scope of the science of genetics and the greater possibilities which it offers.

One of the important factors contributing to progress in genetics has been the recognition of interdependence between genetics and cytology. It is only recently that the closer coordination of genetics and cytology has resulted in demonstrating that gross morphological variations may be caused by irregularities in chromosome distribution and that such irregularities may be induced in some instances by subjecting the plants to abnormally low temperatures. The hypothesis that new species with new chromosome numbers may originate through natural hybridization of existing species has been greatly strengthened by the results of recent coordinated efforts of the cytologist and the geneticist.

Regarding the allusion made by Hall and Clements[5] to Lotsy's proposed definition of a species, when they designate it as "the definition of the geneticists," so far as the writer is aware no other geneticist has proposed that gametic purity be used as a criterion for distinguishing species, and few other geneticists seem to have taken very seriously Lotsy's proposed revolutionary changes in taxonomic nomenclature. On the contrary, the general practise among geneticists is to use the specific names provided by taxonomists, and these in general are still based upon the concept of what is commonly referred to as a Linnaean species. It seems likely that most geneticists will heartily approve of the stand taken by Hall and Clements[6] in advocating the evolutionary criterion: "The evolutionary view of the species is that it is a definite phylogenetic stock, sprung from and related to similar stocks, and itself undergoing modification into a number of variads. As they have recently come from the same stock these variads are more closely related to each other than they are to those of any other species, and they represent a definite phylogenetic unit, the species, at the same time that they mark its further differentiation." It seems inevitable that the general adoption of this criterion along with the safeguards and helps of field studies and experiments, both ecological and genetical, will ultimately simplify the work of naming plants, and who more than the geneticists and plant breeders should welcome such a result?

Another aid to the recognition of interspecific relationships is found in cytology, particularly the study of the chromosomes. While this science may be looked upon as merely a phase of morphology, it is doubtful whether taxonomists recognize the promise that it holds as a means of aiding the solution of very perplexing problems in phylogeny. Thus when Hall and Clements[7] say that: "The only definite measure of the progress of evolution is the degree of morphological difference, and species necessarily share this morphological basis with other units," it is probable that they are speaking of the external morphological characters of the plant. At any rate, no reference is made by them to the characters of the chromosome group as of possible usefulness in taxonomy. This omission is doubtless justified from the viewpoint of most taxonomers on the ground that cytology is the work of specialists and is too time-consuming to be resorted to by systematists. Yet the methods of examining chromosome number and individuality in plants have been shortened and perfected in recent years, and these methods can be standardized for groups of plants so that cytological assistants could obtain dependable data on extensive series within a comparatively short time by working close to the living material. Furthermore, recent cytological investigations, especially those dealing with the series of chromosome numbers found in many plant genera, have demonstrated a definite relation between major plant groups and their chromosome groups. The chromosome group is, therefore, an indicator of phylogenetic relationship. It may seem beyond the possibility of realization in general taxonomy, but to the writer it appeals as highly desirable that certain groups at least of the higher plants should be chosen for critical investigations, combining all the means available in evolutionary taxonomy including chromosome studies and genetic analysis.

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[5] *Loc. cit.*, p. 9.

[6] *Loc. cit.*, p. 11.

[7] *Loc. cit.*, p. 11.

INVESTIGATIONS CONCERNING IMPORTED BIOLOGICAL STAINS

THE Commission on Standardization of Biological Stains has for some time been carrying on an investigation concerning domestic and imported stains. One article on the subject appeared in this journal which together with other activities of the commission has led to the impression that its members are prejudiced against imported stains. The present paper is prepared to show that any statements that may have been made are based upon sufficient evidence. Good stains and poor stains can be obtained from either domestic or foreign sources, and the principal reason why the commission has laid so much stress on the domestic articles is because of the ease with which we can cooperate with the domestic concerns in assisting them to improve their supplies.

There are certain definite objections to imported stains which apply in general to the whole supply. Some of these same objections may be raised in certain cases to American products, but they apply with particular force in the case of the foreign stains be-

cause of the prestige which such stains have enjoyed in the past. In fact, when the commission first began its work, the chief objective it was urged to attain was to bring American stains up to the standards of those available before the war. It came as somewhat of a shock to realize that no such standards ever existed; but the investigations of the last three years have shown plainly that this is the case.

