

ragnar granit

discovery and understanding*

Grappling with the necessity of supplying something of general interest, I remembered the frame of mind in which I had spent the early spring 1941 after a bicycle accident that crushed one knee. Reading could not then fill all my time; besides, it compounded the constraint I felt, being confined to intake alone, while all the time the creative urge demanded release in some form of output. In this predicament I recalled an early lecture of mine to an academic student body under the heading *Talented youngster looking for a teacher*, and this put me to writing a collection of essays, *Ung mans väg till Minerva*⁽¹⁾ (Young man's way to Minerva) which was published that autumn.

My book preceded Cannon's *The Way of an Investigator*⁽²⁾ by a few years. When his work appeared I read it eagerly and found a great deal of overlap, both in its point of view and in its emphasis. Far more has since been written on the same subject—more systematic, better documented books covering the whole field (e.g. Beveridge⁽³⁾).

Thus, it was with feelings of anxiety that I looked up my old work. Rereading it now and musing over it, I found it, indeed, a book by a younger man than my present self, written for young men fired by enthusiasm for a life devoted to science. The tutor, slightly older than his listeners, speaks to them about the courting of Minerva: he tells them of her apparent fickleness and real austerity, of her views on ambition and success, and of much else, not forgetting to mention the radiance of her smile on the rare occasions when she bestows it. There was about these essays an air of intimacy nurtured by convalescence. I have been asked by a publisher to translate them into English but do not trust myself to render into another language something that depended so much on its style of presentation.

Now, thirty years later, I return to such matters in a mood of detachment. Many people regard detachment as one of the great virtues. But it is probably not conducive to scientific creativity of the kind that

* Also published in «Annual Review of Physiology», 34, 1972.

(1) R. Granit, *Ung mans väg till Minerva*, Stockholm, Norstadt, 1941.

(2) W.B. Cannon, *The Way of an Investigator*, New York, Norton, 1945.

(3) W.I.B. Beveridge, *The Art of Scientific Investigation*, London, Heinemann, 1961.

was life itself to the young author of *Minerva*. Passion is a better word for describing that attitude. Young people are out for themselves, to make discoveries, to see something that others have not seen. They may be satisfied with a modicum of analysis because there is always something round the corner to look at—perhaps something new and quite unexpected, exciting and important, at any rate a temptation hard to resist. Later in life one may feel it less compelling to discover something. Rather does one prefer to learn to understand a little of Nature's ways in a wider context. Then, detachment comes in handy. One realizes that it really is a virtue: the virtue of those who have to weigh and judge. In this state of mind, I have decided to offer some comments on discovery and understanding. In the main I shall restrict myself to experimental biology.

By « discovery » we mean in the first instance an experimental result that is new. In a more trivial sense most results are new just as they always impart « knowledge » of some sort. For practical purposes I tend to ask myself when reading a paper: is this knowledge, or real knowledge? Similarly one may ask: is this result new, or really new? In the latter case it is a discovery, and a discovery tends to break the carapace of dogma around an established view, just as a bombardment with heavy particles tends to scatter the nucleus of an atom. In this type of discovery there is an element of unexpectedness. One of the best known examples is Röntgen's discovery that came as a surprise to him and to the rest of the scientific world.

There is a second and equally fundamental type of discovery; the delivery of experimental evidence for a view that is probable, yet not established, because such evidence as there is has not yet excluded alternative possibilities. An example of the latter type is the theory of chemical transmission at synapses, suggested by T. R. Elliot in 1905, but not proved until very much later (Loe-

wi, Dale). This is the most common type of discovery: confirmation by evidence of one theory from a number of alternative hypotheses.

Either type of discovery, to deserve the term, must have far-reaching consequences, as the cases illustrated here indeed have had. Unless this criterion is satisfied, we are not willing to use a grand word like « discovery » instead of speaking modestly of a new result, more or less interesting, as the case may be.

