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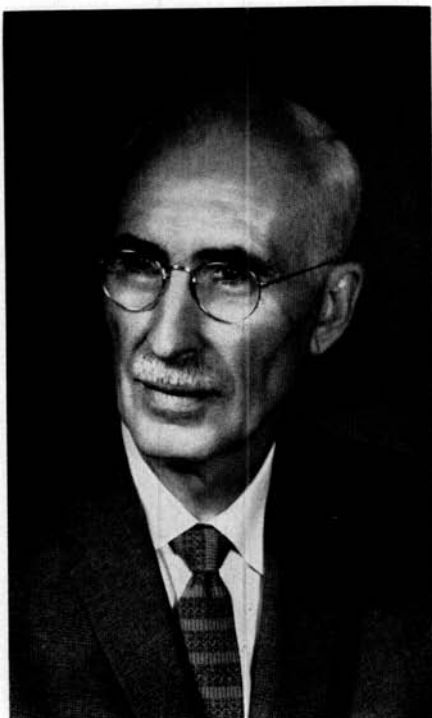
THIS I WOULD LIKE TO KNOW—

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At the 54th Annual Meeting of the Illinois State Academy of Science, the introduction for this address was sounded. Dean Hobart Heller at Eastern Illinois University, acting in the absence of President Quincy Doudna, said that there had been a number of well known scientists who, at one time or another during their careers, had been at Eastern as students or professors. The Botanists present thought of such as E. N. Transeau. He gave an address, as retiring president of the Botanical Society of America, under the title of "The Fifty Golden Years of Botany." It is the failing of retiring presidents to make speeches that act as summaries of a lifetime of activities in that president's field of endeavor, or a history of the organization being addressed. The history of the Illinois State Academy of Science is being written. It is not my desire to anticipate that work.

Those of us who are in the teaching of science use this method as an introduction to a science, or as we say "To give the historical prospective to our subject." This may be done so well and completely that the students get the idea that all the problems are solved, that all the answers are given, and they will turn away from science to other fields



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where there is still some of the unknown to be found. This is an overstatement and is not necessarily true of all sciences nor of any particular science but it may be true of a particular scientist in a particular science.

Part of the function of an instructor before a class should be to raise doubts and questions in the minds of his students as well as to give answers to questions and to solve problems. This asking of questions is the introduction to research. It was with this in mind that the title "This I Would Like To Know—" was chosen. It will be evident at once that this title is wrong and it should read "These I Would Like To Know—".

Some of you will remember that the 1955 Annual Meeting of this Academy was held at Southern Illinois University in what was then the new Life Science Building. Around that building were mounds of mud that the landscape architect had left. The remark was made that the architect was attempting to hasten organic evolution. When asked "How?" the answer was "By providing an environment that might be conducive for the development of mountain goats with webbed feet." A member of the staff of a sister science was heard to growl, "It would be well for him to stick to his own field." I shall try to heed that admonition.

What is hinted at here is that others of the biological sciences have advanced much farther in the explanations of the development of tissues and organs than have the plant sciences. These explanations cannot be applied to plants without extensive modifications. I shall try to stay with the study of plants.

The questions asked here are not especially new. Some of them were asked by the first persons who examined plants and plant structures. Some of the questions

were "Where?," "When?," "How many?," and "How come?" or "Why?" The "where" and "when" have been rather well investigated and recorded. The "how many" is easily counted, analyzed, and recorded but the "how come" or "why" may still elude us.

To many students of the structure of plants, the idea that simplification or reduction of a structure, as an advance in the development of an organ or a plant, is presented. This idea is illustrated in reproductive organs, vegetative organs and in the number of chromosomes in the cell. One example might be the reduction in the number of neck canal cells of the archegonium as seen in such plants as those of the *Bryophyta*. The greater number of cells in the neck canal of the Mosses means that in this character the Mosses are more primitive than the Liverworts. How did the Mosses get the longer neck and greater number of neck canal cells? Those plant scientists who have proposed the theory that Mosses are more primitive than the Liverworts have had trouble with this. The Mosses could have, by reduction of numbers of cells, produced a neck and neck canal of the Liverworts but how did the Mosses get the greater number in the first place?