In the first place, foreign stains have been found to be decidedly impure. The dye content is almost invariably lower than in the case of American stains and does not compare with that of the better stains now available in this country. This low dye content is due in part to inert ingredients that were undoubtedly added to dilute the dye and make it more suitable for textile use. Impurities in the form of foreign dyes are also often present. The presence of such impurities indicates that the stains prepared in Europe both before the war and at the present time were not especially prepared for biological work but were merely textile dyes specially packed and labeled for biological use. Another fact which has been brought out distinctly by recent investigations is that pre-war and post-war importations of the same stain both obtained from the same reliable German concern are not necessarily the same. Apparently, the only constancy that existed was that which was obtained as long as orders were being filled from a single batch. Naturally, the batches on hand at present are different from those from which orders were supplied before the war. This being the case, it is evident that at the present time different lots of stains obtained from foreign sources may vary considerably. As the firms supplying these stains are so distant it would be difficult for them to cooperate with the commission located in this country. As there is no similar body in any other country, there is no scientific control to insure constancy of these foreign stains. Another still more damaging fact which has been discovered is mislabeling. Fortunately, this did not occur very frequently; but it has been observed often enough to shake considerably the reputation of foreign stains in the minds of those who have been carrying on these investigations. A number of specific instances can be given to show the sort of objections to foreign stains that have been found.

Methylen blue is one of the stains in regard to which striking evidence of this kind has been obtained. When the work was started a sample of foreign methylen blue, denoted by number only, was distributed to several zoologists and botanists. In practically every instance this sample was given a very low rating by the investigator, and wherever it has been compared unknowingly with another sample of this same manufacture which the investigator had on hand and which had probably been imported earlier, the sample submitted by the commission was reported to be very much the inferior of the two. Samples of foreign methylen blue have been tested by the commission for their total dye content, and it has been found that many of them contain less than 60 per cent., whereas the American samples run from 80 to 90 per cent., or even higher. One instance has come to our attention which has been very surprising. A certain investigator using methylen blue to stain cartilage found it impossible to obtain American samples giving the same results she had obtained before the war. She finally obtained another sample of foreign methylen blue from the same company as that previously used and found it as poor as the American samples. She was then furnished, thanks to the courtesy of the National Aniline and Chemical Company, with several samples of methylen blue and closely related dyes. One of them proved satisfactory; but this sample was not methylen blue in reality but an entirely different dye known as *new methylen blue*. Evidently the original imported sample she had used must have contained a considerable percentage of this latter dye, and was apparently incorrectly labeled. An interesting point in connection with the labeling of methylen blue concerns that type of stain which used to be denoted as "methylen blue for bacilli." This methylen blue proves to be a zinc salt much like that used for textile purposes but too crude for medicinal use or for delicate staining procedures. In other words, the labeling did not mean that it was especially adapted for staining bacilli but rather that it could not be used for any other purpose. Present investigations show that the purer form is best for practically all purposes. The only purpose for which the zinc salt has been found to be better is not for staining bacteria but for a certain procedure in staining nervous tissue.

In the case of safranin other interesting results have been obtained. It was discovered in 1920 that the pre-war stock was much superior to anything then available in this country. One sample was obtained from abroad in 1914 which was pronounced especially fine by some investigators in this country and too yellow by others. Attempts were made to duplicate this sample in America and the results were very conflicting. It was found that this sample was not a true safranin at all, but a mixture of safranin with some other dye, probably auramin. Other foreign samples were obtained (of pre-war importation in this case), and it was discovered that they were pure safranin, except for inert material, but different from the American samples. Hence the American companies have made an effort to duplicate these, and now true safranin can be obtained in this country of considerably greater concentration than those obtained from Europe. It is still uncertain just how to duplicate

that particular foreign sample which contained the yellow dye and was found especially useful in certain cytological work. Evidently the safranin situation is complicated and can be solved only by close cooperation between dealers and users.

