed at this and inveighed against them as corrupters and destroyers of the pure excellence of geometry, which thus turned her back upon the incorporeal things of abstract thought and sense, making use, moreover, of objects which required much mean and manual labor. For this reason mechanics was made entirely distinct from geometry, and being for a long time ignored by philosophers, came to be regarded as one of the military arts. . . .

The phrase "mean and manual labor" is significant; it will be used frequently in this paper. Quoting again from Plutarch:

> And yet Archimedes possessed such a lofty spirit, so profound a soul, and such a wealth of scientific theory, that although his inventions had won for him a name and fame for superhuman sagacity, he would not consent to leave behind him any treatise on this subject, but, regarding the work of an engineer and every art that ministers to the needs of life as ignoble and vulgar, he devoted his earnest efforts only to those studies the subtlety and charm of which are not affected by the claims of necessity. These studies, he thought, are not to be compared with any others. . . . And although he made many excellent discoveries, he is said to have asked his kinsmen and friends to place over the grave where he should be buried a cylinder enclosing a sphere, with an inscription giving the proportion by which the containing solid exceeds the contained.

Filled with this spirit of intellectual aloofness the Greek architects designed and built beautiful buildings, but left questions of strength to artisans. Hence there was an almost complete lack of planned control of material, of written rules for sizes of beams and columns, and no designs involving long spans. To learn how to design long-span structures it is necessary for the designer to be willing to perform experiments involving "mean and manual labor" and to give his thought to the phenomena of those experiments, and this sort of thinking simply was not done by the intellectuals of ancient Greece.

An interesting example of the results of this attitude is found in the structural details of a Greek naval storehouse at Piraeus, the port of Athens. A lengthy inscription giving somewhat minutely the dimensions of some of the principal beams in this structure has been studied recently by the Danish architect Marstrand. He finds that some one, possibly the architect Philon, seems to have used proportions and dimensions for beams which agree rather closely with the results of calculations using present-day formulas. However, either such rules were deemed too trivial for the dignity of permanent record, as Plato's attitude would suggest, or they were regarded as "mysteries" trade secrets to be jealously guarded from common knowledge.

In any event the intellectual aloofness of the leaders of Greek thought seems to have been effective in checking the development of any body of knowledge concerning the strength of the materials of construction. The philosopher and the artist owe an immense debt to ancient Greece; the structural engineer and the machine designer owe but little. Intellectual aloofness does not give birth to that kind of knowledge which renders possible the building of great bridges and of powerful machines.

For the beginnings of anything which can be called a science of mechanics of materials we must pass over a millennium and three quarters from the time of the Greeks to the time of Galilei, who seems to hold the best claim to be regarded as the founder of this science. The development of mechanics of materials from Galilei to Saint Venant may be regarded practically as the development of a body of knowledge concerning the strength and stiffness of bodies under bending action—beams and long columns.

Galilei attacked the problem of the beam, and in doing so showed none of the intellectual aloofness of the Greek thinkers. In introducing his discussion, which is written in the form of a three-part conversation, he starts not with philosophical speculations, nor with formally stated postulates, but with a comment on the methods used in launching ships in the shipyards of Venice. He observed there that large ships require more complete support on the launching ways than do small ships. This indicates to his mind that the very natural method of designing large beams by making them geometrically similar to successful small beams may be neither correct nor safe. He then discusses internal forces holding beams together, and reasons quite in the manner of the modern text-book writer except that he has no concept of the elastic deformation of the material in the beam and assumes it inextensible and incompressible until the beam breaks. He recurs several times to observed structural happenings in ships and building parts. Apparently his attack on this problem gets its inspiration and many of its suggestions as to methods from the "mean and manual labor" of the shipyard and the marble cutting shed.

Galilei's solution of the beam problem leaves out the vital idea of elastic deformation, but we still honor his pioneer attempt to bring a problem of daily life under the rule of mathematical treatment. He stands out as a great pioneer advocate of experimental study. He calls nothing common or unclean which can aid him in his thinking, and finds many a scientific idea in the works of the artisan.

A striking example of the development of a general law of the science of mechanics of materials from a practical problem is furnished by the development of the law of elastic strain—the law of which Galilei had no inkling—Hooke's law, "As the stretch, so the force."

Robert Hooke was very unlike a Greek philosopher. He was small and ugly of figure, and apparently crabbed of temper. The law which bears his name was a by-product of an invention—a clock which could be used on shipboard and which could be used for determining longitude at sea. The ordinary pendulum clock was not feasible for use on a swaying, pitching deck, and Hooke was led to consider the substitution of a spring balance pendulum. In his invention he was successful technically but unsuccessful financially—a fact which added greatly to his sourness of disposition. He made a few watches of a good degree of accuracy, but his financial plans were deadlocked over details of contract.