A similar situation is found in the theories that explain the structures of the stems of plants. We teach that those plants that have a reduction in the amount of secondary growth are more advanced in this characteristic than those plants which have a greater amount of secondary growth. The theories of

simplification and reduction of tissue or organs are rather well known but the enlargement of an organ or tissue, or the theory of accretion, is seldom expressed.

One of the usual ways in which to explain the larger structures is to say that over a great period of time these structures, organs, or tissues are built up. It does not take a student of the plants of the past very long to discover that the plants with the larger and more complicated structures, organs, or tissues had their origin about the same time as those with reduced structures. One attempt to review the information we have on the accretion theory was made by a member of the Illinois State Academy of Science, Wilson Stewart (1960). He proposed that more than one stem was involved in the development of the stems of some fossil plants. Several stems came together to make one stem with more than one stele. These separate stems might at first be just that, separate stems growing very close together. Later they might "fuse" making a stem with several steles. Still later the structure of the stele may be altered until only the vascular bundles are left and a stem appears with many vascular bundles in what is apparently only one stele. Thus the theories of simplification and reduction come into play. But first, the theory of accretion is necessary to get the larger structures that can be reduced. Other attempts should be made to account for the ideas of simple vs. complex or rather from the complex to the simple or reduced in plant structures.

The greater number of chromo-

somes per nucleus of the cells of some of the Ferns as compared with the numbers of chromosomes in the Angiosperms has caused wonderment. It has been proposed that there has been a doubling or at least an addition of chromosomes to build up a greater number, but here our timetable seems to be in reverse again. The plants with the higher number of chromosomes seem to have been the ancestors of the plants with the fewer chromosomes. How can a plant lose chromosomes without losing the genetic information necessary to keep it alive?

Techniques are now in use that will help us identify the chromosomes of different plants that had a common ancestry. The chromosome number, 256, of some of the Ferns seems to be out of line with the reduced number, 16, of some of the Angiosperms and they bear little similarity to the chromosomes of the Flowering Plants. Do the Ferns require so much more genetic information that they have to have so many more chromosomes than the so-called higher plants? Or will the size of the chromosome make up for the lower number in the Angiosperms?

"This I would like to know—" How does differentiation take place in two sister cells such that they produce different tissues or become a part of different tissues. The sister cells come from the same mother cell; presumably they have the same heritable characteristics. Some contemporary botanists theorize that there is enough genetic information in the make-up of the chromosomes to provide all the possible moro-

phological variations. This may be an oversimplification that will here lead us to obscure some of the details of differentiation. Would not both daughter cells of the same mother cell have the same genetic information under normal conditions? One cell may give rise to very different cells and tissues than its sister cell. Then is there some intrinsic factor which will cause a difference? We have not been able to show that they receive different chemicals from the environment, externally. It does not seem that the position of the cells can account for the difference.

Examples of the differences in cell behavior are pointed up in the work on Junipers by Margaret Kaeiser (1960), as reported in the *Transactions of the Illinois State Academy of Science*. Here not only were different tissues produced from the derivatives of the same mother cell but in different amounts.

As Kaeiser points out, some buds of a leafy shoot produce elongated stems and other produce the dwarf stems. Others have shown, in some plants, that the removal of the apical bud may cause the dwarf laterals to assume the characteristics of the bud that produces an elongated branch. Is there a reversal of genetic information here or will other intrinsic or extrinsic factors be responsible for this change? When will this reversibility cease? Is there, again, chemical or physical factors involved that we do not know?

The first person to work out a mathematical explanation of the arrangement of the leaves on a stem was Leonardo Da Vinci (Welch,

1933) over 400 years ago. He saw "where," "when," and "how many." But "why" he did not answer. Since Da Vinci's time others have found the phyllotaxy is a family characteristic and that it is transmitted from parent to offspring. This would indicate that genetic information is responsible for the arrangement of leaves on a stem. Much work has been done on many plants of many families with some clear results. We know what cells are involved with the first development of anything that looks like a leaf—the leaf primordium. But why these cells and not the cells next to them? Why these cells and not those that are 4 or 5 off to the right or left? Here again we may have to go beyond our ideas of what is inherited in order to explain these cell differences.

