THE CONFLICT OF SCIENCE AND SOCIETY
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BY

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FOREWORD

BY SIR RICHARD GREGORY, BAR., F.R.S.

SINCE the first Conway Memorial Lecture was delivered thirty-eight years ago several distinguished biologists have been included among the lecturers in the series, and each has presented, in his own field and fashion, humanistic values of scientific thought and discovery.

Dr. Darlington, this year’s Conway Lecturer, is the Director of the John Innes Horticultural Institution, Surrey, the first Director of which, in 1910, was Dr. William Bateson, who gave the name “genetics” to that part of biological science which is concerned with the study of heredity and variation. This subject can be studied from many points of view, one of which is that of cytology, or the nature and structures of living cells. It was in recognition of his original work in this field, and of his co-ordination of a diversified body of apparently disconnected facts, that Dr. Darlington was awarded one of the Royal medals of the Royal Society two years ago.

What atoms mean in physics and chemistry is now widely understood, as well as the meaning of atomic fission and its application in
the making of atom bombs. It is also well known that the basis of life in all forms is a viscid substance called protoplasm. Blobs of this clear substance are the atoms of living matter. Somewhere about the middle of each such material spot is a part called the nucleus, out of which threads and rods called chromosomes continually emerge and grow. These chromosomes represent the material basis of heredity. They are aggregations of active little units called genes, each of which plays a decisive part in transmitting particular characteristics to the complete organism into which it enters.

Dr. Darlington has recorded his own observations of chromosome structures and behaviour in many papers contributed to scientific societies, and has interpreted, in several notable volumes, the results obtained by explorations of the fields of cytology and genetics in the two generations during which these branches of science have established themselves. A tangled skein of phenomena has thus been wove into a pattern of fundamental principles.

In the present lecture he deals with the reactions of academic and other social groups to knowledge of any kind which requires readjustments of old looms to produce acceptable patterns with the warp and woof of newly discovered threads. He is as outspoken and
fearless in his denunciation of such obstacles to progressive thought and work at any time or anywhere, as he has always been to the suppressive attitude often presented to interpretations of new observations in his own biological field. He stands for freedom of scientific thought and expression without control by any authority, as is represented in the spirit of the motto of the Royal Society, *Nullius in verba*.

Science, in the sense in which the word is now commonly understood, means organized and formulated knowledge of natural objects and phenomena derived from verifiable observations and experiments. In medieval times it constituted natural philosophy, which was one of the three philosophies of university learning, the two others being moral philosophy and metaphysics. All these subjects are concerned with the pursuit of truth about the nature of man and his place in the universe.

The history of civilization from this point of view is a history of intellectual development in which science has been the chief factor in changing habits of thought from superficial observation and speculative and anthropomorphic theories of causation to clear concepts, rational conclusions, and progressive principles in the advancement of man and society.

Social biology is concerned with the struc-
tures and activities of communities: human biology with those of individuals, each of whom is made up of many millions of corpuscles of protoplasm, commonly called cells, though none of these is bounded by a visible wall and the outlines of all are continually changing. Certain elements of social structures are transmitted from one generation to another, while others vary in time and place as stages of an evolutionary process.

The idea of evolution of forms of life through the action of natural forces goes back to early Greek philosophers; but how and why the processes work was not understood until Darwin found the master-key to the problem in the facts of heredity and variation upon which his principle of natural selection was founded. The rule of the road of heredity was established by Mendel’s experiments on the hybridization of peas and other plants in a monastery garden at Brunn, in Austria, but its meaning was not interpreted until systematic researches into cell structures began about the beginning of the present century. Dr. Darlington is a leading authority in this language of life; and in his lecture on “The Conflict of Science and Society” he uses it as one instance among many of enlightening influences impeded by intellectual and institutional obscurantism.
THE CONFLICT OF SCIENCE AND SOCIETY

I

In speaking of the Conflict between Science and Society, I mean the conflict between Discovery, which I take to be the active principle of science, and Continuity, which is in some measure the necessary condition of society.

What are in general terms the bases of science's relations with society? Society allows or encourages certain of her members to devote their lives to the discovery and organization of new knowledge. This new knowledge, in so far as it is new, undermines the habits and beliefs that have been handed down by society, habits and beliefs that are religious, moral, and social. At the same time the new knowledge undermines the material basis of society, rendering old methods of production and the people, individuals, classes, or even races, using these methods less important, and new methods and new people more important.

