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THE HARVEIAN ORATION

1866.

DELIVERED JUNE 26,

BY

GEORGE E. PAGET,

M. D. CANTAB.,

LATE FELLOW OF GONVILLE AND CAIUS COLLEGE, CAMBRIDGE.



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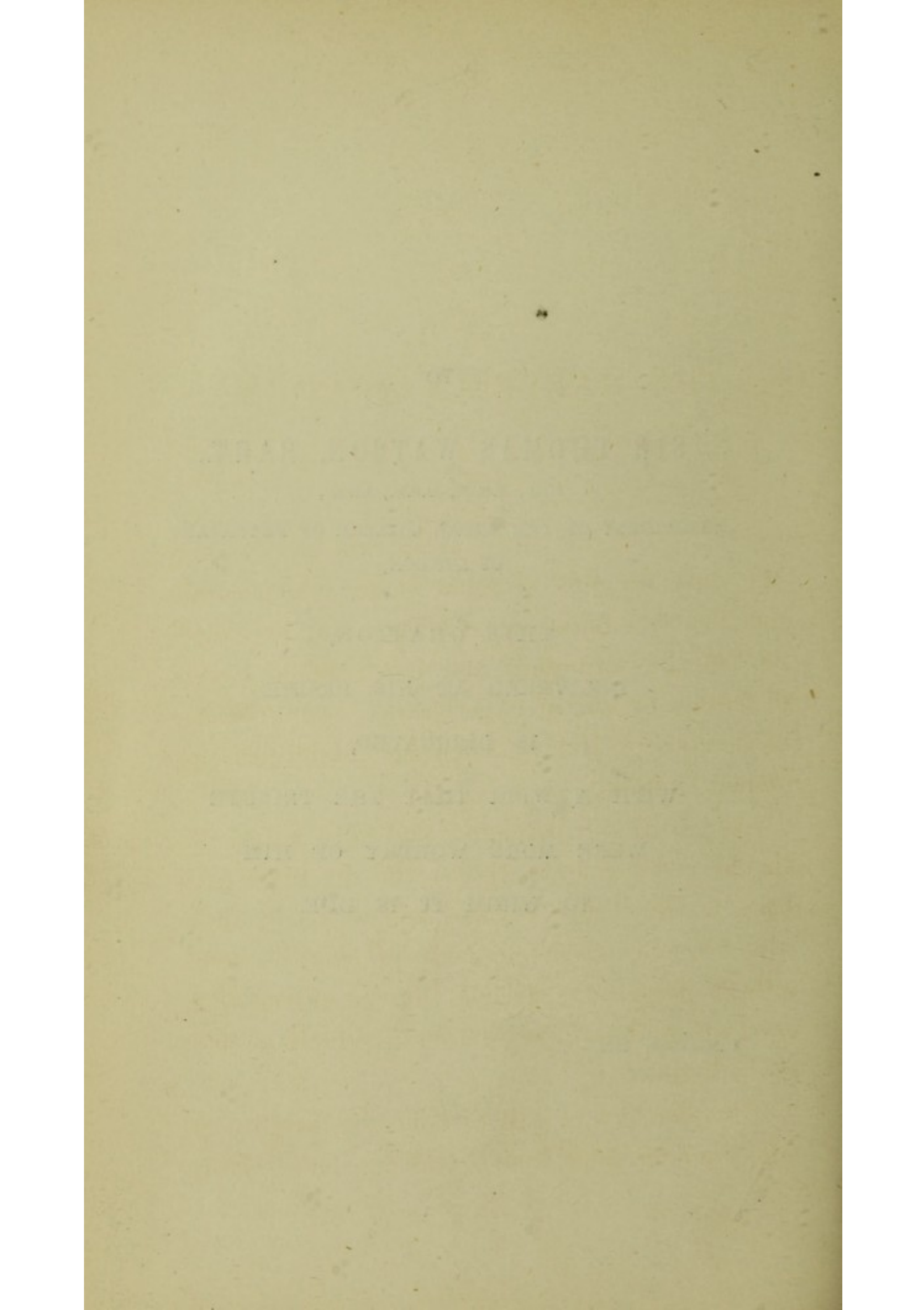
1866

Cambridge:
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TO
SIR THOMAS WATSON, BART.,
M.D., F.R.S., D.C.L., LL.D.,
PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS
OF LONDON,

THIS ORATION,
DELIVERED AT HIS DESIRE,
IS DEDICATED,
WITH A WISH THAT THE TRIBUTE
WERE MORE WORTHY OF HIM
TO WHOM IT IS DUE.

CAMBRIDGE, 1866.



THE HARVEIAN ORATION.

1866.

My duty on this occasion is to commemorate the Benefactors of our College, and to exhort its "members to study and search out the secrets of Nature by way of experiment." Such were the injunctions of Harvey when he established this annual Oration.

And truly it is becoming, that once a year we should commemorate those who in times past have benefited our College, and especially those who for well-considered purposes have bestowed gifts in perpetuity, striving thus to extend their good will and usefulness beyond the limit of their own life-time.

Therefore let us call to remembrance Linacre, our Founder and first President; and Gilbert,

Fellow and President of our College, him to whom the sciences of Magnetism and Electricity owe their origin,—one of those few clear-sighted men who were already practising what Bacon came to teach; and Harvey and Baldwin Hamley, and the founders of the lectureships, Lord Lumley and Dr Caldwell, Lady Sadlier and Dr Goulston; and if I deem it sufficient merely to mention their names, it is because I believe that the College does duly honour them in the best of all ways—by the fulfilment of their wishes; because no posthumous praise can be so suitable a tribute to the memory of Linacre as maintaining worthily, as you, Sir, do maintain, the honour and reputation of his College. We of his College rejoice, Mr President—all men rejoice—that the royal approbation has now set its lasting mark of honour on one, to whom we have so long looked up as among the very worthiest of Linacre's successors.

And now to my other duty, “to exhort the members of the College to study and search out the secrets of Nature by way of experiment.”

