fields for cooperative investigation, and this is being fostered by research committees of organizations like the National Research Council. However, there is another direction in which more effective organization is possible within the universities themselves!

Departments of a somewhat more flexible nature than those to which we are accustomed and which could, more than now, be built around one or two outstanding men in the department, could give these men an opportunity for organization and concentration of effort which is now rarely possible. This would, of course, require careful selection of men. In this matter of organization of departments around the most productive and outstanding men, of taking for granted that they will have research assistants to increase the efficiency of their labors, of selection and recognition of men on the basis of merit and promise rather than seniority, and of wise procedure in the selection of men to fill important posts, America is far more backward and bound by tradition than are those European countries in which scientific achievements have been most rapid. It may surprise you, for instance, as it did me, to learn that in America, the land of wealth and opportunity, there is no university which is able to offer a salary equal dollar for dollar to salaries which universities even in war-ridden Germany will offer to secure the outstanding men. As is the tendency in other things American, our tendency to standardize, which is so useful in some directions, is interfering with our ability to recognize, secure and do our best. This situation in our universities is, I believe, a grave one if we set as our ideal the best possible achievement.

In these remarks I have attempted to suggest some of the accomplishments and opportunities of research and to indicate some of the directions in which we may hope to bring about even more fruitful service of science to society in the future.

This, gentlemen, is the situation. It is a situation that calls for serious thought and constructive action. The things which I have been able to say are not new, but I sincerely hope that you may find in them reasons sufficient to enlist your sympathy and active support of any movement which has for its purpose the better service of science to our country and to humanity.

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SOME PROBLEMS IN BOTANICAL CLASSIFICATION[1]

THE systematic botanist (or taxonomist as he is often called) has a double duty. First, he must give each apparently new plant a name which other workers in plant science may use in describing their experiments with the plant or in drawing conclusions regarding plant distribution and so on. This name must designate the plant's relationships with approximate accuracy and is based on a close study of gross morphological characters. By that I mean such characteristics as shape of leaves, color of flower, number of flower parts and their arrangement—in fact such physical properties, to use a chemical phrase, as can be determined fairly readily with the naked eye or by aid of a small lens. The degree of resemblance of the total of these characters, and especially as regards those of the flower and fruit, between plants has served as the criterion in judging the degree of relationship of the plants. This type of taxonomic work will probably always be based on the same method. It is worth noting here that there still are enormous areas of the earth's surface, the vegetation of which remains comparatively unknown despite the flood of taxonomic publications during nearly two centuries, and these areas offer an attractive field for what I may call this "preliminary naming."

Second, he must revise his conceptions of plant groups in accordance with the progress of work in other botanical fields and such discoveries as are constantly being made that tend to show more clearly the exact relationships of the plants involved.

Of chief interest to us is this second phase—namely, the correlation of taxonomic work with other botanical work and the revision of opinions that necessarily results from many-sided, intensive investigations. (The preliminary naming of the cultivated fruits and vegetables of the north temperate zone has long since been completed.) Taxonomists have perhaps been a little slow in the utilization of other workers' results; but the last decade has seen started a considerable number of new lines of systematic investigation based on the work of plant physiologists and chemists, geneticists and cytologists.

It is, I believe, the correlation between the work of the geneticist and cytologist and that of the systematic botanist that offers the most promising line of attack on the age-old problem of clearing up the relationships of cultivated plants. This problem is of vital interest to us at this station.

In the Division of Horticulture, we have been particularly interested in fruits and vegetables, and, at present, some typical questions of considerable moment are: What are the true species of apple—or, in other words: What are the fairly stable natural groups of the genus Malus? What species are we working with in our attempt to breed better fruits? What are the characteristics of each of these entities? As you may readily perceive, the answers to these

[1] Extract of address before the June staff meeting of the New York State Agricultural Experiment Station, Geneva, N. Y., June 6, 1927.

questions are fundamental to a clear portrayal of breeding problems, and if these answers can be made with some degree of completeness, the formulation of breeding experiments with apples will proceed with greater facility and with increased probability of reaching favorable results. The same questions may be asked in regard to each group of cultivated plants and will have the same bearing on experimental work with them.

