Fashion in Cell Biology

The motives that prompt us to follow fashions in research are various and not always estimable.

Honor B. Fell

I published my first paper in 1922, nearly 40 years ago. To an astronomer or a geologist this might not seem a very long period, but to a cell biologist of 1960, 1922 is prehistory, and the customs official at New York was perfectly right. He asked me what I did for a living; I replied that I was a biologist and studied cells. "Gee, doctor," he said, "I guess you've seen plenty of protoplasm in your time!"

One day I was casting my mind back over my long protoplasmic past, searching rather desperately for a suitable topic for this address. It suddenly struck me what an important part fashion had played in the development of our science during the past 40 years, and it occurred to me that this would make a rather suitable subject for a female president at a Paris congress. So this is what I am going to talk about: fashion in cell biology.

Sartorially speaking we are probably not an outstandingly fashionable group, but where our research is concerned, we can be as fashion-conscious as the most elegant woman in this city.

The effect on scientific progress of this deference to fashion is quite interesting, and I propose to consider the various ways in which it operates, and whether on the whole its effect on our science is good or bad.

Fashion Designers

Sometimes scientific fashions are set quite suddenly by individual research workers. These fashion designers are, of course, a very small minority of the scientific population and are of different types. A few set fashions because they are endowed with a creative intelligence that is far above the common run (the late Ross Harrison was one of these); by the time their achievement has become fashionable, they themselves have usually lost interest in it and gone on to something else. Some have unusual technological talents and devise a technique that opens a new gateway to knowledge. But a fair proportion are quite ordinary people. For example, one of us may have one of those gorgeous strokes of luck that occasionally reward the observant and accidentally stumble on something new and exciting that attracts general attention. Or he may be gifted with unusual plausibility and be well versed in the art of scientific salesmanship. Or again, he may belong to that small group who are not fashion-conscious but pursue their own way, heedless of the prevailing mode; then one day such a person may find that for one reason or another his work has suddenly caught on, and behold! to his delighted surprise (for he is only human) he too becomes a leader of fashion.

But usually fashions develop more slowly. Sometimes information or an idea has been available for years before some astute and enterprising person realizes its significance and places it in the public eye. The history of research on the nucleic acids is an interesting example of this. According to Hughes's *History of Cytology*, nucleic acid was first discovered by Miescher in 1871. Work then proceeded steadily but without attracting very wide interest until the 1930's, when the subject rapidly became extremely popular. Its popularity was mainly due to Caspersson and his group, who not only appreciated the enormous importance of the nucleic acids in cell physiology but applied new methods to their study.

Sometimes, for no obvious reason, an important observation is ignored for years. As an example of this I will mention "pinocytosis," which was discovered in 1931 by W. H. Lewis. He writes: "Certain cells in our cultures, especially the macrophages, take in globules of fluid from the surrounding medium." Lewis not only observed the phenomenon in both normal and malignant cells but was thoroughly aware of its significance and points out that "by pinocytosis the cells are able to take in substances which cannot diffuse into them or be taken in by ordinary phagocytosis of semisolid particles." But for 20 years or more pinocytosis remained "unhonoured and unsung" by cell biologists, until Holter of Copenhagen reopened the subject. Nowadays references to the phenomenon are constantly encountered in the literature.

Those who initiate fashions in research do not always receive their just reward. Sometimes a good idea or a new fact is not accepted when first published. The author may be subjected to severe criticism and have to fight for his brain-child. Then subsequent investigations show that after all he was right, his idea or his discovery becomes widely accepted, a large literature grows up around it, and finally nobody remembers who originally thought of it. Those who set fashions in technique often fare better, because their names usually get attached to their methods, since this is a labor-saving device for later authors.

Recurrence

In science, as in the world of dress, fashions recur. For example, at the turn of the century, cell biologists were much exercised in their minds about fixation artifacts. How far did the structure seen in preparations of fixed and stained material represent a true picture of the living cell? In particular they were concerned about the texture of the nucleus and cytoplasm. In the hope of shedding light on this question, Alfred Fischer in Germany and Sir Wil-

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liam Hardy in Cambridge studied the effect of fixatives on various types of colloidal solutions. Then in the 1920's the critical study of living cells began, and interest in the problem of fixation artifact largely lapsed. But when electron microscopy made its appearance, all the old problems of fixation artifact reappeared on the scene, and once more we find people industriously studying the effect of fixatives on colloidal solutions. But this time we are in much deeper water, because in the foreseeable future there is not much hope of our being able to compare living and fixed cells at the highest magnifications available with the electron microscope.

