MASTERS OF MEDICINE

"HOMINES AD DEOS NULLA IN RE PROPIUS ACCEDUNT QUAM SALUTEM HOMINIBUS DANDO."
CICERO.

Masters of Medicine

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CLAUDE BERNARD

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BY

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NEW YORK
LONGMANS, GREEN & CO.
91 & 93, FIFTH AVENUE
1899

Dedication.

TO THE PHYSIOLOGISTS OF FRANCE,

BOTH TO THOSE WHO HAD THE HAPPINESS TO KNOW CLAUDE BERNARD IN THE FLESH, AND TO THOSE WHO, LIKE MYSELF, NEVER SAW HIS FACE, THIS LITTLE SKETCH IS DEDICATED IN THE HOPE THAT AS HE HAS BEEN TO ME A FATHER IN OUR COMMON SCIENCE, SO I MAY BE ALLOWED TO LOOK UPON THEM AS BRETHREN.

M. FOSTER.

PREFACE

SOME may think that in the pages which follow too much space is given to an exposition of Claude Bernard's scientific work and too little to the details of his life as a man. Although the real life of every great man of science lies in the story of his scientific work and not in the tale of how he passed his days, we all of us wish to clothe the image which we have formed of a man whom we know by his writings only, with as many details as we can gather, of how he moved among his fellow-men, and what befell him on his path through life. And had I been able to do so, I would have added much to what I have written. But the details which can now be gained of Bernard's daily life are very scanty. I can only say that I have done my best; and the little which I have been able to do has been made possible by the kindness of my friends, especially of Prof. A. Dastre, who now holds Bernard's chair at the Sorbonne; of Prof. C. Richet, of the École de Médecine; and of Prof. W. Kühne, of Heidelberg, who was once Bernard's pupil.

CAMBRIDGE,
May 3, 1899.

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CLAUDE BERNARD

I

Early Days

THE traveller speeding southwards by the Paris, Lyons and Mediterranean express, some little time before he reaches Lyons rushes through the station belonging to the town of Villefranche. Not far from that town—two or three leagues distant—lies the little village of Saint-Julien (Rhône), in which on July 12, 1813, Claude Bernard was born. His father was, in common with most of his neighbours, the humble proprietor of a small estate chiefly planted with vines, and derived at least most of his income from making wine. The whole district was, and indeed still is, a wine-producing land; it is part of the old province of Beaujolais, and the wine which it yields bears that name.

The little estate eventually came into Bernard's hands, and so soon as he could afford it, or at least in his later years, he used the paternal cottage as a summer residence during the vacations. Here he yearly renewed his strength by touch with his native earth, exchanging Paris for the simplicities of country life, and mingling quiet literary labours with the amusement of watching the vintage of his own wine. He thus himself describes his home:

"My dwelling is on the hill slopes of Beaujolais which look towards the Dombe. The Alps give me my horizon and when the air is clear I can catch sight of their white summits. At the same time I see spread out before me for two leagues the prairies of the Saône. The slope on which I dwell is surrounded on all sides by vineyards stretching away apparently without limit; these would give the country a monotonous appearance were not this broken by wooded valleys and brooks running down from the mountains to the river. My cottage, situated though it is on a rise, is a very nest of verdure, thanks to a little wood which shades it on the right and to an orchard which flanks it on the left; a great rarity in a land in which they stub up even the coppices in order to plant vines."

In this quiet, out-of-the-way spot, the future physiologist began life as a member of a homely family, whose aspirations scarcely went beyond securing their daily bread. As a child he must have been bright, for the curé took him under his special charge, making him a choir boy and teaching him Latin. He afterwards was sent to the school or so-called college at Villefranche, directed by the Jesuits.

When he had learnt as much as his teachers could teach him at the college, he was sent to Lyons, with the view probably, in the first instance, of his completing his studies and obtaining the Baccalauréat. But his student career at Lyons could not have been a very lengthy one, for he soon entered into active life, exchanging the school for the shop. The reasons which led to this step cannot be ascertained, but they do seem to have been financial. The family, though humble, were not without resources. Claude appears to have been an only son, with one sister, wh eventually married a neighbouring proprietor. The father seems to have been still alive (though he is said to have died while Bernard was yet young) and not yet to have met with the reverses which subsequently crippled the family. Be it as it may, Bernard, somewhere in his teens, became engaged at Lyons in practical pharmacy, having obtained a situation with a pharmacist in the Faubourg de Vaise of that city. He received at first nothing more than his board and lodging for his services in the shop, though after some months, his manual dexterity, shown by the singular neatness of his "dispensing," was rewarded by a humble salary.

