Japan. Each of the seven universities contributes five hundred dollars every other year to send a representative from the United States to Japanese universities.

PREPARATIONS are under way for the centennial commencement of Hamilton College on June 17, 1912. Senator Elihu Root, chairman of the board of trustees, has announced that President Taft and Vice-President Sherman will deliver addresses.

GOVERNOR STUBBS, of Kansas, Chancellor Frank Strong and regents William Allen White, Rodney A. Elward and Scott Hopkins, of the University of Kansas, have spent three days at the University of Wisconsin studying its methods with special reference to the extension of its work in education throughout the state.

DR. THOMAS E. HODGES was installed as president of the University of West Virginia on November 1.

BY the appointment of Professor H. C. Peffer as professor of chemical engineering at Purdue University, this department has been raised to the status of a school coordinate with those of civil, electrical and mechanical engineering, and made independent of the department of chemistry, which, however, will continue to give instruction in general, organic and analytic chemistry. Professor Peffer will, during the current year, give instruction to seniors in industrial organic chemistry and metallurgy, and direct the preparation of graduation theses. Professor Peffer is a graduate (B.S. 1895 and M.S. 1907) of Pennsylvania State College and has been connected as chemist or superintendent with the Carnegie Steel Co., the Pennsylvania Salt Co. and the Pittsburgh Reduction Co., now the Aluminum Company of America.

AT the University of Missouri the following appointments have recently been made: W. J. Calvert, M.D. (Johns Hopkins), professor of preventive medicine; J. A. Ferguson, M.F. (Yale), professor of forestry; R. H. Baker, Ph.D. (Pittsburgh), assistant professor of astronomy and director of the Laws Observatory; H. L. Kempster, B.S. (Michigan Agricultural College), assistant professor of poultry husbandry; Lawrence G. Lowrey, A.M. (Missouri), acting assistant professor of anatomy; A. J. Meyer (formerly of Wisconsin), assistant professor and superintendent of the two-year course in agriculture; Matthew Steel, Ph.D (Columbia), assistant professor of physiological chemistry; G. S. Dodds, Ph.D. (Pennsylvania), instructor in zoology; O. F. Field (formerly of Nebraska), instructor in physical education; R. L. Gainey, A.M. (Washington University), instructor in botany; Paul Phillips, B.S. (Missouri), instructor in manual arts; Ralph E. Root, Ph.D. (Chicago), instructor in mathematics; W. A. Tarr, S.B. (Arizona), instructor in geology and mineralogy. The following promotions have been made: E. A. Trowbridge, from assistant professor to professor of animal husbandry; C. B. Hutchinson, from instructor to assistant professor of agronomy; Horace F. Major, from instructor to assistant professor of landscape gardening; O. W. H. Mitchell, from instructor to assistant professor of pathology; H. C. Rentschler, from instructor to assistant professor of physics; J. C. Hackleman, from assistant to instructor in agronomy; L. G. Rinkle, from assistant to instructor in dairy husbandry; Warren Roberts, from assistant to instructor in civil engineering.

DISCUSSION AND CORRESPONDENCE

CHROMOSOMES AND ASSOCIATIVE INHERITANCE

THE difficulties that Emerson finds in the chiasma type hypothesis are not, I think, as serious as he states (SCIENCE, October 20, 1911); and since the hypothesis appeared to meet the situation so exactly I ventured to suggest that it might be worth consideration. My brief reference to this postulated mechanism (SCIENCE, September 21, 1911) seems not to have been entirely understood by Emerson, for which the brevity of the statement, or failure to express myself clearly may be responsible, but by reference to Janssens's paper ("La Cellule," 1909) I had hoped a brief statement would suffice. In fact, the only difficulty of any weight raised by Emerson is not a difficulty at all when the chiasma type of cross union between the homologous chromosomes is grasped; the difficulty arising rather from my attempt to express in a sentence or two the essence of the mechanism described by Janssens. I said that the well-known twisting of the chromosomes giving a spiral line of separation was followed by a splitting in a single place. Emerson properly objects that if this were strictly carried out some of the genes, those at the nodal points, might be divided quantitatively. In reality according to Janssens the chromosomes break at the nodal point and unite so that the two resulting chromosomes consist of pieces (two or more) of each of the original members of the pair. If, then, the genes do not themselves split when the chiasma is formed there is no opportunity offered for a quantitative division. This is the mechanism that Janssens describes as I understand it.

A second point raised by Emerson is likewise not a serious difficulty, although we need further facts in different animals and plants concerning the nature of the chiasma type before we can speak positively about the matter. Emerson asks how if the mechanism explains the facts of coupling in those cases where some interchange must be admitted (in a case like that of *Drosophila*, for example), can we account for the purity of certain races where certain characters remain coupled and never interchange? My answer is, first, that the hypothesis was offered primarily to account for those cases where the coupling is not absolute and crossing must be admitted; and second, that whether interchange does or does not occur will depend primarily on the nature of the chiasma type; whether, for example, crossing takes place at certain levels (stations) more likely than at others or whether it is entirely a chance crossing. Until cytologists have settled this matter we may leave the question open; but the very latitude that this mechanism offers seems to me to fit the situation far better than one that admits of no such freedom; for the facts themselves are diverse. That complete coupling of several characters may exist, such as yellow, black and chocolate in mice is clear; and the result in such cases may be due to the region of the chromosome (that contains the factors for these colors) holding together as a unit when the chiasma forms; while in other cases the union between a similar series of factors may not be so close, so that crossing is more likely to take place.