The experimenter himself may not always understand what he has seen though realizing that it is something quite new and probably very important. Thus, for instance, when Frithiof Holmgren in 1865 put one electrode on the cornea and another on the cut end of the optic nerve, he recorded a response to anset and cessation of illumination. This he held to be Du Bois-Reymond's « negative variation », that is the action currents of the optic nerve fibres. These were what he had been looking for and therefore expected to find. Six years later, Holmgren started shifting his electrodes around the bulb and soon understood that the distribution of current he obtained required that the response had to originate in the retina itself. Dewar and M'Kendrick independently rediscovered the electroretinogram on the equally false supposition that a retina would display the photoelectric effect, at that time recently discovered by Willoughby Smith. In both cases the electroretinogram was the unexpected result of something expected. It was an important discovery, the first evidence for an electrochemical process generated by stimulation of a sense organ: evidence that something objective connected a physically defined stimulus to a sensory experience. Quite rightly Holmgren titled his first paper (in translation) *Method for objectivating the effect of an impression of light on the retina*. It also satisfied the criterion that a discovery should have far-reaching consequences. I was myself concerned with three of them:

the discovery of inhibition in the retina, the demonstration that an important component of light adaptation and dark adaptation was electrical in origin and not due to photopigments alone, and the development of the theory that generator potentials stimulate sensory nerves to discharge. Subsequent workers in this field could easily extend the list, if further proof of its importance should be required.

Quite interesting is the period of latency between the discovery of the electroretinogram and an elementary understanding of what it meant. In the present context the latency serves to emphasize that « discovery » and « understanding » really are different concepts and are not arbitrarily differentiated. There is in discovery a quality of uniqueness tied to a particular moment in time, while understanding goes on and on from level to level of penetration and insight and thus is a process that lasts for years, in many cases for the discoverer's lifetime.

The young scientist often seems to share with the layman the view that scientific progress can be looked upon as one long string of pearls made up of bright discoveries. This standpoint is reflected in the will of Alfred Nobel whose mind was that of an inventor loaded with good ideas for application. His great Awards in science presuppose definable discoveries. The following are his own formulations from his will: « The most important discovery or invention within the field of physics », « The most important discovery within the domain of physiology or medicine », « The most important chemical discovery or improvement ». Only in chemistry, of which he had first-hand experience as an inventor of smokeless powder and dynamite, did he allow that a Nobel prize could also be given for an « improvement ». It is well known that one of his major contributions to the invention of dynamite was in the nature of an improvement; he made the use of dynamite nearly foolproof by adding kiesel-

guhr to the original « blasting oil » (nitroglycerol) that had proved so dangerous in practice. This finding may have made him realize that there are inventions and discoveries which have to be improved before their significance can be established. One should thread warily through these subtle distinctions. I can think of Nobel Prizes in Chemistry that have been given for « improvements » but do not remember ever in 30 years of Academy voting having heard of any citation legitimized by this term.

It is easy to understand the emphasis, or rather overemphasis, on discovery as the real goal of scientific endeavor. By the definition used here, a discovery has important consequences and initiates a fresh line of development. It catches the eye and in the present age, is pushed into the limelight by various journals devoted to the popularization of science—sometime even by newspapers. In my youth we were much impressed by a philosopher at the University of Lund, Hans Larsson (who, I believe, wrote only in Swedish). I remember a thesis of his to the effect that in our thinking we try to reach points commanding a view. In science, discoveries often serve as viewing towers of this kind. The discoverer himself may not always climb to the top of his own tower. Others make haste to reach it, outpacing him. In the end many people are there, most of them trying to do much the same thing. The discoverer himself should be excused if he is possessed by a desire to find a peaceful retreat where he can do something else and quietly erect another lookout.