Here for some time, probably for two years, Bernard spent most of his days as a pharmaceutical assistant, dispensing medicines, and carrying them to the clients, especially to the Veterinary School. What

opportunities for pharmaceutical study h
had does not appear; but his patron'
mode of conducting his business served t
awake in him early the spirit of medica
scepticism. As was usual at that epoc
the clients of the shop, especially the ol
women of the outlying villages, made
constant demand for a syrup which seeme
to cure everything; and Bernard, to hi
astonishment, found that this favourit
syrup was compounded of all the spoil
drugs and remnants of the shop. When
ever Bernard reported that a bottle o
stuff had gone wrong, "Keep that fo
syrup," replied the master; "that wi
do for making the syrup."

No wonder that Bernard's mind turne
with avidity to other things than pharmac
In common with many of the young me
of his time, he was filled with literar
aspirations; and he was particularly draw

towards the dramatic art. All his free evenings he spent at the play, at the Théâtre des Célestins, and was moved to write himself a vaudeville comedy, entitled "La Rose du Rhône," which was not only accepted, but had a certain success on the boards, though it was never printed.

Encouraged by the result of this first effort, he set himself to the serious work of writing a historic piece in the conventional five acts, giving it at first the form of a tragedy in metre, but subsequently changing it to a prose drama. With this he determined to seek his fortune in Paris. He had in Lyons two friends, like himself, students and poor. All three thought that Lyons was too small a sphere for their abilities, all three determined that the proper place for them was Paris. They parted from each other at Lyons, giving each other the rendezvous in Paris, or as they called it, "la scène du Monde." And Bernard used to tell afterwards, in a joking spirit, how by chance they did later on meet together in the Place du Panthéon, in front of the well-known inscription "Aux grands Hommes la Patrie reconnaissante"! And though the omen did not hold so good for the other two as it did for Bernard, yet one became a Bishop, and the other a Director of Railways.

Bernard, with the help of the few francs which his little comedy had put in his pocket, started in the diligence for Paris, armed with his manuscript of "Arthur de Bretagne," carefully rolled up, and with a letter of introduction to the great critic Saint-Marc Girardin, which a professor in the Faculty of Letters at Lyons had given him. It was in 1834, and he was just about one and twenty years old.

Girardin, then Professor at the Sorbonne, received the young student kindly, and conscientiously read his manuscript. He saw (as indeed one may see now, for the drama was published in a fine edition after Bernard's death in 1886, by the Librairie Dentu of Paris), that the drama showed that the author possessed literary powers of no mean kind; but he shrank from giving to the aspirant the hopes which might have been his due. Instead of encouraging him to devote himself to literature he bade him turn to something else by which to earn his bread, and court the Muses in his leisure moments only. "You have studied pharmacy," said he; "study medicine, you will thereby much more surely gain a livelihood."

Bernard followed the advice, and threw himself heart and soul into medical studies, living in the most frugal manner, supporting himself and paying the necessary fees chiefly with the scant money which he earned by giving lessons; for his family could spare him very little help, at most some trifling sums of money and an occasional hamper of country produce. Moreover his father died about this time, having met with reverses before his death. But a student's life in the Quartier Latin, at that time at all events, was not a costly affair. A garret, shared perhaps by a comrade, served as bedroom and study, and at times as kitchen, for when a gift came from the country home it was cooked and eaten in the "appartement," with the help of utensils impressed or borrowed from the laboratory.

Bernard worked hard at all his medical studies, and in the tumult of the new ideas crowding in upon him his old literary aspirations soon grew faint and vanished.