As to "what has become of the 'individuality' of the chromosomes" if interchange between homologous pairs be admitted, is a matter of very small consequence; since Boveri, who is the chief exponent of the hypothesis of individuality, has long since admitted such an interchange in his definition; and since the facts of Mendelian inheritance call for such an interchange, if the chromosomes be admitted as the most likely vehicles of hereditary factors. All that my hypothesis pretends to account for is that groups of factors that enter together tend to remain together. The chiasma type appears to explain how such union may remain; perhaps some other mechanism may be found that will do as well. *The important point is that the coupling (association) of sex-limited characters that I have found in Drosophila shows that the factors must be referred to the same chromosome, and if so there seems to be no escape from the conclusion that interchange as well as association must be admitted on the chromosome hypothesis.*

Emerson has himself suggested a view to explain the remarkable cases of coupling that he has found in corn.[1] His hypothesis requires that in those cases where no interchange takes place the coupled factors lie in homologous chromosomes, while in those cases where interchange takes place the same or similar factors are contained in non-homologous chromosomes. This may seem probable or improbable, as one prefers, but in the case of *Drosophila,* where the factors in question are sex-limited and coupled with the sex chromosome, we see that his hypothesis can not hold, and that the facts can be explained without need of such an hypothesis.

[1] Annual Report Nebraska Agricultural Experiment Station, 1911.

If I might venture to point out what seems to me to be the weak point in my own view I should regard the evidence that the crossing observed in the chiasma type really takes place is by no means as yet established (see Gregoire, "La Cellule," 1910); for, while the twisting can not be doubted it is still an open question as to whether the chromosomes may untwist before the "split in one plane" appears.

T. H. MORGAN

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THE COTTON WORM

TO THE EDITOR OF SCIENCE: In connection with the correspondence of Dr. H. T. Fernald in the October 13 issue of SCIENCE on the cotton worm in Massachusetts, it may be interesting to note that there has been a very heavy migration of this insect (*Alabama argillacea* Hubn.) in the city of Pittsburgh this year. The moths began to arrive about the tenth of September and reached the maximum numbers on September 23, on which date hundreds were to be found on electric light poles and buildings in the heart of the city and passing street cars stirred up swarms from sunny places. The insects are still present (October 17) but not in very large numbers.

JOHN L. RANDALL

THE AIR BLADDER IN CLUPEA HARANGUS

IN SCIENCE (October 13, 1911) I described the air-bladder of *Ophiocephalus* and called attention to the desirability of an investigation of the condition of the posterior duct to the air-bladder in *Clupea harangus.* In this connection Dr. Gill has kindly called my attention to a lecture by Professor Huxley, published in *Nature* (April 28, 1881) in which he (Huxley) shows conclusively that *Clupea* has the posterior duct actually open to the exterior.

E. C. S.

QUOTATIONS

BENZOATE OF SODA AGAIN

THE American public believes that a question is not settled until it is settled right. This probably accounts for the fact that the sodium benzoate question will not down. And yet, although volumes have been written on this much controverted subject, the problem itself is really a simple one. There are three basic facts on which all are agreed: First, no one denies that sodium benzoate in foods may prove harmful in certain quantities, under certain conditions or when given to certain classes of individuals. Second, no one denies that foodstuffs of a high quality can be put up without the use of sodium benzoate; in fact the best food manufacturers do not use this chemical. Third, no one denies that when this chemical is used, scrupulous cleanliness and extreme care in handling are no longer necessary. These are three incontrovertible facts, admitted grudgingly or frankly, as the case may be, by both pro- and anti-benzoate forces. Under the circumstances, then, it is not irrational to conclude that sodium benzoate should not be used as a food preservative.

And now comes from Berlin the "Expert Opinion of the Royal Scientific Deputation for Medical Affairs Regarding the Use of Benzoic Acid and its Salts for the Preservation of Food." These experts were requested by the Minister of Education and Medical Affairs in Germany to give their opinion on this subject. In their report, they first describe the chemical and physiologic action of these drugs and then briefly summarize the findings of various scientists on the question at issue. Of the decision of the United States referee board, these German scientists say:

> The series of experiments in this connection made by the American scientists are of too short duration and the results coupled with certain limitations, so that they can not be regarded as demonstrating the unconditional non-injurious nature.

After considering all of the evidence on the subject the Scientific Deputation for Medical Affairs reaches the following conclusions:

> In regard to the admissibility of the use of benzoic acid and its salts for the preservation of food it is mentioned that in France on the basis of a decision of the Committé consultatif d'hygiène publique of October 1, 1888, the Minister of Justice