Scientific discovery thus transforms (and at an increasing speed) the conditions at once of human happiness and of human survival. If the expectation of these effects was enough to arouse the suspicions of the Roman Church
600 years ago (not to mention earlier incidents), how much more must the organs of tradition and continuity resent their fulfilment to-day? Yet in fact we find a variety of opinions, or no opinion at all, held on this vital question.

There are many no doubt who would maintain that no such conflict exists beyond what is temporary, trivial, accidental, or personal. Such people often assert that science is, or in some sense can be, free from the control of society. Others, and especially scientists, would probably agree that the conflict was temporary but not that it was trivial. They would argue that science is good for man; that all good men therefore want to develop and apply science for the use of their fellows as effectively and speedily as possible; that certain men resist this process to serve their selfish interests; that such interests must be destroyed and then a more blessed, or better planned, state of things will obtain.

I want now to consider the evidence of what actually happens.

II

Scientific discovery is often carelessly looked upon as the creation of some new knowledge which can be added to the great body of old knowledge. This is true of the strictly trivial discovery. It is not true of the fundamental
discoveries, such as those of the laws of mechanics, of chemical combination, and of evolution, on which scientific advance ultimately depends. These always entail the destruction or disintegration of old knowledge before the new can be created. And it is this destruction, or the fear of it, which arouses the opposition of the well-trained and well-established scientist, as well as of those outside science whose beliefs the new ideas threaten to disintegrate.

The discoverer himself is the first person to fear and to doubt his discovery. His misgivings may prevent him publishing the awful fact for fifteen or twenty years, as Newton or Darwin did. Or they may prevent him admitting that it has the revolutionary consequences that are obvious to others, so that he remains, like Priestley, the last supporter of the theory which his discovery has rendered unnecessary. Or, again, it may lead him to publish his observations in the most obscure place, half-fearing that someone may notice them, as Mendel did when he buried his experiments in the proceedings of a provincial natural history society.

All this timidity and secretiveness is characteristic, not of trivial observations, but of the greatest discoveries. And it is not characteristic of the individuals concerned so much as of the society in which they live. It is a social reaction, and if the discoverer himself over-
comes the beliefs in which he has been brought up, he will find his friends and his teachers willing to provide all the doubts and misgivings that he himself has failed to mobilize. He will find that in the embattled ranks of discovery, to-day as of old,

... those behind cried "Forward"
And those before cried "Back."

A practical example will illustrate the obstacle that mere good training sets up to scientific discovery.* On May 8, 1795, the French astronomer Lalande recorded a new star. On May 10 he saw it again, but it had shifted its position. What did he do? He crossed out the first observation and marked the second as unreliable. Later it was found that he had in fact seen a new planet, Neptune. Now Lalande was an expert on planets. He knew so well that there was no planet out there that he could not discover it. And it was not in fact discovered until it had been predicted from the movements of other planets fifty years later. Even then, the first prediction by Adams in England was not enough. For Adams was unable to persuade either his respectable professor at Cambridge or the busy and important Astronomer Royal to undertake a wild-goose chase, involving a whole month's work, merely

for the sake of verifying the prediction of a
doubtless very irritating young man.
The very training which is the social activity
necessary for discovery serves to impede it,
and it does so in various ways. Curricula and
examinations stabilize the truth and they
stabilize it of necessity far below the level of
the great discoverer. Newton being ploughed
in Euclid, and Keynes failing to satisfy his
examiners in economics, are examples of the
recurring difficulty of finding examiners who
understand the implications, the possibilities of
discovery, latent in their own subjects.
The opposite side of the picture is shown by
the number of great discoveries that have been
made by what we may call interlopers, by
men with little or no training in the field they
have invaded. It is no accident that bacteria
were first seen under the microscope by a draper,
that stratigraphy was first understood by a
canal engineer, that oxygen was first isolated
by a Unitarian minister, that the theory of in-
fection was first established by a chemist, the
theory of heredity by a monastic school teacher,
and the theory of evolution by a man who was
unfitted to be a university instructor in either
botany or zoology. Here again you will see I am
speaking only of the greatest discoveries. The
highest academic training provides no obstacle to
purely technical applications and developments.
Training does harm to great discoverers, because with rare exceptions it is inadequate to their needs. The reasons for this inadequacy are inherent in scientific discovery itself. The very language necessary for communicating the results of one discovery becomes by mechanical repetition an obstacle to the next one. Most people probably imagine that science advances like a steam roller, cracking its problems one by one with even and inexorable force. That simple view, which is taken or implied by most advocates of planned research, is true only of the lower grades of activity. Rather, science advances as though by the pulling out of a drawer which gives on one side only to jam on the other. There is then nothing to be gained except by pulling on the other side. If you pull on the same side, the jamming gets even worse. Three-quarters of all kinds of research work consists in pulling on the wrong side with this very result. And the reason is simple: the same man cannot usually pull on both sides of the drawer. Another man is needed, a competitor, an upstart, an impudent fellow who probably despises the first man and who, profiting by his work, will not hesitate quickly to take the whole credit for the result of the little pull he has had the luck to give at the end.