Most especially did he devote himself to anatomy and physiology. In the former he recognised a branch of science, limited it is true in range, but one which had been studied with such rigour and exactitude as to have become a mental discipline of no mean value. He soon made himself master of the subject, acquiring a knowledge of it at once extensive and minute, while his conspicuous manual dexterity led to the dissections which he made being regarded by others as anatomical preparations of singular completeness and value. Moreover, he seems in these early days to have looked forward to the career of a surgeon as the one by which he might hope to gain a livelihood; and to such a career anatomy was the most direct path. Physiology was at that time in a very different condition from anatomy; in place of light there was for the most part

ırkness, and in place of clear and distinct
ıidance, uncertainty and dubious discus-
on. What Bernard thought of the most
the teaching of physiology of the time,
e can gather from his utterances later
ı; and we can confidently suppose
ıat even in his early days he clearly
istinguished between the science as it was
ught and the science as it ought to be
ught. If anatomy served to supply his
tellectual appetite for exact and minute
nowledge in the shape of concrete facts,
ıysiology served to awaken in his mind
ıe desire to solve problems by a direct
perimental appeal to nature. He lived
a student in the Quartier Latin in a
ttle "entresol" in the old "Passage du
ommerce Saint André des Arts," not
r from the spot where Marat printed
L'Ami du Peuple," from the house in
hich Danton lived, and from that in which

Guillotin tried the value of his famous invention on the necks of sheep. In that little apartment he made his first attempts at experiments on living animals. A little later on he, in partnership with a fellow student, Lasègue, opened a humble experimental laboratory in the old Rue Saint-Jacques, nearly opposite the Collége de France, his purpose in doing so being, apparently, partly to gain some money by fees, but partly, and perhaps chiefly, to obtain ampler and more convenient means for carrying on his own budding researches. Alas, the enterprise was not successful. Only some half-dozen students availed themselves of the opportunity offered. The fees never brought enough to pay the rent and the cost of the rabbits employed, and the little laboratory was soon closed.

Nor in his more strictly professional

ffforts did Bernard at first attract atten-ion. Retiring, thoughtful, with his nind already bent on problems to be olved, awkward in manner, and wholly emoved from the desire so common to others to appear better than he really was, ie did not impress either his comrades or he authorities with his power.

To the former, indeed, except in the lissecting-room, he appeared somewhat idle nd inattentive. They were for the most part unable to criticise their teachers. Their intellectual activity was chiefly eceptive; it rarely went beyond the task of carefully listening to the words which ell from the professorial chair, and reasuring them in their memories to be eproduced at the appointed time in the examination room. He, on the contrary, eems to have begun early to ponder over he questions which were treated of in the lectures; he soon detected flaws in his teachers' reasoning, and thus speedily beginning to doubt the value of the teaching, paid but a listless attention to expositions and discussions, the weakness of which became more and more clear to him as time went on. Such a spirit, though it may bear admirable fruit in the end, is not very profitable at the schools. Possibly, moreover, the young student, quiet and reserved as he seemed, had within him not a little intellectual pride, which tempted him to neglect beyond measure instruction which seemed to him of dubious value. Though in his examinations and otherwise he showed himself to be an able student, neither to his fellow students nor to his teachers did he seem one who was about to make a great mark.

In the fifth year of his studies, however, his opportunity came to him. After serving as usual as "externe" at the Hospitals he was in 1839 appointed "interne"; and Providence ruled that he should be allotted to Magendie, who was then one of the Physicians to the Hôtel Dieu.

Magendie, then the leading physiologist in France, held two offices. He was Physician to the Hôtel Dieu, and Professor of Medicine at the Collége de France.

The latter famous institution was founded by Francis I. in 1530, the year after the Treaty of Cambray, under the name of the *Collége Royal* or *Collége des trois langues*, the first chairs established being those of Greek, Hebrew, and Latin. It was founded as a place of free learning with the desire to antagonise the more rigid and scholastic teaching of the University in the Sorbonne. The Royal Readers or Professors paid directly by the King were free to teach as they pleased within the subjects defined by their titles. The lectures were open to all without fees, and the whole object of the foundation was to encourage liberal and higher learning. At first the College had no fixed abode, the Professors delivering their lectures where they could; indeed it was not until 1610 that Louis XIII., carrying out the promise made by Henry IV., began the construction of a building which, however, through various delays, was not completed and hence not used until 1636.

