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JENNER AND HIS IMPACT ON MEDICAL SCIENCE*

BY

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We are assembled here to-day to celebrate the life and work of Edward Jenner, who was born in Gloucester 200 years ago on May 17, 1749. We come to pay tribute to a man who discovered that vaccination protected against smallpox, and thereby placed in the hands of any community which cared to make proper use of it the means of eliminating from their midst a deadly and disfiguring disease. The method was effective as Jenner presented it, and all the new knowledge and technical skill that have been added in the last 150 years have not changed its fundamental basis or added much to its efficiency.

Apart from the value of vaccination in controlling or even eliminating smallpox, this discovery of Jenner's has much wider significance. It was one of the earliest instances of preventive medicine and of the public control of disease. Its value on the scientific side of medicine is just as imposing, for Jennerian vaccination can be regarded as the parent of modern work on viruses and virus diseases and even of modern immunology. Indeed, it would be difficult to mention any discovery that has had a greater impact both on public health and on medical science, and it is but right that we should pay tribute to the man who was responsible for this important advance in knowledge.

So much emphasis is nowadays placed on technique and statistical approval of investigations that it is well for us to take this opportunity of reminding ourselves that the first object of research is discovery, and that such discoveries may come not only from systematic and prolonged investigation, involving the use of standard methods with elaborate apparatus, but equally well from methods of observation and experiment of the simplest kind. Jenner himself was no professional research worker but a man with exceptional powers of observation and perception, who had also the outstanding quality, remarkable for this period and apparently innately developed, of appreciating the value of the experimental method.

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There is a vast amount of information about Jenner's life. Indeed, it would be difficult to find anybody of that period whose actions, experiences, and thoughts were better documented, and it would have been easier to centre this lecture round his history. I do not propose to do this, however, because I am anxious to paint a broader picture of his work and its influence on medical science. It would, however, be wrong to eliminate all such references, because, clearly, a proper appraisal of his work must depend in part on the state of knowledge at that time and on the conditions under which he was brought up and laboured. There has been a tendency to write about and discuss Jenner as if he had the knowledge, outlook, and facilities of later days. I wish therefore to refer briefly to some of his experiences, especially during the formative years of his life, in order to convey an idea of his training, his equipment, and the circumstances in which he made his observations.

A Brief History

Edward Jenner was the son of a country parson living in Gloucestershire, and at the age of 13 was apprenticed to a doctor with the intention of becoming an apothecary. Under Ludlow, a surgeon at Sodbury, near Bristol, he studied pharmacy and surgery. In 1769, at the age of 20, he had the good fortune to become an apprentice to John Hunter, then a surgeon at St. George's Hospital and the owner of a menagerie at Brompton, where he made his world-famous studies on the structure and habits of animals. Hunter was then 41. Hunter and Jenner dated their intimate friendship from this time and began a correspondence which only ended with Hunter's death in 1793. Hunter expected much from his assistants, but he was equally good in giving them help and opportunities for advancement. Thus, in 1771, when Captain Cook returned from his voyage to the great southern continent with a large cargo of natural history specimens, mostly collected at Botany Bay, it fell to Jenner's lot, through the influence of Hunter and of Sir Joseph Banks, then President of the Royal Society, to prepare and arrange these specimens. This he did with such skill that he was offered the appointment of naturalist to Captain Cook's next expedition, which sailed in 1772.

Other evidence of Jenner's capability is seen in the fact that Hunter also suggested that he might become his partner and give additional lectures on comparative anatomy and surgery. Both these invitations were rejected, however, and Jenner preferred to return to his native village to become a country doctor in the vales of Berkeley and Gloucester.

Throughout his life one of Jenner's main interests was natural history, and it was in 1787 that he sent his manuscript on the behaviour of young cuckoos through Hunter to the Royal Society. In this publication, it will be remembered, he showed that fledgling cuckoos heaved out of the unnatural nest in which they were born other fledglings and eggs, a murderous instinct they lost by the twelfth day after hatching. It was not only Fellows of the Royal Society who found difficulty in believing this observation, and its complete acceptance had indeed to await the arrival of the cinematograph. However, a year later, in 1788, the work was published by the Royal Society in its *Philosophical Transactions*, and Jenner was elected to the Fellowship of the Society.