A systematic classification of types of discovery cannot be attempted here but some comments should be made. There are, for example, the discoveries that ride on the wave of a technical advance. At the time it became possible to stimulate nerves electrically, it became possible to discover any number of new and important mechanisms of nervous control. Small wonder that the great German physiologist Karl Ludwig

could say to his pupils; « wer nur arbeitet, findet immer etwas »⁽⁴⁾. Equally optimistic was Helmholtz when, as professor of physiology at Heidelberg, he said that it was merely necessary to take a deep dig with the spade in order to find something new and interesting. Transferring these amiable opinions of Ludwig and Helmholtz recorded by their pupil Frithiof Holmgren to the present age, one would, for example, expect every one of the large and busy brotherhood of neurophysiologists to turn out discoveries. But is this so? The question is rhetorical.

Today there is a much shorter period of skimming the cream off a new technique than there was in the 1860s. It is not uncommon to find that those workers who depend very largely on a specific technical innovation, soon become sterile even though they themselves may have had an honorable share in the development of the technique they are using.

Those who start with a problem and develop the technique for solving it can in the long run look forward to better prospects. As an example one might take Erlanger and Gasser's use of the newborn cathode ray to measure conduction velocities of the component fibers of nerve trunks. On the basis of W. Thompson's formula for electrical cable conduction, Göthlin had made calculations in 1907 leading to the theory that conduction velocity in thick nerve fibers would be greater than in thin ones. Some fifteen years later Erlanger and Gasser, realizing that amplification made it possible to use the inertia-less cathode ray for tackling this question, took the trouble to overcome the deficiencies with which the early cathode ray tubes were afflicted and, as we all know, solved the problem of conduction velocity in nerve fibers of different diameter.

This is an interesting example of a rather common type of discovery, the one in which it is realized at the outset that something definite can be discovered, provided that the required technical solution can be managed. It presupposes that the experimenter

knows how to formulate a well defined question and realizes what kind of obstacles prevented earlier workers from answering it. In the case of Erlanger and Gasser the basic result could hardly be called unexpected. Nevertheless most neurophysiologists are willing to classify their result as an important discovery, some perhaps merely because it had far-reaching consequences in physiological experimentation. I do so with a further motivation; many things can be predicted with a fair degree of probability, and in all good laboratories a number of such predictions, some passing fancies, others quite significant, are floating about. My respect and admiration goes to the people who reformulate such notions into experimental propositions and do the hard work required for testing them. These people are the real discoverers. The other day I saw in a student journal from the Royal Technical University of Stockholm my viewpoint expressed in a modern version: « It is easy enough to say Hallelujah, but go and do it! ».

The sterilizing effect of a technique stabilized into a routine was briefly alluded to above. What then happens is that those well-up in the routine easily turn into great producers of small things. Of course rejuvenation is possible. A good example is the technique of tissue culture which was for a long time in that particular state of aimless delivery but has since recovered its significance. In my own field of neurophysiology it seems that the technique of evoked mass potentials is balancing on a rather thin edge of functional relevance, all the time running the risk of becoming merely an accessory to anatomy. While this itself is a respectable science, physiology should have different aims in order to remain respectable in its own sphere. There should not be too many people within a field who care merely for the technically soluble and not for what is worth solving. However, this tempting subject will not be pursued now. Most workers, as they

(4) « He who but works will always find something ».

grow older, realize that some kind of borderline exists between those who are interested in a technique as an instrument for producing papers justifying grants, and those who see it as a possible way of furthering long-range projects.

« I have to admit », said Helmholtz in his *Vorträge und Reden*, « that those fields of study have steadily grown more pleasant for me in which one is not constrained to resort to happy coincidences and fancies » (my translation). With that basic attitude to a life in the field of science there is no alternative available than to try to realize some fundamental ideas about biological structures and their functions, that is, to promote understanding. Gradually understanding will ripen into insight. It cannot be denied that for some time « happy coincidences and fancies » may have a value that they otherwise would not possess, when fresh possibilities are opened up by a new technique. But will this inspiration last into one's old age? I daresay Helmholtz was right when he advocated working from a basis of understanding.