To the chairs of the three tongues, Francis I. added those of mathematics, philosophy, and medicine, the first holder of that of medicine being the Florentine Guido Guidi, known as Vidus Vidius, who entered into the post in 1542. He was succeeded in 1550 by the great anatomist Jacques du Bois, better known by his Latin name of Jacobus Sylvius, one Beauvais intervening for three years.

Various other chairs were subsequently founded, and in the course of time numerous changes took place. Charles IX. founded in 1568 a chair of medicine, Henry III. founded one of surgery in 1575, and Henry IV. in 1595 one of anatomy, botany, and pharmacy, the second holder of which was Jean Riolan. By 1680, all these four chairs had come to be all equally devoted to medicine, surgery, botany, and pharmacy.

Towards the close of the eighteenth century, however, great changes were made. The first chair of medicine became a chair of chemistry, held from 1865 onwards by the present eminent chemist Berthelot. The second chair of medicine became a chair of Natural History held by the illustrious Cuvier from 1799 to 1832.

And the chair of surgery became the one chair of Medicine held for some years by Laënnec, and filled at the time of which we are speaking by Magendie.

During all these years, however, the principle of the College remained the same. The professors were at liberty to lecture without the restraints of a syllabus having an examination as its goal, the lectures were free to all who wished to hear, and the general idea that the place was devoted to higher study still maintained its sway.

Following the old tradition, it continued to be recognised that the duty of a Professor holding a chair in the College was not to give didactic lectures suitable to an ordinary student, but rather to use the post as an instrument of research, and to expound new ideas to those who were already advanced in their studies. Thus, for example, the lectures which Magendi delivered there in 1838–40 were afterward published in the well-known work, "Phénomènes Physiques de la Vie." Carryin out the idea of research, there was allotte to the Professor of Medicine in a lowe story of the building accommodation fo research in the form of a dark unwholesom room called a laboratory. There was als provided for the Professor a *préparateu* to assist him in the experiments wit which he illustrated his lectures, or i those connected with his researches.

Bernard's first contact with Magendi was not promising. The professor, in th ward and in the laboratory at least, wa in manner abrupt, even rough and rud He took little notice of his new *intern* hardly even asking his name. After nc many days, however, the conspicuousl skilful way in which Bernard carried ou the dissections which were entrusted to him made a very marked impression on his master ; and the story goes that one day, early in their intercourse, Magendie, on a sudden, called out roughly to Bernard, busy at a dissection, "I say, you there, I take you as my *préparateur* at the Collége de France." Bernard was only too glad to accept the offer. There is some doubt as to whether he was not, during the first year, a mere voluntary assistant, but in any case, in 1841 he had become the official *préparateur*. Bernard's career as a physiologist may be said to date from then. Before proceeding any further, however, it will be desirable to form some idea of the state of physiology in Europe in general, and in France in particular at this epoch.

II

The Condition of Physiological Scienc[e] before Bernard began his Labours

WHEN the physiologist to-day knowing how dominant has bee[n] the influence of Germany on physiolog[y] in the present century, inquires what wa[s] the condition of physiology in German[y] in about the year 1840, when Bernar[d] joined Magendie, he is at once struck wit[h] the fact that the great German physiologist[s] of the present century, Ludwig, Helm-holtz, du Bois-Reymond, and Brücke had at that time not commenced thei[r] labours. All the great work which thes[e] have done was done after that date; their labours were contemporaneous with those of Bernard. The dominant physiological mind in Germany at that time was that of Johannes Müller. That great man had in his earlier years devoted much time to definite physiological inquiry, especially into problems relating to the senses; he was a great teacher of physiology, inspiring a love for the science and a true spirit of inquiry into it among his numerous pupils; and he had written a masterly text book, his "Outlines of Physiology," the influence of which not only on his own countrymen but on others has been profound.

