originated in this way, and the board is making inquires as to the local prevalence of the carrier mosquitoes, and taking other precautions in regard to the disease.

UNIVERSITY AND EDUCATIONAL NEWS

THE University of Illinois college of medicine announces that, beginning with June 3, it will operate on the quadrimester system. In this system there will be three terms of four months each per calendar year. The courses will be so arranged that it will be possible for a student to enter the school at the beginning of any one of the three terms.

AFTER September of this year at Columbia University the doctorate of medicine of the medical school will be conferred only upon men who have had, in addition to four years at the medical school, one full year of service at a hospital under faculty supervision.

DR. WILLARD J. FISHER, at present honorary fellow in physics at Clark University and lecturer in physics at Worcester Polytechnic Institute, goes to Manila as assistant professor in physics at the University of the Philippines, with duties to begin about July first.

MR. KIRTLEY F. MATHER has resigned his position at Queen's University and has accepted the professorship of geology at Denison University, Granville, Ohio.

DR. FRANCIS M. VAN TUYL, assistant professor of geology and mineralogy in the Colorado School of Mines, has been promoted to an associate professorship.

DAVID D. LEIB has been promoted from associate professor to a full professor of mathematics in Connecticut College, New London.

DR. GEORGE A. BAITSELL, instructor in biology in Yale College, was appointed an assistant professor of biology at the March meeting of the Yale Corporation.

MR. WALTER S. BEACH, who will take his doctorate with his thesis in plant pathology this coming commencement, at the University of Illinois, has been appointed as instructor for plant pathological research in the Pennsylvania State College. He will have charge of a separate laboratory located near Philadelphia and is to take up his work at once.

MR. PAUL F. GAEHR, who spent the past year in research at Cornell University, will next year resume charge of the physics department at Wells College.

DISCUSSION AND CORRESPONDENCE

PROFESSIONAL COURTESY

IN the March 8, 1918, number of SCIENCE there appeared from Professor McCollum and Miss Nina Simmonds a reply to Professor Hart's statement on professional courtesy in SCIENCE, March 1, 1918. As the former introduce a question of veracity in a statement concerning me and as they express an eagerness to be judged on "research records" I feel it my duty for the enlightenment of the public to call attention to evidence furnished by such "research records."

It is significant that the article published by Professor McCollum and Nina Simmonds as coming from the Laboratory of Agricultural Chemistry of the University of Wisconsin and to which Professor Hart referred as not indicating proper authorship, was published without the legend "Published with the permission of the Director of the Wisconsin Experiment Station." All publications coming from this station are required to have this official stamp of approval. That the authors complied with this regulation for years and violated it in this and two other recent contributions, is truly significant.

It is also significant that the said authors have not given proper credit to this institution for work done by them at Wisconsin. There has appeared in the February, 1918, number of the *Journal of Biological Chemistry* an article purporting to come as a contribution by E. V. McCollum and N. Simmonds from the Laboratory of Biochemistry of the School of Hygiene and Public Health of the Johns Hopkins University. The article was received for publication December 26, 1917, only twenty-five weeks after the authors, E. V. McCollum and N. Simmonds, had severed their official connection with the University of Wisconsin, yet in this article there were published as bona fide new contributions thirteen growth curves of rats extending over periods of twenty-eight

to forty-three weeks. One curve of growth of forty-three weeks is but a continued curve of thirty weeks stated by Professor McCollum in the *Journal of the American Medical Association* of May 12, 1917, as having been presented by him before the Harvey Society, January 13, 1917. This experiment has actually been completed at least eleven weeks before Professor McCollum left the University of Wisconsin. In fact some of the curves bear a serial number and legend the same—and none bear a higher or lower serial number—as curves of similar experiments previously published by him from this institution.

Again it is significant that Professor McCollum upon severing his relations with the University of Wisconsin removed from the campus all station records accumulated by him, and some of those of other members of the staff, without the permission or knowledge of the staff, or administrators. It is recognized that a university professor doing research work on his own initiative and on his own problems is entitled to the exclusive possession of his notes, but no such *exclusive* right is recognized in the case of experiment-station workers even to their own notes on continuing projects carried out under federal grants for an indeterminate period.