But as a by-product of his study of the spring balance pendulum he found it necessary to study the deflection of springs during loading and unloading. His study was experimental, and was extended to helical springs and long wires, and, on what to-day would be regarded as wholly inadequate data, he formulated a broad general law for all "springy bodies." Failing in his attempt to get a patent he kept this law to himself as a trade secret for a decade or more. In 1676 he "announced" this law, and several others, in the form of anagrams—each anagram a pi of the letters making up the words of the law. In 1678 in his lecture "De Potentia Restitutiva" he announced the law plainly.

All our present-day mechanics of materials takes Hooke's law as a fundamental postulate. It was based on a comparatively few rather crude experiments; it was stated in a generalized form far more sweeping than was justified by the range of experiment; it has been found to be only a crude approximation for some materials, and is, probably, to be regarded as an approximation for all materials, though a very close one for most rolled metals; it has well-defined limits for every material, a fact which was not recognized at all by its author; but it is simple, and it *works* very fairly well. The mathematical elastician has accepted it uncritically and has used it as the foundation for a complex superstructure of formula, but the law came, not from philosophical thought, but from the practical experience of an ingenious inventor, for Hooke has been characterized as the "first mechanician of his age." He was not at all intellectually aloof from "mean and manual labor," and his law is a shining example of a contribution made by applied science to the basic law of a pure science.

The 156 years which intervened between the announcement of Hooke's law and the development of the skeleton of the present-day elastic theory saw an interesting succession of workers in the field. Some of them—Euler, Bernoulli, LaGrange, Coulomb, for example—may well be classified as "pure" scientists—men who were fascinated by the problem of a systematic formulation of the laws of elastic action; while others, such as Girard, Tredgold and Barlow, were distinctly "applied" scientists, men who were seeking some systematic rules for designing structural and machine parts. In this period we find Hooke's law challenged by one investigator, but generally accepted; we see many of the concepts and mathematical methods of the elaborate theory of elasticity brought to light, with much vigorous discussion and with many errors. Then were developed the concept of the neutral axis of a beam, the correlation of Hooke's law with the stresses and strains in a beam, and a concept of that most elusive property of a material, the elastic limit. At the end of this period of development we find an engineer straightening out the work of a century and a half into a logical though rather complicated systematized body of knowledge.

Louis Marie Henri Navier was born in Dijon, France, about the time of the close of the American Revolution. He was the son of a distinguished lawyer, but lost his parents at the age of four. He was brought up by his uncle, the well-known civil engineer, Gauthey. Young Navier followed his uncle's calling and studied at the French National School of Bridges and Roads, and in 1808 entered on his career as an engineer.

Navier became a noted bridge engineer, and his memoir on suspension bridges opened to him the doors of the French Academy of Sciences. He was appointed a member of a commission who visited England to study railways there. He was no cloistered theorist, but was in constant touch with the large engineering projects of his day.

While engaged in this engineering work he wrote several memoirs on elastic action of plates and beams, and in these memoirs put the theory of beam action into the general form it has retained ever since, although the memoirs contain much matter which has since been dropped from the beam problem as superfluous. In 1924 we should have held a centennial of the year when Navier "put the beam theory on the map," for certainly the development of this key problem of the mechanics of materials has played a part in the development of the mechanical age of our civilization in no way second to that played by the development of steam power, electricity and metallurgy.

In 1831 Navier was called to the professorship of analytical mechanics at the School of Bridges and Roads, and shortly thereafter he published his "Lessons in Mechanics," which may well be regarded as the ancestor of both engineering texts in mechanics and of texts on the elaborate theory of elasticity.

So in the formative period of the science of mechanics of materials from the days of Galilei to the days of Navier the main ideas have come fully as frequently from the shop, the shipyard and the bridge site as from the study of the scholar or the laboratory of the pure scientist aloof from the practical needs of his day.

The half century which followed the day of Navier saw his ideas simplified, rearranged and summarized into two lines of treatment: (1) The simple mechanics of stress in common use to-day by structural engineers and machine designers, and (2) an elaborate mathematical theory of elasticity. As might be expected the first line of treatment was developed by engineers for engineers—Weisbach and Rankine are two names which stand out prominently—but, what is somewhat surprising, the name which probably stands highest in the development of the theory of elasticity is that of an engineer—Barre de Saint Venant. Yes, Saint Venant, whose name stands for elaborate mathematical analysis, for "pure" mechanics of materials *par excellence*, was an engineer. Pupil and later successor of Navier, bridge and highway engineer for the city of Paris, and in his later days professor of agricultural engineering in the French Institute of Agronomy at Versailles, Saint Venant was so far from intellectual aloofness to the problems of everyday life that in his great work developing the elaborate theory of elasticity he frankly avowed his purpose of putting that science in a form available for the structural engineer and the machine designer.