Johannes Müller has been called a "vitalist," and in a certain sense he was one. In his "Outlines" he criticises the views of Reil "that the phenomena of life are the result, manifestation, or property of a certain combination of elements," and contends for the necessity of supposing the existence of an "organic or vital principle or force," "the action of which, however, is not independent of certain conditions." In judging Johannes Müller's scientific attitude, it must be remembered that though a physiologist he was also a morphologist, and indeed, on the whole, more the latter than the former; hence the organic principle was of more importance to him as a something determining the form of living beings than as an explanation of the phenomena presented by their activity. Moreover he was not a vitalist in the sense that he discouraged attempts to solve the problems offered by the actions of living beings by experiments based on the view that these were the outcome of physico-chemical agencies, or that he refused to admit a physico-chemical explanation when this could be shown to be adequately valid.

He was a vitalist only in the sense that he was theoretically of opinion that even when the physico-chemical analysis of vital phenomena had been pushed as far as it could, there would still remain a large residue which could not be explained through any such analysis however complete. And indeed the fact that his pupils, the great men mentioned above, were conspicuous by their efforts to solve physiological problems by such a chemico-physical analysis, shows clearly how much the master's vitalism, so far as physiology was concerned, was academic in nature, and that his influence as a teacher was strongly in the direction of guiding the physiologist to attack his problems, by the same methods, and in the same spirit, that the chemist or the physicist attacked his.

The teaching and influence in the same direction of another great man, E.

H. Weber, was even more decided and clear ; for he by the bent of his mind, no less than by fraternal ties, was not only a physiologist, but also a physicist.

The other strong men in Germany were also teaching and practising the same view. It is only necessary to mention the names of Henle, who though in the main an anatomist, was ever in search of physiological light, of Tiedemann, of Volkmann, of Vierordt, and of Bidder, to say nothing of the earlier labours of Theodore Schwann. The mark of all these was that they set about the solution of physiological problems in the same spirit in which the physicist or chemist sets about his ; to them the canons of scientific inquiry were the same for living as for non-living phenomena ; they only had recourse, and then with reluctance, to vitalistic explanations when their means of analysis proved impotent.

On the whole the dominant spirit of physiological inquiry in Germany was at that time very much what it is now. The contrast between to-day and then lies chiefly in the paucity of opportunity for, and in the scarcity of men engaged in, experimental inquiry. Physiological laboratories, such as abound now, were then almost unknown. But in this respect physiology was hardly in a worse condition than the other experimental sciences. Until Liebig began his career at Giessen, chemical laboratories as we now know them did not exist, and physical laboratories were a still later creation. Such things do not come until a call has been made for them. The call was being made for them and they soon came. The time at which Bernard began his work at Paris, was also the time for an independent development in Germany of opportunities for, and of the prosecution of, physiological research; it was then that Ludwig and the other great German physiologists began their active careers.

In England the pursuit of physiology as a distinct science had but few followers, but these for the most part were treading the experimental path. The energetic expositions by Marshall Hall of the reflex actions of the nervous system, following up and extending as they did the earlier leadings of Charles Bell, were showing that this part of physiology, apparently the farthest removed from the sciences of inorganic nature, might be successfully studied by the same experimental methods.

John Reid, by his researches on the cranial nerves, was working in a more special and narrower way towards the same end. The sagacious Sharpey was teaching and was encouraging research in a spirit identical with that which governs physiological inquiry at the present day. And in the very year 1840, up to which we have so far traced Bernard's life, William Bowman made known to the world the results of those inquiries into the structure of muscle, and was about soon to make known the results of those other inquiries into the structure of the kidney, which still remain and will always remain models of histological research so directed as to throw light on physiological problems. One man only of mark, John Goodsir, and he more a morphologist than a physiologist, was teaching views in which a mystical tendency tempted the mind away from the more commonplace paths of simple observation and experiment.

In Italy, pressed down as she was at the time by political difficulties, and bound by ecclesiastical bonds, even less was being done, though, somewhat earlier, Spallanzani and Fontana had shown what could be achieved amid such adverse influences. Matteuci and others were, it is true, carrying out valuable researches on ordinary lines; but no prominent mind was distinctly influencing general physiological thought.