Whatever may be said in denial, these are the facts. The first two are directly supported by evidence submitted in articles published by the aforementioned authors themselves. The third, first mentioned in Professor Hart's statement, they have already not seen fit to deny.

Except for the purpose of bringing out, for the benefit of those who may be concerned in the future, not only a case of transgression of professional courtesy but of professional ethics as well, the writer is not inclined to present arguments in his own behalf, especially in a matter of such small personal moment as credit for the scientific article. For the major portion of the time while the vitamine preparations in the research in question were being made and their stability was being tested Professor McCollum did not even know what was being done, or how it was done; he fed the rats. In fact, the 1916 report of the Director of the Wisconsin Experiment Station gives the writer of this note exclusive credit for activities in this field.

My acquaintances know full well what Professor McCollum's real personal opinion of me was before his transgressions, in an attempt to hamper further experimental work, called forth deniable but unrefutable charges. In place of a lengthy presentation of details actually called for by the indirect question aimed at my veracity and Professor Hart's veracity, but really best forgotten, there have been presented a few general facts in *final* answer instead.

H. Steenbock
University of Wisconsin

To the Editor of Science: Please accept my thanks for submitting to me the attack upon my character by Mr. Steenbock, in order that my reply may be printed together with it. I do not care to be a party to an undignified dispute over the question of the accuracy of the accusations which are contained in this letter, and shall not attempt an elaborate explanation of details. For the benefit of such readers as are not familiar with the original publications of Hart, Steenbock and myself, which will, I feel assured, suffice to prevent my colleagues in the field of biochemistry from giving any serious consideration to this matter, I shall present, briefly, a few facts which will enable them to see the matter of this controversy in its proper perspective. I shall hereafter take no notice of further utterances of this character. Any one who will take the trouble to examine the publications which have emanated from the laboratory of agricultural chemistry of the University of Wisconsin during the last ten years can easily form an opinion for himself as to who was initiating the work in nutrition investigations during that period.

When I left the University of Wisconsin in the summer of 1917, I took with me all the records of the experimental work with my rat colony but not any notes other than my own. No one who had not been closely identified with the work could possibly have correlated the many results, some of which were worthy of

publication, and others, for one reason or another, not satisfactory from which to draw conclusions. Furthermore, it will be generally conceded that no one but the experimentor himself has the right to the interpretation of his data, for he must be responsible for the correctness of such interpretation.

I find on reexamination of the charts in the paper by Miss Simmonds and myself in the *Journal of Biological Chemistry,* 1917, XXXIII., p. 303, that several of the curves of growth were secured in experiments carried out before our removal to Baltimore. I regret that mention was not made of this fact. Most of our papers contain data which was not secured from a series of experiments carried out simultaneously. The later experiments are in most cases planned in the light of the outcome of the earlier ones, the work being continued until a complete demonstration of some principle is secured. No injustice was intended or will in future be done to the University of Wisconsin by withholding proper credit for the facilities which made the work possible. The serial number of an experiment signifies the period when a certain ration was planned and entered in our notes and does not throw any light on when the feeding trial was made.

In 1907 I began to build up my rat colony at the University of Wisconsin for the purpose of studying the problem of the cause of the failure of young animals to grow when restricted to diets consisting of purified proteins, starch, sugars, fats and suitable inorganic salt mixtures. No one in this country at that time had any interest in the enterprise except myself. My first publication describing this work appeared in 1909, and antedated that of any other of similar character by two years. It required five years of fruitless experimenting before the first important observation was made which gave a clue to the solution of the problem. In 1912, Miss Davis and I first observed the peculiar growth-promoting properties of butter fat. We had a ration which we supposed consisted of food substances essentially pure, with which we could induce growth when butter fat was included to the extent of five per cent., whereas the same food mixture containing such a fat as olive oil or lard did not induce any growth. For a time we believed that butter fat contained the only chemically unidentified dietary essential necessary for the promotion of growth or the maintenance of health in a mammal. By 1915, Miss Davis and I, after making several hundred experimental feeding trials variously modified, were forced to the conclusion that a second unknown dietary essential had been contained in the 20 per cent. of supposedly purified milk sugar, which had formed a constant constituent of many of our early diets; we thereupon propose a new working hypothesis concerning what constitutes an adequate diet. This postulates the necessity of two dietary essentials of unknown chemical nature.