I can think of only one way in which it would be becoming in *me* to exhort them. I can point

to some of the paths that, being tried in former times, have led to Nature's secrets, and can speak of the men who trod them. Their deeds are eloquent, however feeble my words may be. And it seems not unsuitable to associate such men with the Benefactors of our College. We may justly reckon among our Benefactors all those who have extended the boundaries of medical knowledge; for, in so doing, they have enlarged our powers of helping our fellow-men, and have enabled us to fulfil more nearly the end of our Collegiate existence.

But there is one inherent difficulty under which a speaker labours in any attempt to interest such an audience as that which I have now the honour of addressing. It is, that he can tell them nothing new—nothing which they have not long known, and known perhaps far better than he has. His best efforts can achieve no more than reviving in their minds some of their own past thoughts and feelings. Yet this attempt, humble though it be, may not be altogether unsuitable in this great metropolis, where, amid the hurry and pressure of every-day life, the morrow is ever seeming so much more important than yesterday.

When one looks into the history of Medicine, one cannot fail to be struck with the diversity of ways in which it has made its progress. It has been advanced as a pure inductive science, the aim of which is the establishment of Laws of Life—of life in its normal state and as disturbed by disease. Thus have been formed the sciences of Physiology and Pathology, which constitute, to a certain extent, the basis of practical Medicine. To a certain extent its basis—but we know that they fall far short of being its sole basis. There is much in the treatment of disease which is neither founded on those sciences, nor deducible from them. Practical Medicine has had modes of progress of its own. At times its advancement has been accomplished laboriously by the results of a large experience methodised into principles. At other times it has owed notable progress to the happy observation of a single fact, and its immediate application to practice. In both these ways it has advanced without the aid of Physiology. In fact, as the study of Medicine is composite, so has been its progress, now in one way and now in another, under the guidance of various minds.

In illustrating these different modes of advancing medical knowledge, we shall see, in each case, the wisdom of the injunction, which points out the "way of experiment."

Of the inductive and physiological study we have not far to seek for an illustration, for we have a very model in the investigations by which our own Harvey discovered and demonstrated the Circulation of the Blood. Let us consider what were the paths he trod in searching out this great secret of Nature, what guides he had, and how he was equipped for the enterprise.

The discovery has sometimes been spoken of as if it had been a simple inference from that of the valves in the veins made by Fabricius; as if it were only one of those

"truths of Science waiting to be caught—

Catch me who can, and make the catcher crown'd."

Even if it were so, the honour would be no small one. To seize the unknown truth in the known fact is the very essence of scientific discovery. But the inference supposed could not have been simple or easy. Consider the length of time that intervened between the two discoveries. Fabricius discovered the valves of the veins in

1574(*a*),—four years before Harvey was born, and fifty-four years before the publication of Harvey's great work, *De motu cordis et sanguinis*; yet no one during that long period anticipated the discovery of the circulation, and so little were anatomists prepared for it, that its truth was but slowly admitted, and among its opponents were such men as Gaspard Hoffman, Gassendi and Riolan(*b*).

To us of the present day, to whom the circulation of the blood is a familiar fact, the action of the valves in the veins may appear very suggestive, or sufficient, evidence; but assuredly it was not sufficient for Fabricius and the other great anatomists of that time. Indeed we must try to put ourselves in their place, if we would form a just estimate of the intellectual merits of Harvey's discovery or even of the steps by which he reached it. We must remember how utterly erroneous were the notions then prevalent as to the functions of the heart, arteries and veins. We must remember that the heart had not been recognised as a propeller of the blood; that its substance was not known to be muscular; that its movements and those of its auricles and ventricles had not

been correctly observed; that its function and that of the arteries were associated with hypothetical vital spirits, the existence of which no one doubted; and that the nourishment of the body was supposed to be derived only from the blood in the veins, moved alternately in a flux and reflux by the respiratory acts.

Harvey had to disabuse first his own, and then other men's minds of these errors, and replace them by sounder views. Accordingly, the subject of his great work is the movements of the heart as well as of the blood, and the proof of the circulation is made up of a series of propositions, successively demonstrated by plain and clear reasoning from the evidence of observation and experiment. Harvey's inference that the blood must pass from the arteries into the veins is a triumph of pure reasoning, for the visible evidence of this fact was not given till many years afterwards by the microscopes of Malpighi and Leeuwenhoek and the injections of Blancard and Ruysch.

The facts which formed the basis of Harvey's reasoning were mostly his own, and were drawn from all available sources. Nature was in his esti-

mation the best interpreter of her own secrets(*c*). He saw the necessity of considering the motions of the heart and arteries not only in man, but in all animals that have hearts(*d*). He recognised the general fact, that to ascertain the function of an organ it must be examined not in one only but in various species of animals. This was one clue to his success. He was the first English Comparative Anatomist.

Another aid in his research was his knowledge as a practical physician(*e*) and pathologist. It is clear that he had a large acquaintance with Morbid Anatomy, and a just appreciation of what might be learned from it(*f*), though at that time it had scarcely been recognised by others as even a subject for study. Had he carried out his intention of publishing his "Medical Anatomy"(*g*), he would have been the founder of that branch of science ; for the *Sepulchretum* of Bonetus did not appear till twenty years, nor the great work of Morgagni until a century, after Harvey's death.

In short, Harvey had mastered all the medical knowledge of his time, and had gone beyond it. He was an untiring student of nature, ever observing, and reflecting on what he observed, and

the knowledge he had thus acquired served as lights to his path in his search after the great secret.

The path he chose was the way he has enjoined to us—the way of experiment. This was at that time no small merit. The influence of ancient authority—of Aristotle and Galen—was then not extinct: it was needful to assert, as Harvey did, that “the facts cognisable by the senses wait upon no opinions.” Moreover, the advantages of experiment, as distinguished from mere passive observation, had not been generally recognised even in physics. The success to which it conducted Harvey was almost the first proof of its value in physiology.

We cannot too much admire the largeness of grasp with which he brought all the science of his day to bear on the one point; the clearness of his mechanical conceptions; the soundness of his reasoning; the long patience with which he laboured for twelve years in perfecting his demonstration. It still remains, at the end of more than two centuries, his most fitting monument—a model for physiological investigation unsurpassed, if not unequalled.