Turning now to France itself we find a somewhat different condition of things. In that country, perhaps, even more than in other countries, patriotism has a tendency to manifest itself by a too exclusive attention to views put forward by native writers; and this was in some ways especially the case at the epoch with which we are dealing. The influences which, as we have just said, were becoming potent as regards physiological inquiry in Germany, and were also producing a marked effect in England, were, it would seem, felt but little in

France. The Frenchman who dealt with physiological questions was in the main influenced by his predecessors in his own land. And at that time the teachings of two men were in different ways especially powerful. The influence of the great Cuvier, who had mastered not only his subject but his opponents, was at that time supreme. He, like other morphologists, impressed with the impotence of the mechanical explanations offered as solutions of morphological problems, was led to depreciate, in like manner, any physico-chemical explanation offered as a solution of a physiological problem, and so became an ardent supporter of vitalistic views. His influence was, so to speak, from the outside ; yet it was a living influence and had only ceased to be such a few years before. Within the science of physiology itself the influence of Bichat, though it was that of a man who had long passed away, was exceedingly powerful. Bichat had laid hold of a great idea, one which rapidly spread from the land in which it was first expounded to other lands, and has in a most marked way helped to make physiology an exact science, the idea that the life of the body is the outcome of the combined and adjusted lives of the constituent tissues. By his brilliant exposition of this fruitful idea, he had opened up new ways for an exact experimental analysis of physiological phenomena. But in the detailed application of this idea, which in the flush of his youthful enthusiasm he strove to make complete and symmetric, undertaking a task the doing of which has needed, and still needs, the continued successive labours of many inquirers, he often went astray. Moreover, he so far remained under the influence of that vitalistic teaching which, in spite of what Haller had done, was still potent in the latter part of the eighteenth century, that he based his whole exposition on the idea that vital manifestations are the result of a conflict between vital forces on the one hand and physico-chemical forces on the other ; he taught that these were essentially antagonistic, and that the latter have full play only when the former vanish in death. Had he lived longer he might perhaps have freed himself from the many conceptions which lessen the value of his great work ; but he was taken away too soon, for he died at the early age of 31, just about the age at which Bernard began to publish. And, as often happens, the parts of his labours which were specious and misleading appeared the most tempting to many who followed him. These dwelt more on his view of organic sensibility and contractility which his division into organic and anim life had led him, than on his coı ception that each tissue had its own liſ and were more pleased with his epigran matic definition of life as the sum ‹ forces which resist death than with h laborious attempts to define the characte: of the several tissues.

Bichat, by dividing the vital princip and distributing it over the several tissue attributing to it different functions i different parts, in reality dealt a deathblo to the old vitalistic conceptions. But h did not live long enough to grasp th conclusion to which his labours were lead ing, and his successors for the most paı stopped also where he himself had left of

Vitalism was thus dominant in Franc especially dominant perhaps in medic doctrines. Some indeed maintained th the phenomena of living bodies could never be the subject of exact experimental inquiry. Bernard himself states that in his earlier days he heard a professor of surgery, one Gerdy, say, "When the physiologist asserts that vital phenomena remain identical under identical conditions, he proclaims an error ; such is only true of non-living bodies."

Within physiology itself a way was opened for the encouragement of experimental inquiry by the development of a modified vitalism which taught that the phenomena of living bodies might be divided into two classes : those which are the outcome of chemico-physical causes, and those which are not, and which can be attributed only to the action of a vital principle or force. The former may be studied by ordinary experimental methods ; the latter are beyond inquiry, " their causes mock alike our conceptions and ‹ curiosity." [1]

Such was the scientific attitude Magendie, who, when Bernard began medical studies, stood far above all othe as the physiologist of France. Floure the showy Perpetual Secretary of the A‹ demy of Sciences, who approached phys logy rather from the side of natural scier than from that of medicine, had, it is trı achieved a great reputation; but, ev when we have made every allowance f his work on the semicircular canals, l influence on physiology, when careful weighed, falls far below that which t fame he enjoyed during his lifetime inc cated. Longet and others were doiı praiseworthy though limited work. B Magendie was the man who, during tl

[1] Magendie, "Phén. phys. de la Vie," vol. p. 47.

first half of the century, was justly acknowledged as the physiologist of France.