While a medical student at Sodbury, Jenner heard the local traditional rumour that milkmaids who had suffered from cowpox never took smallpox. There is much evidence that he was not only intensely interested in this statement but that he brooded on it and studied the matter closely during the next twenty years. In 1796 he made the first vaccination—that of a boy—with lymph made from the vesicle on the hand of a milkmaid infected with cowpox. In 1797, when he was 47, he sent to the Royal Society for publication a record of his observations on the natural history of cowpox, and this was rejected. It is said that the refusal to publish this work was accompanied by an admonition that "as he had gained some reputation by his former papers to the Royal Society, it was not advisable to publish this one, which would injure his established reputation." The paper, revised and extended, was published by Jenner in 1798 as a private pamphlet with the title *An Inquiry into the Causes and Effects of the Variolae Vaccinae, a Disease discovered in some of the western counties of England, particularly Gloucestershire, and known by the name of the Cow Pox.*

Jenner lived in Gloucestershire as a busy country practitioner all his life, except for a short period in 1802, when he was tempted by the importunity and promises of his friends to become a specialist in London. This was a failure, financial and otherwise, and caused a return to his own countryside after three months. It must be added that Jenner did not retain the status of an apothecary, but in 1792 took the M.D. degree of St. Andrews University and did some consulting work in Cheltenham. From the time he published his paper on smallpox in 1798 Jenner, although living a secluded life in the country, was a notorious public figure, much applauded and much criticized. He spent most of the rest of his life vaccinating people and promoting vaccination as a preventive of smallpox. He died at the age of 74.

His Association with Hunter

This, then, is a brief outline of his career. It would be wrong, however, to pass without further discussion Jenner's interest in natural history and especially his relationship to Hunter. There might appear at first sight to be nothing exceptional about Jenner's interest in nature at that time, for, as is well known, there was a tremendous outburst of activity in this field dating from the middle of the eighteenth century and probably stimulated largely by the systematization of animals and plants by Linnaeus. The literature of that period is full of works, often of a very high standard, on natural history. Most of this, however, was of an observational type, and investigations were largely directed to the classification and description of the natural life of the country, including the birds, butterflies, and flowers. But Jenner's interest in natural history was due neither to the fashion of the times nor to Hunter, for at the age of 9 he had made a collection of dormouse nests and one of fossils from the oölite.

In the long correspondence with Hunter, of which only Hunter's letters are extant, it is possible to see not only the close personal relation between these two men but also how they reacted on one another and constantly stimulated each other to further action. Most of Hunter's letters are full of requests for animals and birds of one sort or another. He asks Jenner to send him young blackbirds of different ages, crows' and magpies' nests, an old cuckoo and a nest with a cuckoo's egg in it, a live heron or bittern (" see how they make the noise ! "), a porpoise, white hares (a buck and a doe) from Jenner's friends in Newfoundland, bats from the old castle at Berkeley, and fossils, more hedgehogs —" a colony of them "—and even a bustard. Besides asking for these things, he was constantly telling Jenner to do things, to send him " a true and particular account of the cuckoo and, as far as possible, under your own eye," to take temperatures of hibernating animals ; and, above all, as is well known, it was in this correspondence that he told Jenner not to think but to try the experiment.

Hunter's letter of June 7, 1773 or 1774

" I thank you for your experiment on the hedgehog ; but why do you ask me a question by the way of solving it ? I think your solution is just ; but why think ? Why not try the experiment ? Repeat all the experiments upon a hedgehog as soon as you receive this, and they will give you the solution . . . and let me know the result of the whole."

There were, however, some more human touches about these letters of Hunter's : for instance, when Jenner announced that he had had a severe disappointment in marriage Hunter wrote to him : " I own I was glad when I heard that you was married to a woman of fortune but let her go; never mind. I shall employ you with hedgehogs, for I do not know how far I may trust mine."

On another occasion Jenner asked Hunter to be godfather to his child. Hunter replied, accepting the office as follows: "I wish you joy; it never rains but it pours. Sooner than the brat should not be a Christian I will stand Godfather for I should be unhappy if the poor little thing should go to the Devil because I would not stand Godfather. I hope Mrs. Jenner is well and that you begin to look grave now you are a father." On the whole, however, these letters are very much to the point and concerned almost entirely with either ordering or acknowledging the receipt of natural history specimens or criticizing Jenner's experiments.