The gain to physiology from such a discovery was great. The right conception of a function so important not only corrected innumerable errors, but laid anew the very foundations of the science of life, establishing a fresh and firmer basis for all subsequent investigations. To form a just estimate of its consequences we should have to sum a long series of steps in the progress of that science. Its indirect influence also was considerable. It did for physiology what had been done by Vesalins and others for the easier subject of anatomy. It shook the dogmas of authority, and showed what brilliant results might be looked for from well-devised experiments and logical reasoning from facts. Moreover, it showed the importance of considering the mechanical arrangements of organs, and so encouraged the idea of explaining the functions of the body on mechanical principles. And out of this came some truth and some error. The influence of the non-medical over the medical sciences has not been in all cases an influence for good.

And, now, if we turn from the influence of Harvey's discovery on physiology, to enquire what it did for practical medicine, our first feeling must

be one of disappointment. Its immediate influence seems to have been strangely small in proportion to the magnitude of the discovery. It would be interesting if we could tell to what extent it influenced Harvey's own practice. It seems to have had very little on that of his followers. 'Tis curious to notice how Willis in his Tract on Fevers published in 1659 recognised, that the discovery of the circulation had established a new foundation for medicine, and discredited the Galenical doctrine of the constitution of the blood; and then to notice how he clears away the ancient rubbish of the four humours only to replace it with another hypothesis, and takes this for the basis of all his reasonings on fevers, for his explanations of their phenomena and his indications for their treatment. He shows unconsciously how much need there was in those times of Harvey's exhortation "to search out the secrets of Nature *by way of experiment.*"

Why the immediate practical fruits of Harvey's discovery were so small, seems to me to have been for this reason: that practice at that time was far in advance of theory.

Physicians had already acquired by observation

and experience much real information respecting the symptoms and course of diseases, and the use of remedies, though their notions as to the movements of the blood and all other physiology were almost wholly erroneous. They had observed and utilized phenomena, though they were ignorant, or in error, as to their causes. Centuries before Harvey made known the real cause of the pulse, its states and varieties had been studied with the most minute attention, and made to serve as indications of treatment; from the time of Paré the arrest of arterial hæmorrhage by ligature of the vessel had been practised as it is now.

Thus practical rules, immediately deducible from the scientific facts established by Harvey, had been anticipated by observers who were ignorant of the causes of the phenomena which they took for their guidance. So it happens not unfrequently: practical experience runs ahead of scientific knowledge.

The connexion of gout with the calculous diathesis was known long before Wollaston discovered uric acid in the concretions of gouty joints.

For two centuries physicians have been curing

ague with cinchona bark, but how far are we still from understanding in any scientific sense the specific action by which the cure is effected.

This practical utilization of facts before their real nature has been scientifically determined is not peculiar to medicine. It may be observed in all the arts which nearly concern men's daily welfare and interests. The exigencies of society, the wants or sufferings of some men, the cupidity or ambition of others, are parents of invention far more prolific than the pure love of science.

The rotation of crops was practised by farmers long before botanists could give any reasonable explanation of its advantages. Thus art anticipates science. In mechanics, practice almost invariably precedes theory, and often by very long periods. 'Tis but a few years since Dr Joule determined the thermal equivalent for mechanical work. For ages upon ages the fact had been utilized by savages, when they rubbed together two dry sticks to procure fire. Suction-pumps had been at work for hundreds of years before Torricelli showed the true cause of the suction. The boomerang is used by Australian savages, the screw-propeller moves the navies of great nations,

and yet science has not yet thoroughly explained the actions of these two instruments.

Our purely scientific knowledge in physiology and pathology keeps us from falling into errors; it saves us from being "carried about by every wind of doctrine;" it is often a trustworthy guide in the prevention of disease, and not unfrequently in its treatment; it may, possibly, at some distant day, become the sole basis of medicine, and may enable physicians to select their remedies for disease by simple deduction from established laws; but, until that happy time arrive, we must be content, and only too glad, to make use of all the powers that nature has revealed to us, whether we have, or have not, a clear view as to their modes of operation.

Practical medicine, as I have already said, has had modes of progress of its own. The independent study of disease has often led to real knowledge in advance of physiology. In some cases the method has been inductive, the results of a large experience being generalised into principles and methodised into rules for practice. The work done by Sydenham is an illustration:—the more apt,

because he was so exclusively a "practical man." He derived no aid from physiology, and sought none from it. He considered that the province of the physician was "to cure disease, and to do naught else" (*h*). He not only rejected the flimsy doctrines of his time; but he seems to have had little regard for the actual facts of physiology, and no faith in its future. The circulation of the blood was the great new fact of his age; yet in all his works he refers to it only once or twice, and then cursorily and not very aptly. "What microscope," he exclaimed, "shall exhibit those ducts, through which the blood, conducted by the arteries, is passed onwards to the orifices of the veins?" (*k*) Which same ducts were actually exhibited under a microscope very few years after his words were written. In all his works I have found post-mortem appearances noticed only twice, and then without the exactness, which is so characteristic of his description of symptoms. He looked for the advancement of medicine only in minute observation of the living phenomena of disease, and in attentively watching the effects of remedies and external circumstances; yet, studying these 'by way of experiment,' he accomplished a great reform in the

practice of medicine, and gave his name to an epoch. The chief source of his power was in a mind singularly independent and candid, but little biassed by the theoretical notions with which physicians of his time were so pre-occupied. He could see things as they were, not as they were supposed or expected to be. He, thus, not only improved the treatment of certain diseases; he did more—he showed by his example *how* such improvements might be effected, through a constant and candid appeal to facts, and a distrust of mere doctrines or opinions. Subtle disputations, he says, “are as useful to physicians in driving away diseases, as music is to architects in building houses”(l). Mere opinions without facts are, he says, “only the shadows of the shade of reason”(m). He proved that practical medicine *could* stand on its own strength, and could *not* be trusted when based on a crude and imperfect physiology. He proved that, in his time at least, the practical study of disease was the readiest way of advancing medicine.