And that scientific attitude of his, to which we have just referred, was indirectly the cause of his experimental activity. Having paid his tribute to vitalism by admitting that some of the phenomena of living beings were beyond the scope of experimental investigation, he felt free to throw himself, without any restraint whatever, into the research of those phenomena which he deemed open to experiment. He became in this way the apostle of physiological experiment. While in Germany, as we have just said, researches of an experimental character were relatively rare, and rarer still in England and elsewhere, Magendie from the very commencement of his career, in 1807 or thereabouts, was unceasing in experimental investigations; he did alone with his own hand in

France as much as, or more than, was being done by others in all other lands; and though, as we have said, the spirit of such a kind of inquiry was present both in Germany and elsewhere, and the workers in these lands had no need to go abroad to be taught the true method of inquiry, there can be no doubt that the influence of Magendie's example, as one who subjected every physiological question which he touched to the test of experiment, was felt far and wide, and passed to countries other than his own.

Magendie's contributions to physiological science were many and great; the exact and full proof which he brought forward of the truth which Charles Bell had divined rather than demonstrated, that the anterior and posterior roots of spinal nerves have essentially different functions, a truth which is the very foundation of the physiology of the nervous system, is enough by itself to mark him as a great physiologist; and he did much else besides. But all his lasting results, even when their fullest value is allotted to them, are incommensurate with his activity. If he did many experiments which bore adequate fruit, he did also many which were misleading and many which were useless. For his worship of the experimental method came very near to being idolatry. Repelled by the sterile discussions in which the vitalists and other doctrinaires of the day spent their intellectual activity, he was driven towards the other extreme, and arrived almost at the position of substituting experiment for thinking. So far from regarding an experiment as a thing to be had recourse to as a test, by which to determine whether a view derived from observation and meditation were true or no, he rather thought, or seemed by his practice and indeed his teaching to think, that an experiment was the first step towards getting light. He so to speak thrust his knife here and there, to see what would come of it. He indeed confessed, in a way, that this was the nature of his method. Speaking of himself he says, "Every one is fond of comparing himself to something great and grandiose, as Louis XIV. likened himself to the sun, and others have had like similes. I am more humble. I am a mere street scavenger (*chiffonnier*) of science. With my hook in my hand and my basket on my back, I go about the streets of science, collecting what I find."

Hence, when Bernard entered upon his post as assistant to Magendie, he found himself subject to two influences antagonistic the one to the other. On the one hand, he had to listen, especially in medical circles, to expositions of vitalistic views, to the depreciation of the experimental method as a false guide in inquiries concerning the phenomena of living beings. On the other hand, he was brought into daily touch with a man who scoffed at all theory and ridiculed reasoned discussions, and who, while he refused to apply the test of experiment to certain questions of physiology, exalted experiment in the regions in which he did apply it, to the almost complete exclusion of other means of inquiry.

In spite of the personal influences which Magendie must have exerted on him, the young assistant had the genius to strike out a path for himself. While recognising, as clearly as did his master, the value of experiment as the final test of all physiological views, he, on the one hand,

:posed experiment from its false throne, aking it the servant and not the master ' reasoned speculations ; and, on the other ınd, as we shall see, extended its domains, ıowing how, under proper use, it could : applied without distinction to all the henomena of life. In doing so the pupil ent far beyond the master ; that which ıe multitude of blind experiments made y the latter have left behind as a lasting ɔntribution to physiological science is but ttle compared to that which has come 'om the much fewer experiments of the ɔrmer, guided as they were in every case y previous meditation and thought.

III

Early Labours

BY his appointment as *préparateur* to Magendie Bernard was fairly started on the career of an experimental physiologist. Outside his duties at the Collége de France, he had to devote some time to the delivery of private courses of lectures in order to eke out his slender income; the rest he gave up to research, his investigations being carried on chiefly if not exclusively in some place or other which he had temporarily fitted up as a private laboratory, or in the chemical laboratory of one or other of his friends. There was not room for him, it would appear, to conduct his own private work in the laboratory of the Collége de France.

In May, 1843, he published his first communication, "Recherches anatomiques et physiologiques sur la corde du tympan, pour servir à l'histoire de l'hémiplégie faciale," [1] followed, in December of the same year by his "Thèse pour le Doctorat en médecine" having for title "Du suc gastrique et de son rôle dans la nutrition."