This attitude toward scientific work has the advantage of permitting the experimenters to devote themselves quietly to their labors without filling various journals with preliminary notes to obtain minor priorities. A disadvantage is of course the practical difficulty of persuading various foundations and research councils that their work is of some importance in a world such as ours is at present. The judgment required to appreciate the mode of progress I am advocating may not always be at hand. There is a well-known example in Fulton's biography of Harvey Cushing: After a visit to Sherrington in Liverpool in 1901 Cushing wrote in his diary: « As far as I can see, the reason why he is so much quoted is not that he has done especially big things but that his predecessors have done them all so poorly before ». Sherrington, as we all know, had a good long-range program, and Cushing was no fool. One can only conclu-

de that it can be very difficult to make others even understand the aims of long-range programs—much less support them.

There are so many instances of discoveries having led to major advances that one is compelled to ask whether it is at all possible to make a really important contribution to experimental biology without the support of a striking discovery. Sherrington's life and work throw light on this question. Most neurophysiologists would not hesitate to call him one of the leading pioneers in their field. Yet he never made any discovery. In a systematic and skillful way he made use of known reflex types to illustrate his ideas on synaptic action and spinal cord functions. Reciprocal innervation was known before Sherrington took it up, decerebrate rigidity had been described, many other reflexes were known, inhibition had been discovered, spinal shock was familiar—at least to the group around Goltz in Strasbourg, and the general problem of muscular reception had been formulated. What Sherrington did was to supply the necessary element of « understanding », not, of course, by sitting at his writing desk but by active experimentation around a set of gradually ripening ideas which he corrected and improved in that manner. This went on for years—a life time, to be precise. Ultimately a degree of conceptual accuracy was reached in his definition of synaptic excitation and inhibition that could serve as a basis for the development that has taken place in the last thirty years. His concepts are still with us, now fully incorporated in our present approach to these problems. The insight Sherrington ultimately reached can of course be called a « discovery », but to do so is contrary to usage. Within the experimental sciences the term « discovery » is not applied to theories acquired in this manner, even though the experimenter himself may feel that he has had his moments of insight coming like flashes of discovery after some time of experimentation.

Another example illustrating slow ripening

of fundamental insight is provided by Darwin's life and labours. Back in England after the long cruise in the *Beagle* he went to work. « My first note-book », he said, « was opened in July 1837. I worked on true Baconian principles, and without any theory collected facts on a wholesale scale, more especially with respect to domesticated productions, by printed enquiries, by conversation with skillful breeders and gardeners, and by extensive reading—I soon perceived that selection was the keystone of man's success in making useful races of animals and plants. But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me »⁽⁵⁾. Malthus' *Essay on Population*, a book still quite readable, gave him a « theory by which to work », because, he says, he was « well prepared to appreciate the struggle for existence » which would tend to preserve favorable variations, and tend to destroy unfavorable ones.

Darwin described flashes of insight in his work—as all scientists could do—but essentially it was twenty years of hard labor scrutinizing the evidence for his thoughts that in the end brought clarity. In 1858 he published a preliminary note together with Wallace who had independently arrived at similar conclusions; in 1859 appeared his *Origin of Species*. The idea of evolution was by no means new. His granddaughter Nora Barlow emphasizes that « to Charles Darwin it was the body of evidence supporting evolutionary theory that mattered, and that he knew was his own contribution ». With some justification one can say that today the long, narrow and winding road to real knowledge has become harder to follow. In the face of innumerable distractions it has become increasingly difficult for the individual worker to preserve his identity. This, however, is necessary if he intends to grow and ripen within any branch of science. The point I want to make is that what we read, what we actively remember and what we ourselves contribute to our fields of

interests very gradually build up living and creative structures within us. We do not know how the brain does it; no more than we know how the world of sight gradually becomes upright again when for a while we have carried inverting spectacles. Our knowledge of the workings of our mind is of the scantiest. We simply have to admit that the brain is designed that way.