Thus everyone knows that electric light was
not invented through attempting to improve candles, but everyone, or nearly everyone, acts as though it had been.

Each man's goal has been fixed by his own talents and his own training. He wants to make a discovery but it has to be of the kind that he is fitted to make; it must usually be of a medium size and of a conventional shape. To make it he wishes to live and work in a department of his own dimensions, protected from the large and less familiar world outside. So protected, he resolves upon the track of his advance and nothing will divert him. Indeed he will do everything he can by argument, bluff, threat, or blackmail, as the circumstances demand, to divert anyone else whom he sees to be taking the wrong track. What he conceives to be the right track is the product of Society, of its institutions, its apparatus, its stock of ideas.

Let us now suppose that the discoverer has overcome his internal difficulties. Let us suppose that he dares to offer it for publication. If his discovery is novel enough, it will often lie outside the scope of special journals and editors who refuse to publish it will have a plausible excuse for doing so. All the more so if the publication of the new involves the criticism of the old and the bitter controversy that such criticism always entails. The inno-
vator will then be told (as I have been told by the editor of a scientific weekly) that such sentiments as his "befoul the life blood of science," a life blood, we are bound to notice, that is adjusted to the needs of standing still rather than of moving on.

At this point, you might imagine, great societies and academies set up for the advancement of scientific knowledge would step in. They have been founded for the express purpose of overcoming all obstacles to discovery. Surely they will be quick to seize on any opportunity that will bring them honour and profit. The most illustrious academies have however protected themselves well against this emergency. Before publishing a paper they submit it to a referee who is chosen to be the most eminent authority (apart from the author) in the field in question. This is the man whose reputation will suffer most from a revolutionary discovery. Is it surprising therefore that, while the most trite and trivial communications meet with no objection, the referee's opinion of the most important discoveries is nearly always adverse if not fatal? Waterston and Joule actually had their papers rejected by referees of the Royal Society and Fourier by the French Academy. What comment the French Academy would have passed on the *Origin of Species* it is unnecessary to ask, for in
1864 its Perpetual Secretary, Fourier's successor, volunteered a concise descriptive report which might epitomize for all time the opinion held of each discoverer by the established generation of his predecessors: it was "empty, pretentious, puerile, superannuated."*

When finally a discovery has been published, contradiction and contempt are of course the first and second lines of defence against an author who is so inconsiderate as to insist on its importance. Silence and concealment, however, provide further protection. When the Master of Trinity College hid the Origin of Species from his pupils he was merely doing what always has been done, and always will be done, by those who are appointed to protect society against the incursions of science. When, on the other hand, an author is, as I pointed out, too timid to press his claims he may be disregarded without any trouble. It does not matter then whether the paper is buried in an obscure journal like Mendel's or has appeared in a most prominent position like Avogadro's law† of the molecular density of gases, or

* It is worth noting that the National Academy of Sciences in Washington is still immature in this respect. It has not yet effectively armed itself against discovery and continues to this day to take the awful risk of publishing, without censorship and without delay, the papers submitted by its members.

† Always known as a hypothesis because of its author's unassertive modesty.
Lancaster’s aerodynamics.* Oblivion sheds her poppy carelessly alike over all until a more restless generation has taken possession.

The principle that is now, I hope, emerging is the very general one that any social organ, whether an individual or an institution, which is effective in improving knowledge or in changing society acquires thereby an authority which it uses to stop the very process of change that it has started. Just as Aristotle or Newton, Linnaeus or even Darwin, who had all been forces for discovery during their lifetimes, thereafter became the greatest obstacles to the advance of science, so academies, universities, and societies founded to promote discovery become a means of suppressing it. They begin as the instruments of science. They become the instruments of society.

III

We may now look at these reactions in a wider field. The sciences of botany and zoology entered the nineteenth century each paddling its own canoe. To be sure, an eccentric thinker had noticed that chickens and chickweed equally demanded the newly discovered oxygen, and greatly daring had proposed the name

Biology to cover them both. But for ordinary people each was concerned with the simple task of describing its plants or its animals, comparing their parts, and giving names to their species.