But induction from the results of a large experience is not the only process by which

Practical Medicine is advanced independently of Physiology. There is another way, which I have already mentioned—the immediate application to practice of a single observed fact. The instances are innumerable. They include most of the minor improvements in medicine, and they include improvements of the very highest importance. Such was the observation that the inhalation of ethereal vapours causes a transient insensibility to pain. Such was the observation that lemon-juice prevents scurvy: who can calculate how much suffering, and how many lives, it has saved; how much the commerce of the world and the influence of England owe to this one observation? Such, or nearly such, was the invention of vaccination by Jenner. We cannot rank this among great intellectual achievements; but if we estimate it by its immediate fruits, we must call it, as Dr Baillie did, “the most important discovery ever made in medicine.”

He spoke as an eye-witness of small-pox in the days when it raged unchecked in all its loathsomeness and fatality. *We* at least know for certain, that the lives saved every year by Vaccination must be reckoned by tens of thousands.

Jenner owed to a popular rumour his first idea of the protective power of cow-pox. By the way of *experiment* he established its truth. By experiment also he proved, that the vaccine virus could be transmitted from one human being to another and still retain its pristine virtue, and so could be provided fresh for all times and seasons, independently of the casual outbreaks of the disease among cows. Moreover, he found out, and laid down clearly, rules for the practice of Vaccination, the observance of which was, and is, essential to its success. And, in justice to Jenner, it must not be forgotten, that the idea of permanently and harmlessly and specifically modifying the constitution of a man by imparting to him the disease of a beast was an idea entirely new to science.

Perhaps we may draw some moral from the fact, that a man unobtrusive and unambitious, and not distinguished above his contemporaries for the powers of his genius, did yet become one of the greatest benefactors of his race. It was not, that to Jenner's ears alone the popular rumour had reached, that the cow-pox was a protection against the small-pox. Many and able men had heard of it(*n*). Nor was it merely the opportunities af-

forded by his residence in a dairy-country: for many others were similarly situated. Something was doubtless due to his taste for Natural History and his habits of observation, and more to the influence of his great master John Hunter's example and well-known maxim—a brief paraphrase of Harvey's injunction—not to think only, but to *try*; but most of all Jenner owed to his own modest and teachable spirit, that did not disdain the popular belief of unlearned peasants, but could admit it as a subject for scientific examination, and discern in it a truth of matchless importance.

It may not be amiss to notice, that the calm wisdom of Harvey had commended that very frame of mind which many years afterwards led Jenner to his happy discovery. Not only did Harvey enjoin the searching out the secrets of Nature by way of experiment; but this man, full of knowledge, tells us:—"True philosophers never regard themselves as already so thoroughly informed, but that they welcome further information *from whomsoever and whencesoever* it may come" (*p*). And "the studious and good and true...know full well that many things are discovered by accident, and that many may be learned indifferently *from any*

quarter, by an old man from a youth, by a person of understanding from one of inferior capacity”(q).

Is there nothing more that we may learn from Jenner's discovery? Is there nothing it may suggest?

The protective power of Vaccination appears at present as an isolated fact. An isolated fact in Science(r). But is it an isolated fact in *Nature*? Is it probable that there is but a single instance in Nature of one disease so modifying a man's constitution, as to render him less liable to another disease? Or—to extend the question—is there no manageable agency, derivable from animal life or any other source, by which such a modification might be effected? How are we to explain the familiar fact that some individuals escape a fever, when others, perhaps less exposed, catch the infection? A *greater liability* may be attributable to a low, ill-defined condition of the general health:

“Some low fever ranging round to spy
The weakness of a people or a house,
Like flies that haunt a wound, or deer, or men,
Or almost all that is, hurting the hurt.”—

But the *immunity* from such attacks is plainly not

due in all cases to superior health or robustness of constitution. Instances are familiar of one or two escaping, when an infectious fever runs through a family, and those who escape the infection are by no means always the strongest. The baby often escapes while older children are attacked. Persons above 40 years of age are less liable to typhoid fever than those who are younger; yet not from greater strength of constitution; for, if attacked, they are more likely to succumb.

Is not the immunity in such cases due to some protecting cause, which we do not discern? And, if it be, is the cause one, which we could put into operation, as we use vaccination for the prevention of small-pox? Do not some of these familiar facts seem as if they were pregnant with secrets, which lie before us, as Jenner's lay before him, ready to be disclosed to clear, impartial eyes(s)?

The three signal examples I have given illustrate three characteristic ways in which Physicians "search out the secrets of Nature;" and invention is ever busy in helping their search by devising new instruments and new methods of investiga-

tion. 'Tis true no compass has been found to guide them in the trackless ocean of the unknown, yet have they owed much to instrumental aid. It has made their observations more exact. This is a great service. We cannot too highly value any means of rendering our knowledge more accurate.

Such is the laryngoscope, first used by one recently lost to us, who, however distinguished for his inventive skill, for his practical medical knowledge, and many accomplishments, was even more beloved for his goodness of heart and generous, genial spirit (*t*).

Such are the ophthalmoscope and endoscope, and the use of the thermometer in practical medicine; such the endless applications of the microscope; such the methods of auscultation and percussion; such, above all, the progress of Organic Chemistry—as applied to medical practice, giving more precision to our remedies—as applied to Physiology, ever bringing us nearer and nearer to the solution of the higher problems of life.

There is no more hopeful sign for the future of Medicine than the general recognition at the present day of the necessity for exactness in all state-

ments and investigations. It is at once an assurance of steady progress, and a safeguard against the premature generalizations which sorely tempt medical enquirers. It promotes a wholesome criticism of new views, and is a check on their too hasty adoption.

In following the course of men who have searched out the secrets of Nature, we have had regard chiefly to the intellectual powers and processes by which their success was achieved. Let us now, with one more instance, illustrate their moral qualities and temper of mind. Let our instance be the improvement in the treatment of the insane.