By « keeping track of one's identity » I mean cultivating the talents of listening to the workings of one's own mind, separating minor diversions from main lines of thought, and gratefully accepting what the secret process of automatic creation delivers. I can well understand that many people do not think much of this notion and prefer to regard it as one of my personal idiosyncracies. Others who late in life look over their own activities, are sure to find at least something that looks like a main line of personal identity in the choice of their labors. Up to this point many colleagues are perhaps willing to agree. But a little more than that is meant when I maintain that an active brain is self-fertile in the manner described. I am convinced that if one can take care of one's identity, it, in turn, will take care of one's scientific development.

I am emphasizing all this so strongly because there are today so many distractions preventing scientists from enjoying the quietude and balance required for contact with their own creative life. The cities and the universities are becoming more restless, and the « organization men » with their meddlesome paper work of questionnaires and regulations tend to increase in number while the number of teachers relative to students decreases. This development tends to breed a clientele of anti-scientific undergraduates demanding more and more of the universities and less and less of themselves. The research workers withdraw into separate research institutes—furthering the deterioration of standards in teaching and in intellectual idealism in the faculties of our

(5) N. Barlow (Editor), *The Autobiography of Charles Darwin*, London, Collins, 1958.

ancient sites of learning. Science does indeed need a number of pure research institutes, but university faculties left to themselves and engaged wholly in teaching can hardly be called universities; these should be capable of living up to the true « idea of a university » in the sense that it was once defined by Cardinal Newman in his well known book.

In all creative work there is need for a good deal of time for exercising the talent of listening to oneself, often more profitable than listening to others or, at any rate, an important supplement to the life of symposia and congresses. Perhaps this latter kind of life is also overdone in the present age. There are so many of these meetings nowadays that people can keep on drifting round the world and soon be pumped dry of what is easier to empty than to refill.

My plea for a measure of « self-contact » is really that of the poet and essayist Abraham Cowley (1618-1667) who said that the prime minister has not as much to attend to in the way of public affairs as a wise man has in his solitude. If there are those who experience nothing when trying to listen to themselves, this need not always indicate congenital defects. They may have been badly trained or may have been too lazy to absorb the knowledge and experiences that the brain needs for doing its part of the job.

Against this background one can raise the question of whether all creative originality in science is necessarily inborn or whether there also exists an acquired variety of this valuable property. I suppose that most people share my view that great originality in creative work is part of a man's inheritance. But after half a century of scientific activity I have had the opportunity to observe the development of many contemporary as well as younger colleagues. I feel that a perusal of these experiences—without mentioning any names—might suggest an answer to this question or at least provide an opinion. It seems then that some

of those who as young men did not show much promise of originality, although quite capable of the necessary intellectual effort, later have given original contributions to our science. How should this observation be interpreted? Obviously I may have been mistaken. On the other hand, one does not often make mistakes about real originality—quite apart from the fact that real originality often insists upon being recognized. I do not believe that the category of people of whom I am now speaking were wrongly assessed at an earlier date. Rather it is my conviction that these are the very people who without difficulties have managed to explore their own mental resources so as to make profitable use of them. They have had the capacity for listening quietly to their own minds and to the good advice of others, and in this way have grown, blossomed, and born fruit.

These conclusions will appear more evident if one considers progress within any individual branch of science. It is well known that in each phase of development the same ideas turn up in many laboratories of the scientifically active world. It is hardly necessary to add examples, as two good ones have already been provided: Holmgren with Dewar and M'Kendrick and Darwin with Wallace. Even Newton himself said that he had stood on the shoulders of a giant. At the time when I regularly followed the scrutiny of proposals for Nobel prizes (requiring careful study of priorities), there were many opportunities to observe independent but overlapping discoveries as the bases of proposals from different sources. This is by no means surprising. Why should not well trained people who have read much the same lot of papers and monographs come to similar conclusions about the next step in a logical sequence? Since it is often difficult to foresee what each step implies for subsequent steps, parallel conclusions may in a number of instances lead to quite original contributions based on knowledge and perseverance.