In the middle of the nineteenth century this programme of description was vastly encouraged by the national organization of such collections as those of the Royal Botanic Gardens at Kew and the British Museum of Natural History. The study of botany in the universities was also vastly stimulated, not, however, as providing a scientific basis for growing plants, not for the sake of agriculture at home or in our colonies, but in a more traditional form. In the English universities botany had grown up as a study of the names of plants designed to help the apothecary in seeking out for himself in field and forest the rude remedies for our ancestors' ailments. It was a part of the medical curriculum in the fifteenth century and as such it expanded in the nineteenth and has remained to this day in our universities, a monument to the predominance of our conservative society over any reforming tendency of scientific discovery, a monument to the purity of pure science.

By the middle of the nineteenth century, therefore, a system of teaching and research in botany and zoology had been stabilized in this
country with a well defined simple programme and divided into departments with regular endowments. Vested interests had been created. Civil Service classifications had been laid down.* The directions of development had been determined. This academic organism, scarcely weaned, had to meet two rude shocks. Let us see how it withstood them.

The first shock came when, with the publication of the *Origin of Species*, the hurricane of Darwinism swept across the world of science. The whole work of research into living things suddenly gained a new dimension from Darwin’s theory of evolution. An immense field of experimental research was opened up by the new understanding of the relations of heredity, variation, and reproduction. The disconnected studies of plants and animals, of microbes and men, became one science and the word “biology” began to be used with profound implications for human life and thought.

Did this revolution affect the teaching of the universities? Did the departments of botany and zoology, of medicine and agriculture, put their heads together to see how they could exploit their new opportunities? Did Darwinian studies begin to appear in their curricula?

* Thus the Loveday Report offered National Agricultural Advisory Service Officers £50 per annum extra for having suffered under the discipline of *pure* science, *i.e.*, Geology without Soil, Biology without Heredity, and Mathematics without Statistics.
Were new departments of biology established in new universities? No, nothing of the kind. Everything went on, so far as possible, just as before. The departments remained sovereign and undisturbed, as they remain to this day. But surely, you will say, a Darwinian Institute, or a Chair of Evolution, was set up somewhere? Abroad, yes; but in England, no; nothing of the kind. Not even a question in a University examination and not even a statue in a quiet London square. Darwin was buried in Westminster Abbey and Darwinism was buried in our universities. It was not pure science.

The second shock came from a purely technical development. In 1886 a new optical discovery, the apochromatic objective lens, was introduced by Messrs. Zeiss of Jena. Microscopes were made whose effective magnification was raised from the hundreds to the thousands. In the study of life a whole new world was brought into view.

The development of the apochromatic lens was rather more important for the study of life than was the discovery of America for the study of the earth. Both rounded off a picture which was previously incomplete. But the new lens did even more. It opened the way not only for new facts but for new ideas, indeed for the greatest of all syntheses, the union of the physical and the biological sciences. The
connection between the chemical world of molecules and the living world of cells suddenly became visible. The foundations of the three basic properties of life—heredity, development, and infection—were laid bare by the new microscopes.

But, alas! to enter into this new world demanded not only new microscopes and new dyes; it demanded new ways of handling plants and animals, new experimental operations and mathematical treatments. It demanded, in addition to all these, a new way of thinking about life and living organisms.

These were steps that British botanists and zoologists safely ensconced in their new museums, herbaria, laboratories, and departments were not prepared to take. Dissection, classification, enumeration, repetition flourished like the green bay tree. Innovation, analysis, and experiment, in one place after another, withered and died. These things are not pure science.

There was, however (I will not conceal the fact), a good deal of bustling about. In 1875 it was becoming obvious to people acquainted with European science that British botany was falling behind the rest of the world. Its slackening was especially due to the lack of such fine optical apparatus as was already being developed in Germany. Everybody was de-
lighted therefore when a certain Mr. Jodrell promised to build and equip a room at a cost of £1,500 for the use of unpaid visiting workers at Kew. It was hoped that this new venture would put British botany on its feet again. It might even provide the scientific foundations for exploiting the plant wealth of a large and increasing empire. Darwin wrote insisting that German examples should be followed and that the best equipment should be bought. It was bought: hundreds of pounds must have been spent. And the stuff was so good that it has been said* that "it was not found necessary to add many items to this equipment until after 1930": two generations had passed by. No one had noticed the invention of the apochromatic lens.

Thus to-day, over seventy years later, we find that the same work continues at the Royal Botanic Gardens at Kew: it continues without the assistance of the new methods that could give new life to its almost dead inquiries. Indeed it is not enough that this unimproving task should proceed at Kew alone. Five miles away another collection is maintained by its own staff at the British Museum of Natural History, likewise divorced from the new techniques of experiment and microscopy. Each