UNIVERSITY AND EDUCATIONAL NOTES

A GIFT of $100,000 has been made by Mr. and Mrs. R. T. Crane, Jr., Ipswich, Mass., toward the endowment of the department of therapeutics of New York University and Bellevue Hospital Medical College.

THE University of Southern California plans to ask the medical profession of Southern California to raise $500,000 for the endowment of the new medical school which will open in September. An entire new faculty is to be appointed in the reopening of the medical school. The appointment of the full-time professors for the preclinical work will be made in the next few months.

THE corporation of Yale University has increased by $500 the salaries in each grade of the assistant professorships and associate professorships and has raised the minimum salary for full professors from $5,000 to $6,000.

CAPTAIN EDWARD STEIDLE, of the Carnegie Institute of Technology, has been appointed dean of the school of mines and metallurgy at the Pennsylvania State College.

DR. JOHN FRAZER, professor of chemistry and for the past sixteen years dean of the Towne Scientific School of the University of Pennsylvania, has resigned as dean in order to continue his teaching and to devote himself to research in chemistry. He has been granted a leave of absence for the coming year.

DR. ROBERT CHAMBERS, JR., professor of microscopic anatomy in the Cornell University Medical College, has been appointed by New York University to be chairman of the department of biology and research professor of biology at its Washington Square College.

DR. SYDNEY W. BRITTON, associate in physiology at the Johns Hopkins University, will leave at the end of the scholastic year to become professor of physiology at the University of Virginia Department of Medicine, Charlottesville.

DR. SHERMAN C. BISHOP, zoologist in the New York State museum since 1916, has been appointed professor of zoology in the department of biology at the University of Rochester.

DR. JOHN G. SINCLAIR, of the department of anatomy at the University of Wisconsin, will go to the University of Texas as associate professor of embryology and histology in the medical school at Galveston, Texas.

PROFESSOR J. M. BRYANT, of the University of Texas, has been appointed professor of electrical engineering and head of the department at the University of Minnesota. Professor I. M. Kolthoff, of the University of Utrecht, Holland, has been appointed professor of analytical chemistry and chief of the division.

DR. ARTHUR T. EVANS, for the past five years head of the department of botany and plant pathology at South Dakota State College, has resigned to become head of the department of botany at Miami University, Oxford, Ohio.

AT the University of London the following appointments have been made: Dr. Percival Hartley, of the National Institute of Medical Research, has been appointed to the university chair of biochemistry tenable at the London School of Hygiene and Tropical Medicine. Dr. C. B. Fawcett, reader in geography in the University of Leeds, has been appointed to the university chair of economic geography tenable at University College. Dr. Geoffrey Hadfield has been appointed to the university chair of pathology tenable at the London School of Medicine for Women.

DISCUSSION AND CORRESPONDENCE

OVARIAN SECRETION AND TUMOR INCIDENCE

IN SCIENCE for April 13, 1928, Dr. W. S. Murray[1] has published, in reply to my note in SCIENCE of January 27, 1928,[2] a second article on the relation between the internal secretion of the ovary and the origin of tumors of the mammary gland in mice. Inasmuch as the statements of this author as to the facts, on which the proof of the significance of internal secretions of the ovary for the development of mammary cancer in mice is based, are incorrect, and in particular as his statements as to my work concerning this problem are incorrect, I feel constrained to state briefly what I believe to be the correct interpretation of the facts in this case.

(1) According to Dr. Murray, in my series of experiments (published twelve and nine years ago),[3] my own figures prove that the reduction of tumor rate through castration on the one hand, and through prevention of breeding on the other hand, are of approximately the same order. This conclusion is made possible only by adding together all my castration experiments, irrespective of the time at which the castrations were carried out. This is a procedure which is misleading, the inadvisability of which I have emphasized in my paper published nine years ago. There I pointed out that mice castrated at the age of three to four months remain practically free from tumors. There was one doubtful case among fifty-four castrated animals between the age of three and six months

[1] Murray, W. S., SCIENCE, 66, 600, 1927, and 67, 396, 1928.

[2] Loeb, Leo, SCIENCE, 67, 104, 1928.