The work with vegetative buds has had its counterpart in work with flower buds. And right away we could ask the question — Why are flower buds ever produced? In this Academy Barbara Palser (1958) has presented several papers with her students. Here, it has been pointed out, there are great differences in the development of the cells that go through microsporogenesis and those that go through a megasporogenesis. Yet these cells had their inheritance from the same cells and tissues. These original tissues were developed from the same cells that produced the vegetative organs. How do these certain cells in the flowers of the Heath family differ from all other cells that these will produce microspore mother cells and megaspore mother cells but no other cells will

produce similar initials? The genetic information may be the same in all cells. Then is there some other factor necessary? Here again we ask is there an intrinsic factor involved? Is this factor in the cell itself or is it from surrounding cells?

Some research is being carried out by a member of this Academy to see if certain chemicals can be made to perform as a substitute for an intrinsic factor or, in this case, be an extrinsic factor. Are the growth regulating substances the excitants that will initiate morphogenesis in plant structures? The point is initiate. We know that the auxin related growth regulator substances will enhance the development but will they initiate the development of cells, tissues, or organs? This is one of the things this researcher is attempting to illustrate. He is using growth regulating, auxin-like chemicals on woody cuttings to determine what happens when adventitious roots are developed.

There has been some discussion between plant physiologists and morphologists as to whether the growth centers are already present or developed by the auxin related chemicals. There is no clear evidence one way or the other. If there was sufficient evidence, we would not have the question. The work cited may not answer the questions but this and other research may bring us nearer the answer.

"This I would like to know—" What initiates cell division? Many of the cells of the meristems of plants seem to have all that is necessary to support mitosis or for that matter — meiosis. There seems to be enough

food. The structure of the nucleus and cytoplasm seem to be such that mitosis and cell division could take place. Then why don't these cells divide more often or sooner than they do? Some cells of a meristem have established a rhythm that has been recorded. Others have not or at least it is not reported. Is the rhythm or lack of rhythm determined by some chemical that needs to be brought in to stimulate the activity? There seems to be enough of all chemicals necessary including enough of the ribonucleic acid. There seems to be more than enough deoxynibonucleic acid present to provide for all the chromosome material needed. In fact for some theory-makers there seems to be too much DNA in the "resting cells."

Then why does the cell not divide sooner or more often? One of the popular explanations for such activities, control or rhythm, is that there is an inhibitor present. No activity can take place until this inhibitor is removed or inactivated.

A member of this Academy is attempting research on the process of mitosis to see if there is, indeed, an inhibitor present and if it is, can it be inactivated. There is some evidence that some chemicals are present in the "resting cell" that will disappear as the cell divides. Are these inhibiting substances? Attempts are now being made to make the suspected inhibitor disappear. If it can be made to disappear or be inactivated, then it will have to be determined if cell division is speeded up.

These are some of the things I would like to know. You may have

some of the answers; others will come as the result of research. And this research will be replaced by still further research.

The first requirement of this research should be an idea. The idea should be dealt with with imagination. Too often we are apt to let a piece of equipment do all the interpretation of the information gathered. Too often the measure of the stature of a man in the scientific community is the measure of the number of dollars he can bring in from outside foundations. Those who assign, assist, direct or otherwise encourage research should encourage the use of the idea equally, if not in greater measure, than the use of equipment.

Research will go on continually. Research is as much of a compromise as is the passing of time and it can't be stopped. If the philosopher can say "today is a compromise between

yesterday and tomorrow," then we can paraphrase to say "we teach today what we did not know yesterday, only to have it disproved tomorrow." Such is our imperfect state of knowledge. We can hope that with the results of research we will teach fewer falsehoods tomorrow than we teach today.

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