We may doubt whether anything in the history of the world can be found more sad, or more humiliating to the pride of civilization, than the descriptions we have of the condition of lunatics in the time of our fathers. The influence of kind and gentle treatment was unknown. It had not been tried. Those who aimed at controlling the lunatic sought to do it by severity—by inspiring him with awe and dread. The bath of surprise and the whirling chair were among the most refined of

their remedies. Various restraints of a painful kind were regarded as not only necessary, but beneficial. Many persons supposed that insanity was incurable, and that little or nothing could be done for the unhappy sufferer, but secluding him from the eyes of the world, and preventing him from injuring himself and others. Public asylums were looked upon rather as prisons for dangerous persons than as hospitals for the cure of their malady. Hence it followed, that the treatment of the insane too often passed from the hands of the physician into those of men destitute of all medical knowledge, unfeeling and unprincipled. Then came neglect and cruelty, and all the horrors of which we read:—the manacles and fetters; the iron collars by which the poor creatures were chained to the walls, incarcerated for years in narrow cells, dark, damp, and cold, like mediæval dungeons, and filthy beyond description, or in cages in which they were exposed as a sight for public curiosity, and made a show of like wild beasts; their beds the bare ground, or straw seldom changed; their scanty clothing, or very nakedness; the blows and stripes that aggravated at once their sufferings and their malady, and debased them below the very

beasts of the field—below all, except their own brutal keepers.

It is difficult to realise, that this state of things was common in civilized Europe at the close of the last century; and that little more than fifty years ago it had not been wholly abolished in this very town—in the metropolis of the land, where Howard had long before rescued the worst felons from such misery and degradation. And all this suffering and humiliation heaped upon poor creatures, whose only fault was the most terrible and grievous of human afflictions!

Honour to those whose wisdom and courage changed all this! who opened men's eyes and hearts to its folly and its shame!

The honour of the first step is due to Tenon, who published in 1786 the first suggestions for an humane and gentle treatment. Daquin went further. In an *Essay on Insanity* (*v*), written in 1791, and dedicated to *Humanity*, he not only condemned cells and chains, but recommended good diet, fresh air, exercise and occupation. He urged the advantages of rational and moral treatment, and of gentleness mingled with firmness. He had tried these remedies in the Asylum at Chambery,

and had proved their advantages. In urging their adoption he certainly anticipated Pinel, and merited an equal fame.

Pinel, it is said(*w*), was first drawn to the study of Insanity, in 1785, by the shocking fate of a young friend of his, who became maniacal through excessive study, and in that state escaped from his father's house into the neighbouring woods, and was there devoured by wolves. About this time an asylum was established for the treatment of the insane. The first patient was sent to it by Pinel, and it was there apparently he made his first trial of humane and rational treatment. In 1791 the Société Royale de Médecine offered a prize for an essay on the most efficacious means of treating mental derangement. Pinel was one of the competitors. Whether he was successful, or not, is unknown. The political convulsions of that time have left a gap in the records of the Society.

In 1792, amidst the tempest of the French Revolution, Pinel was appointed physician to Bicêtre, and there found work to do, the doing of which has for ever placed his name high among the great and the good.

The state of the lunatics in that great Parisian Hospital exemplified all the horrors I have mentioned. Their attendants were malefactors drawn from prison. The madmen were such as were supposed to be incurable. Many of them, irritated by barbarous usage, had become ferocious and revengeful. Chained though they were, they were dreaded by their keepers. To Pinel they were objects of pity, for *he* recognised in their paroxysms of fury only the natural outburst of indignation at their wrongs.

He applied to the authorities for permission to remove the chains. Their only answer was to call him an aristocrat, an epithet then almost equivalent to a death-warrant. He then went in person to the Commune, and pressed his suit earnestly and warmly. At length it was answered by the wretch Couthon, who said he would visit Bicêtre, and see whether some of the enemies of the people were not concealed among the lunatics. The sights and sounds that met him there soon put an end to his search. He broke off with an exclamation, that Pinel must be mad himself to think of unchaining such animals. The required permission was granted, but not without a warn-

ing to Pinel that he would fall a victim to his temerity.

Then did Pinel, by one courageous and decisive experiment, prove for ever the soothing influence of humanity and kindness. He began by releasing twelve of the madmen from their chains. How instructive are some of the details(*x*)! what a picture is that of the first who was unchained, the English captain, of whose history no one knew anything, except that he had been in chains for five and forty years. He was regarded as the most dangerous of all, for in a fit of fury he had killed one of his keepers with a single blow of his manacles. Pinel enters his cell alone, and addresses him calmly, "Captain, if I were to have your irons removed, and give you liberty to walk in the court, would you promise me to be reasonable and not to hurt anybody?" "I promise; but you are making game of me; they are too much afraid, and so are you." "I'm not afraid, for I've six men at hand to make me respected, if necessary; but take my word, I will give you your liberty, if you will let this waistcoat be put on you instead of the irons." The captain submits willingly, shrugging his shoulders, without

a word more. The chains are removed and he is left in his cell, the door open. Several times he raises himself from his seat and falls again; he had been sitting so long that his legs were stiff and weakened by disuse. In a quarter of an hour he gets on his feet, and comes tottering to the door of his cell. His first look is at the sky, and he cries out in ecstasy, "How beautiful!" During the rest of the day he is constantly in motion, walking up and down the staircases, and exclaiming again and again, "How beautiful! how good!" At night he returned of his own accord to his cell, slept tranquilly on a better bed which had been prepared for him, and during the two remaining years he passed at Bicêtre his paroxysms not once returned, and he made himself useful in the house, exercising a certain authority over the lunatics.

Then the second, an old French officer, who had been in chains thirty-six years. His maniacal delirium had ceased, but reason had not returned. He sat mute and motionless, with rigid and shrunken limbs, still in the same chains, though he had become too weak even to lift them. They were removed, and he was carried to bed in the

infirmary, and lived some months longer; but never became conscious even of his deliverance.

Three unhappy Prussians who had been chained for many years, nobody knew why, and who were habitually calm and inoffensive, resisted violently the removal of their chains, and would not leave their cell, apparently suspecting that some worse mischief was intended.

Perhaps the most interesting of all was a French soldier, famous for his enormous strength, who had been fastened to the wall with a chain and iron collar for ten years. Pinel perceived that his excitement was kept up only by ill usage, and on liberating him, at once engaged him to assist in releasing others, and promised, if he behaved well, to take him into his own service. The cure was completed on the instant. From that moment the man became a model of good conduct and gratitude. During the reign of terror he several times saved the life of Pinel, and the rest of his days were spent in one continuous act of devotion to his benefactor.

In the course of a few days fifty-three of the poor creatures were liberated from their fetters; and then tranquillity came over the place, which

had so long resounded with cries and howlings and clanking of chains.