In the last instance the front line of research is created by minds whose combined effort more or less perfectly represents the inner logic characteristic of a particular period. Many professional scientists have good intuitive contact with the broad lines of this development. This is expressed in the saying that something reflects the « characteristic note of the age ». The original part of what is called « originality » is a capacity for understanding, intuitively as well as logically, what is an important step forward within any specific branch of science. A creative scientist has more numerous, better developed and more precise contacts with the characteristic note of his age, and can therefore, if health and perseverance do not fail him, make greater contributions than others.

I think I have said enough in defense of my thesis that acquired originality exists. Such acquisition requires intense work—preferably within one particular sphere of problems—and, obviously, enough talent to support a reasonable rate of intake. I have always believed that in most cases people have enough talent for handling research, at least when working with a team, and that failures should be accounted for by other factors which I need not enumerate in this connection.

All this implies that, in the present era of rapid communication by many channels, the individual scientist has but a share in the process of scientific discovery and understanding: even if he abandons his field of research, its development will continue, though perhaps in a slightly different way or at a slower pace.

From this standpoint it is profitable to contemplate the disturbing and often pathological quarrels concerned with the ownership of ideas. Ideas, notions, and suggestions are often thrown out in passing at meetings or in laboratory discussions and may sometimes fall on fertile ground. Who then is the owner? The one who made the suggestion may or may not have intended to make

anything out of it. Again I maintain that the only definable ownership belongs to the man who develops the idea experimentally, or propounds it as a definite and well-formulated hypothesis capable of being tested. Fights about priorities are never as violent as when a discovery is at stake. This is well known, and, if I mention such matters briefly, it is merely to point out the dangers of too much emphasis on the need for making a discovery, and to contrast it against the more peaceful life of development of understanding—without looking askance at « discovery » and what it may bring in its trail in the way of specific rewards.

I began by comparing the efforts of young men with those of men old enough not to be called young and by trying to show with the aid of two famous examples that it is by no means necessary to make any discoveries at all to do extremely well in science. It is not my intention to undervalue discoveries, but only to emphasize that it is really understanding that scientists are after, even when they are making discoveries. These are or can be of little interest as long as they are mere facts. They have to be understood, at least in a general way, and such understanding implies placing them into a structural whole where they illuminate a relevant step forward or solidify known ideas within it.

Since understanding or insight is the real goal of our labors, why make so much noise about discoveries? Why indeed? Perhaps because they provide instantaneous excitement—releasing the « eureka », whose echo we hear reflected across the centuries, and because they offer the immediate rewards found in the appreciation of colleagues, laymen, and donors. The alternative, the slow development of a world of conceptual understanding in the manner of a Darwin or a Sherrington is of course far more difficult to follow. If it is worth a great deal to have some good ideas when one wants to make a discovery, then it is an

absolute necessity to have them if one intends to take that long road whose ultimate goal is to reveal fundamental principles guiding the development of knowledge in any field.

This second variant of scientific endeavour does not always suit the impatient passion of the young, ruled by an ambition which craves immediate satisfaction, but a little later in life it provides feelings of assurance and satisfaction in one's work. The pleasure of living to see a synthesis mature after years of labor helps the worker to maintain a more generous attitude toward the results

of others and also to mention more freely the names of colleagues whose findings have contributed to the understanding ultimately achieved. Work becomes less competitive and the atmosphere of a laboratory friendlier. Such an attitude is particularly valuable in research institutes where people have to defend themselves by delivering results and have no chance of escaping into teaching or administration. The long-range program protects the individual worker and fosters insight of the kind that makes disputes about « intellectual ownership » meaningless.

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