[3] Loeb, Leo, *Am. Journ. Cancer Research*, 1, 1, 1916, and *Journal of Medical Research*, 40, 477, 1919.

in which a tumor arose, and, as I then stated, there was some reason for believing that in this instance we had to deal with an animal which was older than four months. But even conceding that there developed one tumor among fifty-four castrated mice, we would have accomplished through castration in mice, at an early stage of sexual maturity, a reduction in the tumor rate from 68 per cent. to 1.8 per cent. These results, and similar ones obtained by Cori,[4] whose mice were castrated at a still earlier period of life and remained entirely free from tumors, show that castration carried out in early life, by removing the cyclic stimuli exerted on the mammary gland through internal secretions given off by the ovary, prevents the development of mammary cancer in mice, which otherwise were destined hereditarily to have a higher tumor rate.

There was in addition one group of seventeen mice castrated between the age of four to six months, among which there were therefore no mice younger than four months; in this group there appeared four tumors. If we include this group among the animals castrated between the age of three to six months we obtain five mice with tumors among seventy-two castrated mice; this corresponds to 7 per cent. It is only if we castrate mice fully six months old or older that the incidence of cancer becomes higher.

(2) This is a result totally different from the effect produced through prevention of breeding. Through the latter procedure it was possible, in these investigations carried out by Miss Lathrop and myself, to effect in some strains a reduction of 30 per cent. or slightly more; but in other strains the reduction was much smaller. In one strain of non-bred mice there was even an increase in the tumor rate, partly owing to the increase of length of life due to non-breeding. On the average the tumor rate of the mice belonging to families with a high tumor rate fell to about 30 per cent. and in one case to about 23 per cent., while in other cases it remained considerably higher. In general, in non-breeding mice the tumor appeared later in life than in mice which were bred. Different strains varied as to the effect of prevention of breeding on the cancer rate, but we did not obtain by these means a diminution of the tumor rate below 20 per cent. In the strain used by Dr. Cori the non-breeding caused only a slight reduction in the tumor rate.

The fact, shown in my previous experiments, that prevention of breeding, in general, reduces the tumor rate, although to a less extent than early castration, proves that, in addition to the cyclic stimuli exerted by other ovarian structures, also the stimuli given off by the internal secretion of the corpus luteum and the stimuli associated with the abrupt cessation of the action of the corpus luteum following the preceding growth period, may play a rôle in the development of mammary carcinoma in mice.

(3) As I pointed out in my previous note, I attempted to confirm the results thus obtained through transplantation of ovaries into castrated male mice belonging to strains with a high incidence of cancer. Owing to conditions over which I had no control, the number of experiments had to be limited and I obtained therefore negative results. Murray, in operating upon a much larger number of male mice, succeeded in obtaining positive results in a small minority of his animals. I consider his results as a valuable contribution to this problem; however, as far as the main point at issue is concerned, the importance of the internal secretion of the ovary in the production of mammary tumors in mice and of the quantitative interaction of these substances with hereditary factors, this had been demonstrated conclusively through our earlier experiments and it has been confirmed by the work of Cori.

(4) As far as the stock of mice is concerned which served for these experiments and which is referred to by Dr. Murray, I may state that I observed personally not only the mice castrated by myself previously, but also certain control mice which developed tumors under conditions under which the mice castrated, at an early period, remained free from tumors.

I may further state that the investigations on heredity of mammary cancer in mice, on which the experiments concerning the action of internal secretions were based, were jointly carried out by Miss Lathrop and myself. While the mice used for breeding were almost exclusively attended to personally by Miss Lathrop, I visited the breeding establishment from time to time, and had occasion to convince myself of the untiring energy, care and trustworthiness with which Miss Lathrop carried out the breeding experiments which I had suggested and in which she was deeply interested. In many cases, I controlled the results as to the development of tumors through autopsies performed on this stock, and if a mistake did happen Miss Lathrop made it a point to inform me of such an occurrence. I have no doubt that the care which Miss Lathrop gave to the actual breeding of the mice was vastly superior to the attention which an ordinary attendant in laboratories is accustomed to give to such work; and it is on the cooperation of others that investigators have to rely in breeding experiments which are carried out on a large scale. Furthermore, in its important aspects the results which we have obtained as to the hereditary transmission of tumor rate in distinct strains of mice have been confirmed by subsequent investigators, a fact which may be considered as a proof that the breeding

[4] Cori, C. F., *Journ. Exper. Med.*, 45, 983, 1927.

experiments had been carried out with the necessary care.