After a long series of experiments planned to show the distribution of these two substances in natural food-stuffs, it was found that one of them is associated with certain fats, whereas the other is never associated with fats of either animal or vegetable origin. The latter is soluble in water, the former readily soluble in fats. Miss Kennedy and I, therefore, proposed the terms fat-soluble A and water-soluble B as provisional names pending such a time as we should learn enough about their nature to be able to give them names which would be suited to their peculiar structure, and fit in with the nomenclature of organic chemistry.

About two thousand feeding experiments have now been completed, each lasting between six weeks and two years. These were all interpreted in the light of our working hypothesis described above, and also in the light of the composition of the proteins as revealed by the studies of Fischer, Abderhalden, Osborne and others, and have made clear the nature of the dietary deficiencies of several representatives of each of the several classes of natural foods, seeds, tubers, roots, leaves, meats, eggs and milk. These results have made possible important generalizations, which must eventually lead to great improvement in the health of large groups of peoples who are now suffering

from malnutrition, due to their living on poorly constituted diets, and also to greater efficiency in the use of feeding-stuffs in animal production.

Our solution of the problem of successfully feeding diets of purified foodstuffs together with the two unidentified food essentials, fat-soluble A and water-soluble B, greatly simplified the study of the problem of isolating the latter substances. Indeed without it the study of this problem can scarcely succeed. My associates and I have further simplified the problem of their isolation by the demonstration that similar "protecting" substances do not exist for the diseases scurvy and pellagra. It had become a widely accepted belief that there existed not less than four such unknown dietary essentials, one for the prevention of beri-beri, another each for scurvy, pellagra and for rickets. This belief rested on the "vitamine" hypothesis of Funk. I need not here dwell upon the important studies of Eijkmann, Fraser and Stanton, Stepp, Holst, Funk, Williams, Osborne and Mendel, and of Goldberger, a critical study of whose papers greatly aided us in the planning of our experimental diets and in the interpretation of our results.

During my stay at the University of Wisconsin nobody had anything to do with independent work with my rat colony, except in a small way an independent study was carried on by Mr. V. E. Nelson during the months just preceding July, 1917. I reiterate my statement in my reply to Professor Hart in Science for March 8, that the work which they charged I had made dishonest use of, which was participated in by Mr. Steenbock, was planned entirely by me, and was carried out by him as directed, in the capacity of an assistant. He was not consulted about the interpretation of the data in the paper by McCollum and Simmonds (*Jour. Biol. Chem.*, January, 1917), because his personal attitude towards me before I left Wisconsin made impossible a joint preparation of the paper, and he was therefore given credit for the preparation of the materials employed in the experiments instead of being made joint author, as I should have been glad to have made him under other circumstances.

When one leaves an institution after having made observations of a fundamental character, and having for several years made use of these in the development of new and important lines of research, his colleagues who remain behind have, of course, a right to continue investigation in this field, just as any one located elsewhere has the right to take advantage of the observations of others, and attempt to further the acquisition of knowledge. There is no property right in research or its results so long as it is incomplete and not protected by patent. Some proceed on this theory, attempting the while to perfect details, and to add some element of originality, and to give their work the mark of independent thought. Others prefer to spend their time in making experiments of an exploratory character, at the risk of doing much unprofitable work in order to make some observation which will open up a new field of investigation which they may follow with profit. A few prefer to attempt to bring into disrepute some investigator who has opened up a new field of research when he has reached a point where much further work remains to be done, which is obvious to every one who studies his published results, in the hope that they may thereby so discredit him that his work will be interfered with, with a view to making possible the reaping of a harvest of opportunity which his absence from the field would make possible. Many believing that the author of the first important observation has the right to be allowed to develop the new field without annoyance, refuse, from a sense of self-respect, to pounce upon, and, in haste, complete what another is doing, when a study of fundamental nature makes possible a new type of investigation. Judgment as to which course one should pursue will, of course, be determined by the standards of the individual.

E. V. McCollum

Baltimore, Md.

THE WORLD'S CALENDAR

To the Editor of Science: A communication by W. J. Spillman in Science of May 17 discloses the fact that a bill was introduced in the Congress on April 16 with the object of