One of Jenner's great characteristics was that he seemed to make use of every opportunity that was offered to him. For instance when he met Hunter in Bath on one occasion he saw at once that Hunter was suffering from angina pectoris, which was the ultimate cause of his death. The ordinary man would have been greatly perturbed at seeing this condition in a friend but would have done nothing about it. Jenner, however, did not take this line, but wrote to Heberden, who was in medical charge of Hunter, about his diagnosis, and, moreover, began to take an interest in the condition of the heart which was associated with this disease.

The result was that he made the first observation on the thickening and calcification of the coronary arteries in this disease, and, although out of a natural reticence he did not pass on this information to Hunter, he told Home, Hunter's brother-in-law, of the fact. After the post-mortem examination on Hunter, Home wrote to Jenner in the following words: "It is singular that the circumstance you mentioned to me and were always afraid to touch upon with Mr. Hunter should have been a particular part of his complaint, as the coronary arteries of the heart were considerably ossified." When we remember the long period of time that elapsed before it was generally accepted that the pathological basis of angina pectoris was coronary disease, it is remarkable that this observation should have been made by Jenner at that time.

Although the records of Jenner's observations on natural history which he wrote in his notebook between 1787 and 1806 are meagre, from them it can be seen at once that he was no mere collector or casual observer of nature but a man who observed accurately and persistently and had the faculty of picking out the essential from the dross.

His post-mortem records of the egg-forming organs of birds and of the abnormalities to be observed in dogs which had died of distemper show these qualities. Nor did he hesitate to put his ideas to the test, as, for instance, when he exchanged eggs and fledglings from one nest to another or when he marked birds before migration and observed the results. Where Jenner made simple observations or experiments his recorded results can be regarded as correct. When he had preconceived ideas on subjects about which there was little or no knowledge, he made mistakes in deduction, as, for instance, when he decided that tubercles in the lungs were derived from hydatids.

The general impression to be derived from these notes is that Jenner had the mental outlook and qualities of a genuine discoverer, and that it is no mere chance that he did in fact make discoveries.

Discovery of Vaccination

Probably one of the simplest ways of focusing attention on Jenner's great discovery of vaccination against smallpox, and especially upon its basis, would be to recall the criticism that has often been brought against him for having given the name variolae vaccinae to cowpox. Even the fact that he put this into Latin has caused criticism. The further charge was that, by giving such a name, he insinuated into the minds of medical men that cowpox was smallpox. He was accused of having introduced " an unblushing invention of a misleading name," that he was " wanting in the rudiments of common candour," that this was an action of sheer trickery, and that the profession were thereby mystified and hoodwinked about the true nature of cowpox. Had it turned out that the facts upon which Jenner had given this name were incorrect, these critical scholars would certainly have had some cause for rejoicing, but, as we all now know, Jenner was right on almost every practical and scientific point.

Let us first see what he himself thought on this matter when he introduced the words variolae vaccinae. He wrote: " There are certainly more forms than one (without considering the common variation between the confluent and distinct) in which the smallpox appears in what is called the natural way. It will be inquired (if the foregoing reason be *a priori* correct) in what way can the action of cowpox (or the equine pock) in preventing subsequent smallpox be reconciled with the established laws of the animal economy ? My reply is, for the reasons which I have stated on the basis of fact, that they were not *bona fide* dissimilar in their nature but, on the contrary, identical. On this ground I gave my first book the title of ' An

Inquiry into Causes and Effects of the Variolae Vaccinae,' a circumstance which has since been regarded by many as the happy foresight of a connexion which was destined by future evidence to become warranted."

Let us now see what modern scientific evidence has proved and what posterity has had to say about this claim of Jenner's, which in reality was the essential basis of his discovery of vaccination for smallpox. In 1902 Copeman inoculated a monkey with smallpox virus, vaccinated a calf from the monkey, and produced typical cowpox. Later Blaxall found that both alastrim (variola minor) and variola major viruses from human subjects produced a papulo-vesicular lesion on the skin of a monkey but not on the skins of calves or rabbits. Either of these two variola viruses protected the monkey against vaccinia and, conversely, vaccinia protected it against both of them. He then succeeded in increasing the virulence of both forms of variola virus for the calf, so that in the space of three successive passages the calf developed the typical lesions of cowpox.