(5) In these investigations we considered it sufficient to divide the mice to be used in the castration experiments into age groups covering in some cases two, in other cases three months periods. For statistical purposes, I combined these mice into one class, including the mice between three and six months of age. However, Miss Lathrop kept a record of the time of birth of each litter used in our work on the heredity of cancer in mice, and there is no justification for the conclusion on the part of Murray that such records were not kept. As to the mice which were castrated at this age (between three and six months), they had been prevented from breeding previous to the operation; but if, contrary to our plans and knowledge, they should have bred, the result of castration in preventing the development of mammary cancers in these mice would have been the more striking.

(6) In conclusion I may state again that the investigations, on which Murray reports, without exception represent the type of experiments which I had carried out previously and that through this earlier work the significance of the various internal secretions, given off by the ovary, for the development of mammary cancer in mice had been proven.

LEO LOEB

DEPARTMENT OF PATHOLOGY,
WASHINGTON UNIVERSITY
SCHOOL OF MEDICINE

THE RING METHOD IN CHANGING SURFACE TENSION

I HAVE just seen a very excellent paper by S. L. Bigelow and E. R. Washburn, published in the *Journal of Physical Chemistry*, on "Variations in the Surface Tension of Solutions."

It is a great pleasure indeed to read such a reliable and conscientious piece of work. I hope the authors will find it natural that I should explain a statement which I made somewhere and which they quote as "remarkable," not in the complimentary sense of the word, I am afraid. The sentence read: "It is only through the ring method that it is possible to observe and study this phenomenon (changing surface tension) as it is the only procedure which permits the measurement of surface tension of the same layer of liquid at very short intervals." This statement is not accepted by the authors of the paper, who decide that it is "manifestly in error," and they are quite surprised that I should have found anything by this method.

In the first place, they are right, as it is obviously not the "only" method whereby such changes can be observed, but I maintain that it is the only one which, as I said, makes readings possible at "very short intervals." Of course we may not call "very short intervals" the same thing. What I meant were intervals of the order of one second and less. I have published in my book experiments where eight measurements were taken in the first minute. The technique is described in the same volume. That, I still believe, is impossible with the capillary method. Furthermore, if Messrs. Bigelow and Washburn had read the aforesaid book carefully, they might have understood how it was I managed to observe phenomena which they confirmed: "Pulling off a ring, they say, and replacing it must seriously upset any molecular arrangement in the surface . . ." Well, it does, but I did not always pull it off and replace it. I used a different sample of the same solution for every measurement. The ring was pulled off only once.

However, I must add that, even when such precautions are not taken, the phenomenon of time-drop can be followed, but not as accurately, of course.

LECOMTE DU NOÜY

INSTITUT PASTEUR, PARIS

"NUTRILITES"

THE term "vitamine" was introduced by Funk to designate those unknown factors in nutrition which were thought to prevent various diseases. This term with a modified spelling has become widely adopted in spite of its obvious defects. The term has been applied in some cases to unknown substances which in small amounts are effective in the nutrition of fungi (including yeast), bacteria and other organisms. At present, however, the tendency is to restrict the use of the word "vitamin" entirely to substances concerned in *animal* nutrition.

The word "bios" was introduced by Wildiers to designate an unknown substance which in small amounts stimulates yeast growth. The word "auximones" was likewise introduced by Bottomley to designate substances of a similar nature which were thought to be effective in the nutrition of certain green plants. It is increasingly apparent that there are unknown factors which function in the nutrition of many types of organisms. It is also obvious that there is need for a general term to designate these factors. Otherwise it will be necessary to invent new names for substances found to be effective in the nutrition of bacteria, molds and other forms of life. None of the terms in use at present applies.

It is suggested that the word "nutrilite" be used to designate all those vitamin-like substances which in small amounts function in the nutrition of organisms in general. The term has the advantage that it indi-