It was the close of the year 1792. Terror then reigned throughout the rest of Paris. But within the walls of Bicêtre an ancient reign of terror had ceased; a bloodless revolution had been accomplished by the courage, humanity and wisdom of one man searching for nature's secrets by experiment.

Amidst this blessed calm of his own creating Pinel pursued his study of mental disorder; regulating with assiduous care the internal arrangements of the hospital, comparing month by month and year by year the results of his treatment; for severity and violence substituting the moral influence of gentleness and kindly attention, and seeking to correct what was deranged by appealing to what was yet sound. He thus established a system of treatment which soon bore the fruits of numerous and striking cures.

The treatise, in which he gave to the world, in 1801, the happy results of this large experience, must ever mark an epoch in the history of medicine. It made inexcusable the gross ignorance

which had prevailed respecting the habits and tendencies of the insane. It established principles for their treatment equally sagacious and humane.

The treatment of insanity by some English physicians of our own time illustrates what Bacon calls *Productio Experimenti*. Pinel's experiment has been extended; it has been urged to an *effect more subtle*. When an experiment is thus pushed further, the result, we know, in some cases differs from the original conclusions. These are proved to be less general than had been supposed, and lose their pretensions as laws of nature. How then has it fared with Pinel's conclusions when tried by an extension of his experiment?

Pinel and the early reformers, while abolishing chains and other obviously degrading means of restraining lunatics, did not deem it expedient to abolish *all* mechanical restraints. They still employed the strait waistcoat in certain emergencies; and other physicians, less enlightened than they were, continued the use of various mechanical contrivances both hurtful and humiliating to the patients. To abolish these, was to repeat Pinel's

experiment: to abolish also the strait waistcoat and all mechanical restraints whatever, was an *extension* of the experiment. This trial was first made at the Lincoln Asylum. Under the directions of Dr Charlesworth the various instruments of coercion were, in the course of years, one by one discontinued, until in 1837, when Mr Gardiner Hill was house-surgeon, the last mechanical restraints were wholly abolished.

In June 1839 Dr Conolly was appointed resident physician at Hanwell. In September he had abolished all the mechanical restraints. The experiment was a trying one, for this great asylum contained eight hundred patients. But the experiment was successful; and continued experience proved incontestably, that, in a well ordered asylum, the use even of the strait waistcoat might be entirely discarded. Dr Conolly went further than this. He maintained, that such restraints are in all cases positively injurious, that their use is utterly inconsistent with a good system of treatment; and that, on the contrary, the absence of all such restraints is naturally and necessarily associated with treatment such as that of lunatics

ought to be, one which substitutes mental for bodily control, and is governed in all its details by the purpose of *preventing* mental excitement, or of soothing it before it bursts out into violence. He urged this with feeling and persuasive eloquence, and gave in proof of it the results of his own experiment at Hanwell. For, from the time that all mechanical restraints were abolished, the occurrence of frantic behaviour among the lunatics became less and less frequent(*y*).

Thus did the experiments of Charlesworth and Conolly confirm the principles of treatment inaugurated by Daquin and Pinel; and prove that the best guide to the treatment of lunatics is to be found in the dictates of an enlightened and refined benevolence. And so the progress of science, by way of experiment, has led men to rules of practice nearer and nearer to the teachings of Christianity. To my eyes a Pauper Lunatic Asylum, such as may now be seen in our English counties, with its pleasant grounds, its airy and cleanly wards, its many comforts, and wise and kindly superintendence, provided for those whose lot it is to bear the double burthen of poverty

and mental derangement—I say this sight is to me the most blessed manifestation of true civilization that the world can present.

This result we owe to the courage and philanthropy of such men as Pinel and Conolly. Pinel's large acquirements and practical intellect would alone have availed nothing; his first step would never have been taken, but for the generous impulses of a feeling heart and courageous spirit. Conolly's experiment at Hanwell would have been foiled by opposition and discouragement, had he not been sustained by a spirit of earnest benevolence towards his unhappy patients.

The spirit which animated these two men is the spirit without which much of the progress of practical medicine would have been impossible. For, however diverse may be the intellectual powers that find their several fit places in the study and practice of medicine, there is but one right temper for it, the temper of benevolence and courage; the temper in which Larrey invented the *ambulances volantes*, that he might bring help to the wounded under fire; the temper in which physicians have devoted themselves to

the study of the plague and other infectious fevers; that same temper which has originated and sustained the highest Christian enterprizes, and which ennobles any man, who, possessing it, with an honest and true heart does his duty in our profession.

NOTES.

Note (a). Page 6.

Long even before the general discovery by Fabricius, the valves at the orifices of the venæ cavæ and pulmonary veins, and of some other veins, had been described by Béranger and Sylvius; those at the orifices of the hepatic veins had been noticed by Stephanus and Vesalius; those of the coronary veins by Eustachius; those of the crural by Posthius; those of the renal by Alberti; those at the orifice of the vena azygos by Cannani.

Moreover the general existence of valves in veins was discovered by Paul Sarpi at the same time as by Fabricius. The fact must therefore have been familiarly known among anatomists long before Harvey's discovery of the circulation.

Sprengel, Vol. iv. Sect. 11, ch. 4.

Note (b). Page 6.

One reason why Harvey's demonstration did not at once carry conviction to all who read it, may have been

its dependence on observations on living animals, and particularly on the hearts of living animals. The many who had not the opportunities or skill for repeating such observations and experiments, would not appreciate the cogency of their evidence. Those who had witnessed them in Harvey's lectures at the College of Physicians were convinced. None of the Fellows of the College were among Harvey's opponents in the controversy that ensued.

Note (c). Page 8.

Harveii Opera, a Coll. Med. Lond. edit. 1766. pp. 162, 169.

Harvey's *Works*, translated by Dr R. Willis, for the Sydenham Society, 1847, pages 146, 153.

Note (d). Page 8.

Harveii Opera, pp. 19, 37. *Translation* by Willis, pages 19, 35.

Note (e). Page 8.

Harveii Opera, pp. 27, 58, 86. *Translation* by Willis, pages 25, 59, 84.

Note (f). Page 8.