The speaker believes that he is not going too far when he claims that in the development of the science of mechanics of materials the influence of the engineer has been the major factor.

So much for the past; how about the present? In the science of mechanics of materials is applied science still an important source of ideas and concepts for the development of the "pure" science? Two present-day problems in that science will serve as illustrations:

A question of primary importance, still undecided, concerns the cause of structural failure in materials. Is such failure due primarily to excessive internal forces—to *stresses?* Is it due to excessive distortion—to *strains?* Is it due to a tendency to excessive internal stored *energy?* Each of these theories has been held to be the cause of failure, and all with the possible exception of the first-named, stress, have advocates today.

There is not time to review, even in outline, the experimental study of this problem; suffice it to point out that the names of engineers are outstanding in the literature of this problem. There is Guest, the advocate of the shear theory, who began his work in this field about the time he was professor of applied mechanics at Worcester Polytechnic Institute; Hancock, professor of applied mechanics at Purdue, who later was at Worcester; Becker, professor of applied mechanics at North Dakota, who as a graduate student at Illinois contributed an interesting study showing the importance both of shear and of strain; Haigh, metallurgist and engineer at the British Royal Naval Academy at Greenwich, who is the outstanding advocate of the energy theory, and Matsumura and Hamabe, of the Engineering Department of the Kyoto Imperial University in Japan, who have done pioneer work in studying the failure of brittle metals.

The speaker ventures the opinion that this fundamental question is to-day receiving fully as much attention in laboratories of applied mechanics as in laboratories of physics.

A second question of vital importance to the science of mechanics of materials has to do with establishing the limits of accuracy and usefulness of the whole theory of elasticity.

The microscope has become one of the recognized tools of the student of mechanics of materials,[1] and it has shown that the fundamental assumptions of homogeneity, continuity and indefinite divisibility of solids are by no means strictly true.

The study of the behavior of materials under repeated stress has emphasized this fact. Under repeated stress little localized areas of high stress—areas around holes, screw threads and grooves for example—are in danger of becoming nuclei for destructive spreading cracks. Such little areas have no appreciable effect on the behavior of a machine part as a whole under a load repeated but a few times. The civil engineer can usually neglect these localized stresses—and usually does so; the mechanical engineer can not safely neglect them.

However, when the effect of holes, grooves, screw, threads and the like on the actual strength of parts subjected to repeated stress is studied by means of tests to destruction and is compared with the theoretical "stress-raising" effect as given by the formulas of the theory of elasticity, serious discrepancies are found, and the effect of such "stress raisers" is, in nearly all cases studied, less than the theory of elasticity would lead us to expect.

[1] In this connection it may be noted that what seems to be the first micrograph of steel was made by Reaumur in 1722. Reaumur started his scientific career as a mathematician, but made his microscopic study of the constitution of steel as a part of his work under a commission by the French government to prepare a report on the useful industries of France. He made this isolated pioneer contribution to our knowledge of materials while working as an applied scientist.

Now the development of this study of the effect of localized stress has taken place almost wholly in engineering laboratories—in the laboratories of the British Aircraft Research Panel, in the engineering laboratories of the British National Physical Laboratory, in the Naval Engineering Experiment Station at Annapolis and in the Materials Laboratory of the U. S. Air Service at McCook Field at Dayton, in the metallurgical and engineering laboratories of the Bureau of Standards. We think we have done some work along this line in the engineering experiment station of the University of Illinois.

The writer believes that it may be justly said of the present-day study of the science of mechanics of materials—the science as distinguished from the art—that to-day the greater part of the work is being done by applied scientists in engineering laboratories.

The rôle of prophet is a fascinatingly dangerous one. The writer can not resist the temptation to attempt that rôle and to make a prediction of a division of the science of mechanics of materials which he thinks will assume major importance within the next generation, and also a prediction that engineers will take a prominent part in its development.

Two practical problems are the forerunners of this division of the science. For some years oil well drillers have found their long steel drill rods breaking at places where stress and strain was supposed to be small, and they have been much puzzled thereby.

A few years ago users of steam turbines were much disturbed by failures of the disc wheels of turbines which seemed to be caused by sidewise "fluttering" at certain critical speeds. There used to be a saying that if he hit just the right note a fiddler could "fiddle down a bridge"—and while, so far as the writer knows, no bridge failure has ever been caused in this way, yet it did seem that in these turbine discs there were failures caused by "tuned" vibration.