Eosin is another stain in regard to which extreme variation in the imported product has been noticed. A sample of this stain ordered from the usual German sources in 1914 and not delivered in this country until the close of the war was distributed to various investigators. One of these investigators received this eosin and a German methylen blue, both denoted by the number 250. These were the only foreign samples sent him, but he compared them with some samples of his own imported from the same German firm before the war. In regard to them he replied: "The thirteen dyes sent us . . . have been examined and I think that with the exception of two—eosin 250 and methylen blue 250—excellent differential results were obtained after fixation in Zenker's and formol solutions." This same eosin was sent to a bacteriologist who found it so much inferior to any pre-war importation and to the American samples sent him that, when he learned its origin, he said that he did not see how possibly any two American samples could differ from each other to a greater extent than this sample differed from those which he had on hand imported before the war.

A very interesting illustration of the variation in foreign samples of stain is in the case of a rather rare dye known as cresyl violet or cresylecht violet. A paper on this subject recently appeared by Williams.[1] This work was done in cooperation with the commission, and the samples which he used were obtained by the commission for him. His article reports decided differences between two foreign samples of this dye. One of these, which he denotes "original batch," was obtained before the war. The other, which he calls "2nd sample," was obtained after the war from the Will Corporation, but was known to be derived from the same German source. He reports that the two samples each seemed to contain a blue and a red dye in different proportions. The 2nd sample contained so much of the red dye as to be unsatisfactory. He compares them with a National Aniline sample, which proved not to contain this red dye, but to give very satisfactory results with his technic. Spectrometric analysis has proved the general truth of his conclusions. The results were so interesting that they will probably be published separately later in another paper. It is very evident from analysis that his "original batch" was a mixture of two dyes, one of which was actually cresylecht violet. His "second sample" was apparently the wrong one of these two dyes; while the National Aniline sample was apparently pure cresyl violet.

Methyl green has proved a very interesting stain in the course of the present investigation. At the start it seemed a very discouraging stain to work with. A set of several samples was distributed to different zoologists and botanists in 1922. Most of these samples were of American origin, but two of them were imported samples which had been supposed to be the same as pre-war supply. As the reports concerning these samples began to come in it was soon discovered that none of them were to be regarded as satisfactory. The imported samples were better than the domestic samples, but apparently not what they should be. One investigator in particular, without knowing the origin of these samples, reported them "not so good as Grübler's." Such reports showed that although the American methyl green was plainly unsatisfactory at that time the foreign supply could not be relied upon to be constant. The question of preparing a satisfactory methyl green was put squarely up to the American producers with the result that in a comparatively short time the National Aniline and Chemical Company had prepared for us the purest lot of methyl green we had ever examined. Unfortunately, however, this very pure sample did not have the proper staining qualities. It seems there are two important factors in obtaining a satisfactory methyl green. In the first place, it must be *methyl green* instead of methylene green, and in the second place it must *not* be completely free from methyl violet. Methyl violet is a dye so closely related to methyl green that it is almost impossible to free the latter dye from it in the course of manufacture. No pre-war sample was ever free from methyl violet, and to this impurity was due the metachromatic feature of methyl green with which histologists were familiar. Naturally, therefore, the very pure methyl green just mentioned did not have such staining properties. The necessity of this small amount of methyl violet may have been known in Germany before the war, but inasmuch as the ordinary textile dye contains methyl violet the probability is that the presence of this other dye was due to accident rather than intention. At the present it appears that good results may be obtained by intentionally adding a small amount of methyl violet to a pure methyl green. It has also been learned that a reliable methyl green can be now obtained from at least one or two American sources. It should be possible now that the exact facts of the case are known to maintain a supply of this stain of more constant quality than was available from foreign sources before the war.

The conclusions that have been reached as the re-

[1] B. G. R. Williams, "Cresylecht violet, a rare dye," *J. Lab. and Clin. Med.* 8, No. 4, Jan., 1923.

sult of this work are: first, that imported stains available before the war were not necessarily constant because they bore the name of the same firm; second, that there is no evidence that stains imported to-day are the same as those obtained before the war; and third, that if the quality of stains is to be kept under scientific control, there is much more promise of doing so with the cooperation of domestic concerns than through dependence upon the foreign market.