Harveii Opera, pp. 73, 91, 92, 112. *Translation* by Willis, pages 72, 89, 90, 112.

Note (g). Page 8.

It seems strange that Harvey's "Medical Anatomy" or "Medical Observations" was not published after his death, and still stranger that this and other writings of his should have been lost, and that we should have no satisfactory account of the loss. I will venture to reproduce here some remarks I published on the subject in 1850.

In the Life prefixed to the College-Edition of his works we are informed that he had written

Observationes de usu lienis.

Observationes de motu locali.

Tractatum Physiologicum.

Observationes Medicinales.

De Amore, Libidine et Coitu animalium.

In the same place it is stated that all these writings, together with observations on the generation of insects, perished when his house was plundered by parliamentary soldiers in the civil war. No authority is given for this statement; and its correctness may justly be disputed. In regard to more than one of the writings it is almost certainly erroneous.

Aubrey, who had from Harvey himself the fact that he had thus lost some very valuable papers, tells the story as follows: "He had made dissections of frogs, "toads, and a number of other animals, and had curious "observations on them, which papers, together with his "goods, in his lodgings at Whitehall, were plundered at

"the beginning of the Rebellion; he being for the King
 "and with him at Oxon" (Aubrey's Letters and Lives).
 From this account we should infer that the papers that
 were lost were not on any of the five subjects above
 mentioned, but rather on Comparative Anatomy.

Besides, let us consider the time of the occurrence.
 The "beginning of the Rebellion" fixes the date probably
 in 1642. In that year Charles I. left London; in August
 he raised his standard at Nottingham, and the civil war
 commenced. In October of the same year was fought
 the battle of Edge-hill, at which Harvey was present.
 Soon after this he retired with the King to Oxford, where
 he remained until 1646, when he returned to London
 and began to live with his brothers. It is therefore cer-
 tain that this loss of his papers at Whitehall could not
 have occurred subsequently to 1646.

Now the *Observationes Medicinales* are referred to,
 and their future publication promised, in works that
 were written by Harvey long subsequently to 1646.
 They are thus referred to in his second Exercise addressed
 to Riolan. *Ex. gr. "De quibus omnibus, in observationi-
 bus meis medicinalibus, admiratione digna tradam"* (Coll.
 Ed. p. 129, Willis' Ed. pages 129, 130), and "*Inter Medi-
 cinales Observationes, et in pathologia, ea tradere potero,
 quæ nunquam hactenus a quovis observata comperio*" (Coll.
 Ed. p. 141, Willis' Ed. p. 141). This Exercise was first
 published at Cambridge in 1649, and was in answer to
 part of a work of Riolan's which had been published
 in the same year. It is manifest, therefore, that the
Observationes Medicinales could not have been lost at

Whitehall, but were in Harvey's own possession in 1649.

For similar reasons, it is most probable, that what he had written "*De Amore, Libidine et Coitu Animalium*" was in existence in 1650, the year in which his Treatise on Generation was published. (See the end of the 6th Chapter.)

There are indications in Harvey's published works of his having made other researches besides all those which have been enumerated. Thus, in his letter to Slegel (1651) he mentions that he had been preparing an answer to Riolan's more recent arguments. Again, in the Treatise on the motion of the Heart and Blood are passages which show that he had assiduously investigated the physiology of the lungs (Willis' Ed. pages 40, 80); and that his researches had not been unattended with success may be inferred not only from the terms in which he mentions them (*multa quamplurimis observationibus a me deprehensa*), but also from the remarkable passage in his Chapter on Parturition, which indicates that he had obtained a glimpse, at least, of one of the true uses of air in respiration.

If any further argument be needed to prove that many writings of Harvey's must have existed and been in his own possession subsequently to the plundering of Whitehall, we may be satisfied by the following extract from his will. "Touching my bookes & household stuffe
"Pictures & apparell of which I have not already disposed, I give to the Colledg of Physicians all my bookes
"& *papers* & my best Persia long Carpet...Item I give

“my velvet gowne to my lo friend M^r Doctor Scarbrough
 “desiring him & my lo friend M^r Doctor Ent to looke
 “over those scattered remnant of my poore Librarie &
 “what bookes, *papers* or rare Collections they shall
 “thinke fit to present to the Colledge & the rest to be
 “sold & with the money buy better.” (Harvey’s Works,
 edited by Willis, p. xciii.)

It is therefore certain

1. That Harvey wrote much more than has yet been published.
2. That many of his writings which are supposed to have been lost or destroyed at the plundering of Whitehall did not so perish, but were in Harvey’s own possession long subsequently to that occurrence.

And it is probable

That some of his unpublished papers passed after his death into the hands of Drs Scarbrough and Ent, and from them to the College of Physicians. It is at least presumable that these papers were valuable, and it is surprising that they were never published, the more so when we consider the estimation in which Harvey was held. His dissection of Thomas Parr, the MS. of which was in private hands, was published in 1669, *i.e.* twelve years after his death; and some Anatomical notes attributed to him were read at a meeting of the Royal Society in 1687, and published in their proceedings. Yet of his papers bequeathed to the College of Physicians we know

nothing, except that they cannot now be discovered, and have probably been out of the possession of the College for a very long time. It may be conjectured that they perished in the great fire of London, when, as we know, the College was destroyed; but this is merely a conjecture, and is somewhat weakened by the fact that the Annals of the College, written by Dr Caius, the second President, have been preserved to the present day. "*Omnia periere*," says Dr Walter Charlton, referring to the conflagration of the College, in his Harveian Oration for the year 1680; but he does not specially mention manuscripts among the valuables that were destroyed.

Note (h). Page 15.

Thomæ Sydenham Opera Omnia. Ed. Greenhill, 1844, p. 362.

Sydenham's *Works*, translated by Dr R. G. Latham, 1850, II. 83.

Note (k). Page 15.

Opera, p. 464. *Translation*, II. 171.

Note (l). Page 16.

Opera, p. 116.

Note (m). Page 16.

Opera, p. 341. *Translation*, II. 65.

Note (n). Page 18.

Among others, John Hunter and Sir George Baker.

Note (p). Page 19.

Harveii Opera, p. 6. *Harvey's Works*, by Willis, p. 6.