This taxonomic problem we are now attacking by a new method which has already shown promise in the case of the allied genus of Roses. The preliminary announcement of this work by Hurst just four years ago constitutes the first attempt to untangle a complex group of cultivated and wild forms by reference to cytological evidence.

At this point it may elucidate the situation somewhat if I review briefly Hurst's discoveries in Roses.

The inception of his work seems to have been more or less accidental. He says that one day while comparing the taxonomic characters of certain species of roses at Kew, he was struck by the fact that a species he knew to have 28 chromosomes showed the combined characters of two distinct species having 14 chromosomes each, while one containing 42 chromosomes showed the combined characters of three distinct species of the smaller number of chromosomes, and one with 56 showed the combined character of four of the others.

Further investigation of about 400 forms of Rosa have revealed 5 distinct species of 14 chromosomes each and have shown about 50 different taxonomic characters associated with each. Hurst thinks there may result over 200 commutations and permutations of these characters, and he has identified a large number of these.

Now to consider some of the possibilities arising from the uniting of germ-cells of different constitution—that is, of those containing chromosomes of different make-up, or of different numbers. It seems from such evidence as is at present available that a union of germ-cells will not take place if their constitution is too divergent. However, it also seems patent that, provided the germ-cells are very largely similar in content, a union may take place even though the number of chromosomes differ. For example, germ-cells of a species of Rose containing 14 chromosomes will sometimes unite with germ-cells containing 21 chromosomes (of another species of Rose, manifestly). In the subsequent body or somatic division of cells (as has probably already suggested itself to you) the 35 chromosomes may not divide evenly; and certainly in reforming germ-cells there can be no even division of an odd number.

The plants which arise from such combinations of germ-cells and all their descendants naturally vary a great deal in their characteristics. It is my personal belief that 99 per cent. of the difficulties which confront both taxonomists and plant breeders are due to these plants whose exact constitution can not be predetermined nor indeed determined with at all complete accuracy by use of any of the older methods.

With the foregoing considerations in mind I started this spring to attempt to unravel the species of apple by aid of such cytological and genetic evidence as might be obtainable. Dr. Bernhard Nebel, of Halle a. S., kindly took upon himself the cytological work involved. There has been almost no cytological work done on the so-called species or hybrids of Malus, nor indeed on apples at all, except for a few recent papers on the relation of chromosomes to fertility of pollen.

It is too soon to speak with much confidence as to what has been found or as to what bearing these discoveries may have on the problem; and what I have to say now may upon careful study have to be modified considerably. However, these seem to be the results and their significance at present.

Several of the plants which I believe to be good species, that are fairly stable associations of genetically closely allied plants, show 14 chromosomes in the nucleus, others show 28. These plants also show nearly regular nuclear divisions. This would seem to indicate that apples may show (as roses undoubtedly do) that good fundamental species will have either 7 or some multiple of 7 as the basic number of chromosomes. Whenever we find, as in the case of *Malus Scheideckeri*, a variable number of chromosomes and very irregular division, I think we may assume that we are not dealing with a good species but with one of the variable types of hybrids. Our work is not yet far enough along to discuss further results.

It may be interesting, however, to point out some purely speculative possibilities.

If 7 pairs of chromosomes is the fundamental number in apples, then there are probably several good species containing sets of 7 pairs each possessed of somewhat different characteristics. A union of germ-cells between two such species might give rise to another good species and undoubtedly has done so. Moreover, apple species of 21, 28, or 35 chromosomes may exist—according to one investigator—and combinations between these have probably produced other good and valid species. *But* a combination between a species with 14 chromosomes and one with 21 might and probably has produced a very variable series of offspring.

Just how far these possibilities may fit with the facts as shown by results of experimental work done on apples is now a part of the problem.