Thus it was demonstrated that the biological difference which specimens of alastrim and variola viruses exhibited when removed from man disappeared when they were passed through the calf. As a control virus Blaxall used material from varicella (chicken-pox), which he found, as previous observers had done, to be without effect on the skin of the monkey or other animals. Similarly, by employing the specific test of allergy introduced by von Pirquet, he showed that the viruses of vaccinia, variola, and alastrim behaved alike but were sharply differentiated from the virus of varicella.

Later Gordon showed that vaccinia protected the monkey against other strains of mild and severe types of smallpox better than these strains of variola protected the animal against vaccinia. He further showed that the variola virus from five outbreaks, including three of the mild and two of the severe types of smallpox, reacted positively with antivaccinia serum in the complement-fixation and agglutination tests.

The Father of Modern Virus Studies

In a lecture of this kind it is possible to refer to only a trivial amount of evidence on this question, but Jenner would indeed derive much fun and satisfaction from the mass of investigation on vaccinia and variola viruses if he could visit us to-day. He would see the unassailable evidence of the truth of his observations on man in 1798 that vaccinia protected against variola. He would find that we now know more about the morphology and the biological properties of vaccinia virus than of any other virus. He would greatly appreciate modern methods of altering the type of lesions produced by vaccinia virus and of selecting different strains of virus which, although antigenically and immunologically indistinguishable, can cause under appropriate conditions cutaneous lesions, meningo-encephalitis, orchitis, pneumonia, or keratitis—i.e., degrees of difference in biological action of one virus greater than those of the smallpox and cowpox virus with which he was familiar.

He would be greatly interested to learn that, in spite of all these different biological properties that can be induced in vaccinia virus and in spite of the remarkable cross-immunization properties and the closely related antigenic and immunological actions of variola and vaccinia, it is widely believed that variola and vaccinia viruses are different entities, and that the evidence that one is ever completely transformed to the other is insecure.

At this stage of my reading about pox viruses I also began to feel insecure, and decided to call in an expert bacteriologist for the latest stop-press views about the interrelation. He provided me with the following statement : " It has been generally assumed, since the days of Jenner, that vaccinia is variola modified by passage through the calf. For this there is good evidence, supported by modern experimental work. The further assumption, however, that vaccinia is the same disease as naturally occurring cowpox virus is more doubtful, since Downie has shown that vaccinia and cowpox viruses differ in their heat-labile antigens and give rise to different lesions in animals. Both viruses can, of course, protect against variola. Thus vaccinia virus is almost certainly a derivation of human smallpox virus, but cowpox virus is a naturally occurring and rather different virus belonging to a much larger group of animal pox viruses."

Jenner would be amazed to learn of the present enormous field of knowledge of other virus diseases and of the information that has been accumulated about the properties of these viruses. He might just be a little uneasy about some of the new knowledge. For instance, he might think that, from the scientific angle, it was fortunate his particular problem concerned smallpox and cowpox and not the influenza viruses A and B, which, while producing similar morbid effects in man, do not protect against each other. He would realize that he probably would not have made much headway in preventing distemper in dogs—a disease which did interest him intensely—on learning that when the virus of this disease is transmitted to ferrets the infected material from the ferret will not protect against distemper in the dog nearly so well as the dog's virus itself, in spite of the fact that the infective agent is the same in each case.

However, Jenner in fact did choose smallpox as his objective, and, in spite of the complexity of virus problems which now face the scientific world, some due to their innate properties and some to the various species of animals and the different tissues in which their biological reactions have been studied, he would have the supreme satisfaction of realizing that he started all this work, that his views on the relationship of smallpox and vaccinia have been generally confirmed, and that indeed he can well be regarded as the father of modern virus studies and of the biological and pathological reactions they produce.

Official Approval

So far as the subsequent history of vaccination is concerned, I shall simply recall that Jenner's triumph was great when the National Vaccine Board, consisting of the President and four Censors of the Royal College of Physicians and the Master and two Governors of the Royal College of Surgeons, was set up in 1808 by the Government. The immediate stimulus to this action was the report of the Royal College of Physicians, published in 1807, which concluded with the following words:

"The College of Physicians feel it their duty to strongly recommend the practice of vaccination. They have been led to this conclusion by no preconceived opinion, but by the most unbiased judgment, formed from an irresistible weight of evidence which has been laid before them. For when the number, the respectability, the disinterestedness and the extensive experience of its advocates are compared with the feeble and imperfect testimonies of its few opposers; and when it is considered that many, who were once adverse to vaccination have been convinced by further trials, and are now to be ranked among its warmest supporters, the truth seems to be established as firmly as the nature of such a question admits; so that the College of Physicians conceive that the public may reasonably look forward with some degree of hope to the time when all opposition shall cease, and the general concurrence of mankind shall at length be able to put an end to the ravages at least, if not to the existence, of the smallpox."