Note (q). Page 20.

Harveii Opera, p. 7. *Translation*, by Willis, p. 7.

Note (r). Page 20.

In the opinion of Jenner small-pox and cow-pox were the same disease; cow-pox was small-pox in the cow; vaccinia was variola, which had been modified by the constitution of the cow. Later researches seemed to confirm this opinion, which was all the more probable, in that it connected the fact of the protecting power of vaccination with the familiar fact of the protecting power of a first attack of small-pox. But the recent researches of the Lyons Committee furnish evidence which seems conclusive against this opinion. They have proved that small-pox and cow-pox, though closely allied, are yet distinct diseases. So the protective power of vaccination has become an isolated fact.

Note (s). Page 21.

How little exact knowledge have we of the conditions of the body which increase or lessen the liability to

infection from the various fevers ! Instances are familiar to every medical practitioner, in which a person apparently in good health catches a fever, to which, on a prior occasion, he had been exposed with impunity. A wife, who has nursed her husband through scarlet fever without suffering from the infection, takes it some years afterwards while nursing her daughter. A medical practitioner, who all his life had been attending cases of fever with impunity, catches it in his old age.

Again, with respect to the protective power of Vaccination, and why it is perfect in some persons, imperfect in others? Are *all* the latter cases to be explained by want of care in vaccinating, or lapse of time since the operation? May not the liability be due in some cases to a fresh modification of the constitution, ex. gr. by some disease subsequent to the vaccination?

At Swavesey, a populous village in Cambridgeshire, typhoid fever was very prevalent and severe in the year 1850. In 1865, on the occurrence of a few cases of small-pox, the inhabitants in alarm, young and old alike, sought for vaccination. Accordingly, Mr Daniell, the resident surgeon, vaccinated 616, many of whom had been vaccinated on a former occasion, or had had small-pox. Of course in a large proportion of these Mr Daniell's vaccination was unsuccessful, but by no means in all such. Mr Daniell noticed the remarkable fact, that the cases of successful vaccination, in those who had been already vaccinated or had had small-pox, were chiefly, or almost wholly, among persons who had had the typhoid fever in 1850.

One was a woman aged 60, who had had small-pox 20 years before, and whose face was pitted with it. The vaccination proceeded with her quite normally. She had had typhoid fever severely in 1850. It seemed as if the typhoid fever had not only produced a permanent effect on the constitution, but removed that condition of inaptitude for vaccination which had been left by the first vaccination or by the small-pox.

Note (t). Page 22.

Dr B. G. Babington.

Note (v). Page 25.

“La Philosophie de Folie, ou Essai philosophique sur le Traitement des Personnes attaquées de Folie.” Par Joseph Daquin. 8vo. Paris, 1792.

The Censor's report and Imprimatur are dated Chambery, July, 1791.

This little essay is in many respects a very remarkable production. The only copy of it, with which I have met, is in the Library of the Royal College of Surgeons of England.

Note (w). Page 26.

“Éloge de Pinel,” par Pariset, *Mémoires de l'Académie Royale de Médecine*. 1828.

Note (x). Page 28.

“Bicêtre en 1792.”

“De l’abolition des chaines.” Par Scipion Pinel.
Mémoires de l’Académie Royale de Médecine. 1836.

Also in *Traité du Régime sanitaire des Aliénés.* Par Scipion Pinel. Paris, 1836.

Note (y). Page 34.

Time would not have permitted me to give a complete history of the improvement in the treatment of the insane, even if I had proposed so to do. Such a history would have required some reference to the efforts of the philanthropic Duc de la Rochefoucauld, and a more particular mention of the aid given to Pinel by his assistant, Pussin, an unlettered but very remarkable man. Living in the midst of the insane, Pussin had a thorough insight into their habits and all their symptoms. From him Pinel acquired much knowledge of details. He had even anticipated Pinel in venturing to release some of the madmen from their fetters. When, three years after the reform at Bicêtre, Pinel was solicited to undertake the like task at the other great hospital, La Salpêtrière, he made it a condition of his acceptance of the charge, that there also he should have the aid of Pussin.

The reform in the treatment of lunatics in England had its origin in the well-known “Retreat,” near York. This asylum was projected by William Tuke and other

members of the Society of Friends, in 1792, and was opened for patients in 1796, the first physician being Dr Fowler. The first superintendent having died at the end of two months, William Tuke, though not a member of the medical profession, undertook the office for nearly twelve months, until a suitable successor could be found. George Jepson, who was then appointed to be resident apothecary and superintendent, contributed much to the success of the gentle treatment.

(See *The Description of the Retreat*. By Samuel Tuke. 1813.)

There are reasons for believing that the chief public asylums of England were in a better state than those of France prior to Pinel's reform at Bicêtre and La Salpêtrière.

Tenon in his *Mémoires sur les Hôpitaux de Paris*, 1788, says (p. 393):

“Les deux hôpitaux de fous les mieux conçus que nous connaissions sont ceux de Bethléem et de S. Luc à Londres, &c.”

And again, in his Preface, p. xxv.:

“Le premier remède est d'offrir au fou une certaine liberté, de faire qu'il puisse se livrer mesurément aux impulsions que la nature lui commande; ce qu'on a très bien compris et exécuté aux Hôpitaux de Bethléem et de Saint-Luc à Londres.”

Daquin, in his Essay already referred to, when treating of the construction of Asylums, adds in a note:

“Il y a à Londres l'hôpital de Bethléem où les fous

sont traités avec toute l'humanité et tous les soins imaginables, et à Manchester on en a bâti un depuis peu, où, d'après le compte qui en a été rendu, les succès ont été étonnans."

In *Observations on Insanity*, by John Haslam, Apothecary to Bethlem Hospital, 1798, the moral treatment recommended is kindly and not unwise. (I have not seen the Edition of 1794.)

It seems as if the treatment of lunatics in Bethlem Hospital had deteriorated between the end of the last century and the time of the Parliamentary Enquiry in 1814. The period was one in which the thoughts and energies of England were engrossed in war.

According to Daquin (*Op. cit.*) the Knights of Malta were accustomed to treat their insane patients with gentleness and kindness. If so, they were not altogether degenerate representatives of their once famous order.

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