Even philosophic concepts may recur, though in a different form. Earlier this year I had the pleasure of attending the Growth Symposium at Brandeis University. After one of the sessions a lively discussion developed, and the chairman wrote on the board the subject of this debate. What he wrote was: "Preformation versus epigenesis." However, the meeting was not really concerned with the problem of whether the sperm contained a homunculus. If I remember rightly, on this occasion the preformationists held that tissue differentiation is achieved by the deletion or inhibition of all but a certain selection of the genes originally present in the fertilized egg, whereas the epigeneticists preferred to think that the nucleus as well as the cytoplasm undergoes a progressive differentiation and acquires new properties during embryonic development.

There is one form of recurrence that is wholly regrettable, and which is one of the unfortunate consequences of the vast expansion of research and the monstrous and unwieldy literature that it now produces. I will mention a small example of the sort of thing that I have in mind. In the 1930's some of my colleagues did a rather extensive series of experiments which they duly published. A few years ago, an account of an almost identical research with the same results appeared in one of the journals, but with no mention of the earlier study. One of my colleagues wrote and pointed this out to the author, who replied that he never quoted any literature prior to 1946.

One had a certain sympathy with the younger man, who was working in what is now a densely crowded field. But this is a situation that is bound to get steadily worse, partly because of the legitimate increase in publication due to increased output of information, and partly owing to multiple publication of the same information, a crime of which I am afraid most of us are guilty in one way or another. Our scientific world is becoming like a crowded cocktail party, in which everyone shouts a little louder in the hope of making himself heard, until at last the volume of speech is such that almost nothing can be distinguished.

In the future, the scientific historian is likely to become a person of increasing practical importance. At present we tend to think of him as a pleasant, scholarly type whose works we enjoy reading in our leisure hours, for relaxation. But I foresee a time when he alone will be able to save research from progressing like a stage army, with the same old investigations coming round over and over again—briefly fashionable, and then forgotten until next time.

A Survey

I find it interesting to look back over my research life and consider the various fashions in cell biology that have come and gone and sometimes come again. It is odd to realize that during this period only the select few really thought of the cell as an intact, functional organism. Always, it seems to me, we concerned ourselves with one or another part of the cell, but seldom with the whole. When I was young, we concentrated on the cytoplasm. We studied the Golgi apparatus, which we demonstrated by silver impregnation as an elegant network; if we did not find elegant networks in our preparation, we threw the slide in the refuse bucket. Was the Golgi apparatus a reality or an artifact? The deepest passions were stirred by this question.

As a result of the rapid advance in genetics, the general interest shifted to the chromosomes. The shape, size, and number of the chromosomes were meticulously studied in a wide range of plant and animal species, but the physiology of cell division and the role of the nucleus in the cell's everyday economy received much less attention. Eventually cytogenetics passed its peak of fashion. Cell structure was more and more disregarded, and the reign of the homogenizer began. Cells were savagely disembowelled, and people hurried to investigate the biochemical properties of the isolated organelles. Surprisingly (to a morphologist like myself), a lot of interesting and valuable information emerged from this holocaust. But naturally the results were one-sided, and the risk of physiological artifact was considerable. Who could tell how far the functional activities of a mitochondrion sitting cozily in the living cytoplasm would resemble those of a mitochondrion torn from its natural home and exiled in a completely abnormal environment?

During the past few years an admirable new fashion has begun to grow. Everywhere attempts are now being made to integrate the mass of knowledge that has been accumulated about the parts into a coherent picture of the whole. Thanks to the electron microscope, structure has come into its own again, but now it is closely correlated with function, and personally I think that the most exciting era in the study of the cell is now upon us—the era of molecular biology.

Tissue Culture

One subject that has experienced more fluctuation of fashion than most is tissue culture. When I was a research student at Edinburgh, tissue culture was very much the "coming thing," and in 1923 I hurried off to Cambridge to learn the technique from Strangeways. But ten years later its heyday was past, and by 1939 it had sunk to a low ebb in public esteem. The sheer beauty of the technique was partly its undoing. The very idea of growing cells outside the body was so romantic and exciting, and the living cells in culture were such lovely objects under the microscope, that far too much was expected, and the expectations were not fulfilled. In a sense, tissue culture was born before its time, because the optical and biochemical methods necessary for its proper exploitation were not yet available. But after World War II, the development of phase contrast microscopy and of microchemistry enormously enlarged the possibilities of tissue culture as a research method, and the technique underwent a striking renaissance. Even organ culture is now enjoying a modest vogue. Cell cultures have proved to be unsuitable for many types of in vitro experiments, because unorganized cells do not respond to many biologically active agents in the same way as differ-

entiated tissues. Consequently, many more laboratories are practicing the organ culture method now than were using it a few years ago.