In the light of this edict it is strange that variolation—namely, the inoculation of smallpox material as a prophylactic against the disease—which had been practised in the East from time immemorial and introduced into England largely owing to the influence of Lady Mary Wortley Montagu about 1722, continued to be practised in England until it was made a penal offence by the Vaccination Act of 1840. The long continuation of inoculation for smallpox for a period of over 30 years after the official acceptance of vaccination is an example of official inertia, especially in view of the truth and force of Jenner's arguments that it was a more dangerous procedure to the individual than vaccination, and that it allowed the continuation of smallpox itself throughout the community. However, in most ways vaccination had a rapid and world-wide acceptance.

Influence of Jenner's Discoveries

When we turn to the wider question of the influence of Jenner's discoveries on epidemiological and scientific advance of knowledge we find a different and more depressing picture which, because of its ultimate great triumph, makes us realize more fully how long before its time, from a scientific angle, Jenner's discovery was made. For 80 years, apart from smallpox itself, Jenner's work was without influence in the wider field of protection against infective and infectious disease. In 1877, however, the scene was changed, for it was in that year that Pasteur, whose reputation was already very high because of his work on fermentation and infection of plant life, turned his attention to human and animal disease.

There is no doubt that when he began to investigate anthrax and other diseases he studied closely the literature of immunity to smallpox following variolation and Jenner vaccination, and that it was Jenner's work which primarily caused him to think that a similar state of affairs might hold for other diseases; the first malady he studied from this angle was chicken cholera. It will be remembered that in 1881 he gave an address in London at the International Medical Congress on vaccination in relation to chicken cholera and splenic fever. It was on this occasion that he explained his adoption of the words "vaccine" and "vaccination" to denote the process of prophylactic inoculation in general and expressed his indebtedness to Jenner's work in the following words:

"I cannot complete this address, however, without testifying the great pleasure I feel that it is as a member of an international medical congress meeting in England that I finally communicate to you the vaccination of a disease probably more terrible for domestic animals than smallpox for man. I have given to the term vaccination an extension which science, I hope, will consecrate as a homage to the merit of and to the immense services rendered by one of the greatest of Englishmen, your Jenner. I am indeed happy to be able to praise this immortal name in the noble and hospitable city of London."

It may not be without interest to mention that the president of this congress, the late Sir James Paget, in thanking Pasteur for his address, pointed out that what Jenner had done for the good of the human race Pasteur had done for the good of animals, but, whereas Jenner had had to fight his battle for the benefit of men's lives against a vehement opposition, Pasteur had met with no such opposition in his work for the benefit of cattle. We still meet with this kind of relative reaction—an indication that human nature has not changed very much in the last 150 years.

Pasteur follows Jenner

In developing a treatment for fowl cholera Pasteur followed Jenner in first producing enfeeblement of the virus, which he called attenuation. As in the case of smallpox he noticed that if fowls recovered from the effects of inoculation of the virus the disease was not likely to recur, and that if relapses did occur they were in inverse ratio to the severity of the first attack. If the virus was transplanted from medium to medium at intervals varying from days to a month or two, no change was observed in the virulence for fowls. If, however, the interval was prolonged to three, four, or five months the cultures became less and less virulent and the fowls, even if they fell ill, recovered and if they were now injected with a virulent cholera culture they survived the injection. By this means he discovered prophylaxis of fowl cholera by attenuated virus, a principle which he also established for anthrax, swine erysipelas, and rabies.

In the case of anthrax he noticed that domestic animals occasionally recovered from the disease, which suggested that they developed natural immunity. He observed that the anthrax bacillus did not grow at 45° C. but grew well at 42–43° C. At the latter temperature, however, the culture became asporogenous and died out altogether in a month. When a virulent anthrax culture was kept at a temperature of 42–43° C. for eight days it was found to have lost much of its potency and was innocuous when injected into guinea-pigs, rabbits, and sheep.