H. J. CONN, *chairman*,
Commission on Standardization of Biological Stains

GENEVA, N. Y.

SCIENTIFIC EVENTS

PAUL EHRLICH

A BOOK on Paul Ehrlich as a man and a worker has been published to celebrate the seventieth anniversary of his birth on March 14, 1854. *The British Medical Journal* writes:

The book reveals very clearly that Ehrlich was a true genius. He conformed to very few or no rules, and performed his experimental work with the very simplest of apparatus. His laboratory contained a large table covered with endless bottles of reagents, with a Bunsen burner and a few test tubes in one corner; this constituted practically the whole of the apparatus which he used. Ehrlich had apparently a special kind of intuition which enabled him to divine from simple experiments the most profitable lines of further research. It must not be supposed, however, that his methods in any way resembled guesswork. He read enormously and very rapidly, and possessed the power of extracting with great speed from any new book those points which were of real importance to him. It is interesting to note that he considered the acquiring of unnecessary knowledge as actually undesirable. He experimented continuously, and tested and retested every result with the most scrupulous care. "Much work, little publication, and no preliminary communications" was his motto, and he followed it exactly. For example, salvarsan was discovered in 1907, but the discovery was followed by two years of careful animal experimentation before the drug was tried on man, and, as is well known, Ehrlich first gave it out to hospitals for testing clinically under carefully controlled conditions. It is interesting to note that 65,000 samples were distributed gratis by Ehrlich in this way. It was not until 1910 that the clinical results of his work were published, and it was only after these years of trial that the drug was put on the market. The whole procedure may well serve as a model for future workers.

The discovery of salvarsan was a result of immeasurable practical importance, and it is very instructive to learn that Ehrlich insisted that the discovery came as a mere by-product of a research started with purely theoretical aims.

Many characteristic anecdotes regarding Ehrlich are told in this book. One of the most amusing is the account of how he was asked to give the Herter Lectures in America in 1904, and for many months previous to the voyage he refused to see visitors on the ground that he must prepare his lectures; Professor Reid Hunt told him one day that he was certain that the lectures would actually be written on the steamer; later, in America, Professor Reid Hunt happened to visit Ehrlich an hour before the first lecture was to be given; Ehrlich turned to him and said, "You did me a great wrong when you said that I should write my lectures on the steamer; I did not do so, I have only started to write them now."

THE AMERICAN SCHOOL OF PREHISTORIC RESEARCH

THE American School of Prehistoric Research in Europe, which is affiliated with the Archaeological Institute of America and the American Anthropological Association will begin its fourth year on July 1. Qualified students of both sexes are admitted from both North and South America; enrollment may be for the summer term or for a longer period. There is no tuition fee, but contributions to the funds of the school by those who can well afford it are welcome.

The work of the summer term consists of excavations sufficient to give the students first-hand knowledge of methods and culture sequence; the study of museum collections; lectures by the director of the school, also (at various places) by distinguished European anthropologists; and excursions to the most important Paleolithic, Neolithic, Bronze and Iron-age sites.

The tentative program is as follows:

Southern England—London; Ipswich and Grime's Graves; Avebury and Stonehenge.
France—Somme valley; Paris; Saint-Germain; Brittany.
Belgium—Liège (French Assoc. for the Adv. of Science); Brussels.
Holland—Amsterdam; Haarlem (*Pithecanthropus*); The Hague (Intern. Congr. of Americanists, 1st part).
Denmark—Copenhagen.
Sweden—Göteborg (Intern. Congr. of Americanists, 2d part); Stockholm (if time permits).
Germany—Berlin; Halle; Leipzig; Jena; Weimar; Stuttgart; Tübingen; Constance.
Switzerland—Schaffhausen; Zürich; Berne; Bienne; Neuchâtel; Geneva.
France—Lyons; Roanne; Solutré; Aurillac; Brive; Périgueux; Les Eyzies (and the stations in the Vézère valley); Charente (Angoulême and La Quina).

After consultation with the director, students may choose for the winter term the center of learning which may offer them the best facilities for the working out of the problems in which they are interested.

Applications for admission to the school should be sent to the director, Dr. George Grant MacCurdy,