Why We Follow Fashion

The motives that prompt us to follow fashions in research are various and not always estimable. Let us consider first some of the reasons why a new technique "catches on." The obvious and legitimate reason—that it is likely to provide an answer to many people's current problems—naturally accounts for much of its sudden popularity, but not for all. Some techniques become fashionable partly because they are difficult, expensive, or (better still) both, and this gives them a certain snob value; they are, as it were, the mink coats of research. Other techniques become fashionable for exactly the opposite reason—because they are so cheap and simple that anyone can use them, and yet they are *new*, and that in itself confers upon them a certain prestige.

When an important new discovery is made, it usually presents the broad outlines of a new picture, but the details have yet to be filled in. Very few of us are capable of drawing the broad outline, but if we are competent scientists we are perfectly capable of filling in the details, and this we rush to do, for a number of different reasons. Some do it out of passionate curiosity, because they long to see what the finished picture will be like; others do it because they are short of ideas, and the new work has disclosed some line of investigation that they are well qualified to follow. These are both very sound reasons. But less worthy motives sometimes operate. The field is new, and so prestige is to be gained from working in it; but what is even more important, being new, it is likely to attract money from granting bodies.

And this, I think, brings us to a rather pernicious aspect of fashion in research. In general, the waves of interest in something fresh that constantly sweep through our world are vital to its well-being, and without them research would indeed be stagnant and dreary. But rushing after new things merely because they are new (or what is more commonly termed "jumping on the band wagon") is another matter; it leads to the abandonment of existing lines of work that ought to be carried much farther, and even to contempt for the realities of nature, as in the disdain for structure that was such a regrettable fashion in cell biology a few years ago.

So where do we stand in this matter—is fashion in cell biology to be deplored or encouraged? Whichever way you decide to answer this question, the fact remains that hundreds of us have converged on Paris from all over the world for the sole purpose of watching what I am sure will be a magnificent display of all the latest modes—of course I mean in cell biology!

AAAS New York Meeting

New York meeting information; Annual Exposition of Science and Industry; concluding section and society programs.

Raymond L. Taylor

In more than 112 years, the Association has met no more than seven times in any one city, and, indeed, only five cities have been host that often (New York, Boston, Philadelphia, Washington, and Chicago). This year's 127th meeting will be the eighth New York meeting.

Every meeting in New York has been a large one, and it is possible that this Christmas week the record-breaking New York meeting of 1949 will be exceeded in number of registrants. That meeting was held in the four Pennsylvania-zone hotels. Fortunately, this year's meeting in the Grand Central zone has more capacity, both in session rooms and in sleeping accommodations at uniform, moderate rates. Indicators of heavy attendance have been advance registrations—about twice as many as last year—and hotel reservations, which have been heavy, with a larger number of wives than usual attending. The national meeting of the American Astronomical Society will be in a downtown hotel along with other participating societies, instead of uptown, as in 1956. An officer of the American Society of Zoologists has estimated that this will be perhaps their largest and most comprehensive meeting to date. Exclusive of several multisessioned symposia, that society's sessions for short papers were increased from 10 to 19.

The original 103 booths of the Annual Exposition of Science and Industry were sold out during the summer. Fortunately, the Biltmore Hotel was able to accelerate the remodeling of two session rooms on the same floor as the exposition, and thereby made space available for booths 104 to 119.

The preparations for any large scientific meeting—even if it is a recurrent yearly event, and one with a basic pattern—are difficult fully to appreciate except by those who have been involved. The annual national meeting of the American Association for the Advancement of Science is particularly complex, uniquely interdisciplinary, and variable with respect to the number and identity of the many participating societies. Typically, all 18 AAAS sections have programs, often symposia one to six sessions in length; some 40 to 50 of the 245 affiliated societies will meet with the Association and sponsor programs varying from single sessions or social events to full-scale national meetings with concurrent sessions extending over four or five days. Several affiliates regularly arrange regional meetings or sponsor special two- to five-session