Pasteur then proceeded to give a public demonstration of this work on 24 sheep, 6 cows, and 1 goat. On May 5, 1881, these animals were each inoculated with a living attenuated culture of anthrax bacilli. On May 17 the animals were reinoculated with a less-attenuated culture. On May 31 all 31 animals received a highly virulent anthrax culture, which was also inoculated into 24 sheep, 1 goat, and 4 cows not previously inoculated and serving as controls. On June 2 all the vaccinated animals were well, 21 of the control sheep and the goat were dead from anthrax, 2 of the control sheep dying in the presence of the spectators, which included a correspondent of *The Times*. The result of this test created an enormous sensation, and from this time immunology may be regarded as having been established. Pasteur himself said of this work that it was " un progrès sensible sur le vaccin Jennerien."

In this final phase of his work Pasteur, although a cripple, following a cerebral haemorrhage, but mentally as alert as ever, turned his attention to rabies. Making use of the fact discovered by Galtier in 1881 that rabies was transmissible to rabbits, he first showed with Chamberland and Rous that the virus of rabies entered the central nervous system. His next key observation was that spontaneous recovery very occasionally happened in dogs, and in five such dogs he found that subsequent intracerebral inoculations were without effect. Here again he had a disease with an immunity factor and with a virus transmissible to animals and therefore with a basic similarity to smallpox and anthrax. Clearly the principle of attenuation of the virus was presented.

As is well known, he proceeded on the one hand to exalt the activity of the virus by passage through rabbits, and on the other hand to attenuate its activity by suspending infected spinal cords in dry, sterile, and still air. After inoculation of emulsions of the attenuated cord he found it possible to inject emulsions of less-attenuated cord, and finally emulsions of the most powerfully active virus, with impunity. In 1885 he extended these observations to human beings, when he treated a boy aged 9 who was brought to him suffering from extensive bites inflicted by a dog with rabies. The boy was first injected with attenuated rabbit spinal cord which had been kept for 14 days. In a further series of 12 injections he received virus that was stronger and stronger until he was injected with the most virulent spinal cord, which had been taken from the rabbit after one day only. This boy remained well. That was the beginning of the modern method of treating this disease.

I have given this brief summary of Pasteur's work on the production of immunity on the basis of attenuation of virus because it seems to me that Pasteur's public recognition of Jenner's influence on his great work should be widely recognized. The same basic principles underlying Pasteur's remarkable discoveries are obviously present in Jenner's work on vaccination and smallpox. In all the diseases studied by Pasteur, as in Jenner's discovery, there was evidence of the invasion of the animal body by a virus (using the word in its older sense), and of the animal's power to build up a natural resistance to the disease, and the problem in each case was to find or produce a virus in an attenuated form which on injection promoted immunity to the fully virulent agent. The subsequent development of the whole subject of immunology since Pasteur's day has of course been enormous, not only in what are now known as virus diseases but also in diseases due to other types of pathogenic micro-organisms. Should we therefore be far wrong if we extended the suggestion previously made that

Jenner was the father of modern work on virus disease to the proposition that he might also well be regarded as the father of the whole domain of immunology ?

Unjust Criticism

So far I have refrained from discussing the personal qualities of Jenner, except to say that his mental characteristics were clearly those of a potential discoverer ; I have been content to deal with his experiences, his work and its results, and allowed them to speak for themselves. It is impossible, however, to read the extensive literature about Jenner without finding that he has been subject to more than his share of criticism and even of defamation, in regard to both his work and his character. This criticism has come not only from ignorant people with strong views about vaccination but also from some who would be regarded as scholars of medicine. In other words, into this literature scholasticism has entered.

We have been fortunate in medical science in having had only a minimum of the kind of writing which was such a prominent feature in theological studies of the Middle Ages. One of Jenner's critics who has been described by Greenwood as " an exact scholar and a highly educated man " and by W. Bulloch as " a scholar and philosopher—the most learned man I ever knew," in the course of what has been described as an exposure of Jenner wrote about him as follows : " They would probably have found Jenner to be the vain, imaginative, loose-thinking person that he certainly was by nature, and they might have so acted as to prevent him from becoming the impostor and shuffler that the course of events made him."