I refer to "experimental work done" because for a number of years it will be necessary to utilize all of the data obtainable from experiments that have been tried with other aims in view. This is due to the facts, first, that no attempts have as yet been made by geneticists to breed apple species for the specific purpose of determining their taxonomic position in relation to each other; and, second, that apples are so slow in maturing that it takes half a life time to get three generations. Perhaps I should have put the statement the other way about and said that man lives so short a time that he can manage to see only 3 or 4 generations of apples in his life. Of course, these statements also apply, with more or less aptness, to other perennial fruit crops. The vegetable crops offer a more satisfactory field in point of their shorter individual lives and greatly increased number of generations per given time. These, too, we hope to investigate in the same way.

This whole situation leads much farther than the naming of types of plants. It promises, I believe, a tremendous advance in taxonomic accuracy for the very reason that it may give us a clear-cut definition of a species. Heretofore, a species has always been a concept and no two taxonomists have agreed entirely in their specific concepts. The more variation there is in a group of plants, the greater the diversity of concepts existing among specialists on that group. In cultivated plants, because of the tremendous variations induced by men by selection, crossing, and removal of natural competition and other difficulties in what might be called the "normal life struggle" of the plant, this diversity has been increased many fold. The remarkable thing, it seems to me, is that there exists as much agreement in specific concepts as there is. This new method seems to promise an approximation, at least, to mathematical accuracy. Given a definite number of pairs of chromosomes and a perfectly regular method of cell division allied with a distinct group of other morphological characteristics and we have a good species. Given an indefinite number of chromosomes and irregular division, we haven't a good species but a hybrid of sorts, characterized in every case investigated thus far by considerable variation in gross morphological characters.

If, as I have assumed, the greater part of the difficulties lie in the varying concepts regarding those plants which are not *good* species according to my definition of a moment ago, then a clarification of the status of those plants by reference to their cytological behavior will go a great ways in stabilizing nomenclature of plants in general. Perhaps, I should have said "higher plants," for I fear this investigation of chromosomes will be of little use to the bacteriologists who have yet to find such things in their whole category of delightful pests. However, the bacteriologists by their use of physiological reactions have gone further, I believe, in attaining accuracy in delimitation of related forms than the systematic botanists who deal with seed plants and trust to their eyes alone to determine differences and likenesses.

This may lead to some difficulties in terminology involving among other things the uses of such words as hybrid, cross, variety, form and species. It seems to me that the term hybrid will have to be held merely for those results of crossing in which the ensuing nuclear divisions exhibit irregularity of behavior coupled with varying degrees of sterility. And it is to be noted that so far as known at present some degree of sterility is *always* associated with this irregular chromosome behavior. It seems likewise evident that when the result of a cross is an organism with perfectly regular nuclear division and complete potential fertility, it must be reckoned a good species regardless of whether it has been found wild or is known only under cultivation.

I believe I have exhausted my time and possibly your interest. My own interest, I have to confess, is white hot. I feel that we are in view of some striking advances in taxonomic work and it is a rare pleasure to be in at the very beginning of the adventure.

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SCIENTIFIC EVENTS

THE INTERNATIONAL CONGRESS OF ENTOMOLOGY

THE Fourth International Congress of Entomology is to be held in August, 1928, at Cornell University, Ithaca, N. Y. Previous congresses have met at Brussels (1910), Oxford (1912) and Zurich (1925). Every important interest—educational, scientific and economic—will be provided for in the program. Invitations have been forwarded through the state department to foreign governments to send representatives and later invitations will also be sent to the individual entomologists. A program will be arranged in which some of the leading entomologists of the world will take part. It is planned that in the forenoons throughout the week papers of general interest are to be read before all members of the Congress. In the afternoon sections will be formed dealing with (1) taxonomy, distribution and nomenclature; (2) morphology, physiology and genetics; (3) ecology; (4) medical and veterinary entomology; (5) economic entomology with its subdivisions relating to forest, fruit, vegetable and cereal insects, bees, insecticides