Now in both the long drill rods and in the turbine discs we face the problem not of *static* balanced stress, but of *kinematic* stress with interference of waves of stress and resulting high stress in unexpected places. Students of acoustics and of earthquake waves have given hints concerning this problem, but the writer is of the opinion that the science of kinematics of stress and of stress waves is one which is at present in about the same stage of development as was the science of static stress before the days of Navier and Saint Venant, and he further believes that engineers working in engineering laboratories will have a large share in its development.

In support of this last claim there may be cited the work of the late Wilfred Campbell on the mechanics of stress in a rotating turbine disc. This work was done in the shops of the General Electric Company and involved both careful work in the study and much "mean and manual labor" in experimentation. Plato would certainly have been shocked and alarmed at the conditions under which Mr. Campbell carried on his beautiful and delicate studies of rotating and fluttering discs. He solved a special problem in the kinematics of stress and left as his monument what the writer believes will be recognized as an important piece of pioneer work in a new division of the science of the mechanics of materials.

The writer is not one of those who would meet the attitude of "intellectual aloofness" of the old-time classical philosopher with a scorn of "theory" which characterizes a certain type of "practical" man. He believes that both intellectual aloofness and self-satisfied practicality are signs of a narrow mind. There have come to all departments of science contributions from the study, the library and the laboratories of the sciences which are little concerned with the immediate practical results of their experimentation; there have come contributions also from the machine shop, the structural shop and the engineering laboratory.

May we not picture pure science as occupying quarters in an impressive stone building at one end of a busy street, and applied science as occupying quarters in a plain well-lighted shop at the other end. Between the two structures many messengers go back and forth carrying books and papers and driving trucks loaded with machines and apparatus. The highway between pure science and applied science is *not* a one-way street.

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SCIENTIFIC EVENTS

COOPERATIVE ETHNOLOGICAL AND ARCHEOLOGICAL INVESTIGATIONS BETWEEN THE SMITHSONIAN INSTITUTION AND STATE, EDUCATIONAL, AND SCIENTIFIC INSTITUTIONS

AT the past session of the Congress, the following act authorizing cooperation in ethnological and archeological investigations was enacted:

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That the Secretary of the Smithsonian Institution is hereby authorized to cooperate with any State, educational institution, or scientific organization in the United States for continuing ethnological researches among the American Indians and the excavation and preservation of archeological remains.

Sec. 2. That there is hereby authorized to be appropriated, out of any money in the treasury not otherwise appropriated, the sum of $20,000, which shall be available until expended for the above purposes: *Provided,* That at such time as the Smithsonian Institution is satisfied that any State, educational institution, or scientific organization in any of the United States is prepared to contribute to such investigation and when in its judgment such investigation shall appear meritorious, the Secretary of the Smithsonian Institution may direct that an amount from this sum equal to that contributed by such State, educational institution or scientific organization, not to exceed $2,000, to be expended from such sum in any one State during any calendar year, be made available for cooperative investigation: *Provided further,* That all such cooperative work and division of the result thereof shall be under the direction of the Secretary of the Smithsonian Institution: *Provided further,* That where lands are involved which are under the jurisdiction of the Bureau of Indian Affairs or the National Park Service, cooperative work thereon shall be under such regulations and conditions as the Secretary of the Interior may provide.

Approved, April 10, 1928. (Public—No. 248—70th Congress.)

The appropriation of $20,000 authorized by the above act was made in the Deficiency Act, approved May 29, 1928.

1. From the above appropriation, the Secretary of the Smithsonian Institution may approve expenditure of a sum equal to that provided by any state or educational or scientific organization, not exceeding $2,000 in any one state in any one year, when satisfied that such state or organization is prepared to contribute to such investigation, and when in his judgment cooperation by the Institution in such investigation is justified.

A. Requests for cooperation should be made by the responsible officer of the State, educational institution or scientific organization interested.

B. Applications should be accompanied by full explanatory statements of the work proposed, the location, purpose and any other pertinent details, the name of the field representative, if any, of the applicant, and should state whether any supervisory salaries are to be paid from that portion of the joint fund provided by the applicant, and if so, the amount thereof. It is intended that all funds provided for such cooperative work shall be devoted strictly to the prosecution of definite projects contemplated by the act, and shall not be used for the payment of regular salaries or other regular expenses of any organization.

C. Applicants must present suitable evidence of the availability of funds for cooperative use and will present at regular intervals detailed accounts of expenditures therefrom. Full instructions will be furnished regarding expenditures from allotments by the Institution, which must be made to conform with the accounting regulations of the United States Treasury Department.