If this accusation stood alone it might well be disregarded, but I find that my friend Greenwood fully accepts the statement as regards Jenner being " vain, imaginative, and loose-thinking," though he suggests that the words " impostor and shuffler " in the second part of the sentence might be replaced by the description " fact-blinded enthusiast." In my view these judgments are not only imbalanced but wrong, and could have been made only by those who have little knowledge of and make but little allowance for human nature. We all know instances of writers of this type, and I want to take this opportunity of saying that it will be a sad day for medical science if such writing becomes a prominent feature, for there is but little room in scientific work for this sort of scholasticism. " Killing Kruger with your mouth " is at any time a poor game and in scientific work has no place. This was well recognized by the Royal Society when they adopted the motto " Nullius in Verba."

If a man thinks that the facts described by an investigator are wrong, then it is no good simply writing about them. The critic must go into the hospital or laboratory and make better observations or better experiments in order to prove his contention. The test of the acceptability of a discovery depends upon whether it is true and not whether it seems sensible or even whether it can be verified by the statistician. Most of Jenner's deductions from his work proved to be true. A few of his observations, such as, for instance, that a condition called " grease," a disease of the heels of horses, is the initial source of cowpox, have, I believe, proved to be untrue (although he was right in believing that there is a pox disease of horses), and occasionally his deductions, especially those concerning the infallibility of the protecting influence of vaccination against smallpox, were exaggerated, but this kind of defect will be found in the works of most scientific investigators, and the more fundamental their discoveries in medicine and biology the more likely are they to make mistakes at some time or other.

Speaking on the basis of my own experience in medical research, it seems to me that most facts published by scientific men are true but that they are apt to make two types of error, especially in the early accounts of their work. The first is that they sometimes forget that their results are true only for the conditions under which they are working, and the second is that they are apt to make unwarranted deductions regarding the implications of their results. In the latter case these often prove to be wrong. As regards the first source of error, it is clearly the object of the critic, if he once finds that the first man's results no longer hold, to search out the conditions which may have modified the earlier results. This is the normal method of procedure in research, and nearly always leads to new knowledge.

If, on the other hand, a man is to be condemned and denigrated because the implications of some discovery prove to be unsound, then I can only say that very few scientific men, even of the finest type, will have a shred of reputation left when they have been handsomelv dealt with by our scholars. No part of a man's work in biological and medical science is more difficult than that of foreseeing the implications of a new fact which opens up a new branch of knowledge, for, unlike research in the physical sciences, it is often impossible to realize the complexity of the conditions or to prejudge the relative importance of the factors concerned.

When the critic goes further and extends his criticism from the man's experimental work to his character and to his motives the position becomes intolerable, and it is this feature of the criticisms of Jenner which induced me to make this protest. I have read a good deal about the life and the work of Jenner in the last few days, and I find that the judgments of the above-mentioned scholars on this man are just incredible. My views about Jenner as an investigator can be seen from what I have said, and I think I should add that my judgment about his character is that he was a fine type of man. The whole of his life's history seems to me to point to this : his desire to avoid the scurry and publicity of life, his kindness to his family and relations, the time and trouble he took over his patients, his readiness to participate in the local life of his village community, his interest and participation in the arts, including poetry and music, his social relations with his fellow doctors, as judged by the local medical societies he formed—all indicate a man who both appreciated the best things in life and wished to help to the utmost his fellow-man. Independently of any of his discoveries and their results, he could be regarded as the best type of country doctor.

Outstanding and Successful Pioneer

It is, however, the man in relation to his discoveries whom we celebrate to-day, and I have tried to picture one who was bound to make discoveries wherever he was placed and who deliberately chose the circumstances where in fact he made the greatest of discoveries. This not only gave direct control of one of the most devastating of diseases but it also formed the basis of all modern work on immunology and of the fruitful field of virus disease.

Apart from Jenner's distinction, may I add how proper it seems to me that the Royal College of Surgeons and the Royal College of Physicians should participate in this celebration to-day, even if it serves only as a counterblast to the constant reiteration of public men in Parliament, in the Press, and on the radio that scientific research has as its main object the discovery of weapons and machinery for the destruction of man. All public attention seems to be given nowadays to this point of view, and it is useful to take the opportunity, such as is afforded to-day, of reminding the world that there is at least one branch of science which is wholly directed to the good of man, to the cure and elimination of disease and the prevention of untimely death. Up to the present, even if war is taken as a criterion, medical science in its work for the protection of man against disease and for his defence against injury has nothing to fear from comparison with that prostitution of the physical sciences which has been concerned with the production of methods of destruction.

In this beneficial work we proudly proclaim Jenner as an outstanding and successful pioneer.

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