The work on the chorda tympani, in which we may recognise the influence of Magendie's long continued labours on the functions of nerves, is in part anatomical and in part physiological. The former part is one of the many illustrations of Bernard's exact and extensive anatomical knowledge, in acquiring which he was greatly aided by

[1] "Annales médico-psychologiques," I., 1843, p. 408.

his remarkable manual dexterity in dissection, a knowledge and dexterity which some years later was displayed in his contributions to Huette's popular work on surgical anatomy. In the physiological part he deals chiefly with the relations of the nerve to taste and hearing; and it is interesting to note that though he was destined hereafter to carry out many important researches, of which the action of the chorda tympani on the submaxillary gland was the pivot, so that this first memoir serves in a certain sense as the beginning of long series of investigations on the relations of nerves to secretion, he began by a false step. He contended that the chorda had no influence on the secretory activity of the submaxillary gland or on the contractile efforts of its duct.

The thesis on the gastric juice was still more emphatically the first of a long series of investigations ; as we shall see it was the first step in an inquiry which before long led him to the discovery of the glycogenic function of the liver. The main result made known in the thesis was that while cane sugar injected directly into the veins readily appeared in the urine, this did not occur when the cane sugar, previous to the injection, had been subjected to the influence of the gastric juice. He inferred that cane sugar as such in the blood was unsuitable for the nutrition of the tissues, and was consequently cast out, but that by the influence of gastric juice it was so modified as to become suitable, and in that condition was retained and utilised. In this simple result lay the germ of much that was to come afterwards. The paper is also interesting as containing the record of the experiment which has since become classical, of the simultaneous injection of potassium ferrocyanide, and ferrous sulphate, by which he showed that the acid of gastric juice makes its appearance on the surface and not in the depths of the gastric glands.

These two researches in a way illustrate, when put together, the main idea which governed almost the entire course of Bernard's labours. The action of the nervous system on the chemical changes which constitute the basis of nutrition, was a problem always present to his mind, and one which he attempted to solve on the one hand by experimental investigations on nerves, and on the other by direct chemical researches; he was almost always busy with the one or with the other, and happy when he was employing the two methods at the same time and in concert.

The reference to facial paralysis in his paper on the chorda, indicates also that from the outset of his career he had grasped the ew that physiology may fitly be called cperimental medicine, the results of the boratory being, with due precautions, ailable for use at the bedside.

His succeeding researches were carried it much on the same line as the first two. Ie investigated the spinal accessory nerve, id he continued his studies in digestion, id other problems of chemical physiology, nding in for his thesis at the "Concours our l'agrégation" a memoir on the colourg matters present in the human body.

No very remarkable result was obtained any of the above researches, but in the ar 1846, in the course of an investigation the differences in digestion and nutrition tween herbivora and carnivora, an instigation which he carried on, as he did any of his earlier chemical researches, in mpany with his friend Barreswil, he made observation which proved to be the starting-point of his first really important discovery.

"We had observed," says he,[1] "that when we introduced fat into the stomach of a rabbit, the fat passing on from the stomach was not modified until it had reached a certain distance from the stomach at a point much lower than that at which a like change takes place in the case of dogs. The same difference manifested itself in the absorption of the fat by the lacteals. We saw that these in the rabbit first became white and opaque through the presence of fat at a considerable distance from the pylorus, whereas in dogs the change was visible at the very commencement of the duodenum. This difference between dogs and rabbits as to the place at which the modification and

[1] Leçons de Physiologie Expérimentale. Cours. 1855, II., p. 178.

absorption of fat begins to take pl
having been confirmed by repeated obs
vation, it was natural to look for t
cause in some special disposition of t
intestines; and now we noted that t
difference coincided with a difference
the entrance of the pancreatic duct into t
intestine. In the dog the pancreatic ju
is discharged into the intestine quite cl
to the pylorus, whereas in the rabbit t
principal pancreatic duct opens into t
intestine at a point thirty or thirty-fi
centimetres below the opening of the bilia
duct. It was precisely at this point th
the change in the fat began to take pla
and that the lacteals were able to abso
it."

This observation was the starting-poi
of Bernard's remarkable researches on t
properties and uses of the pancreas. A
it may be noted that in this, as inde

in some other instances, Bernard was led to an important truth by an observation not wholly accurate. It was pointed out afterwards, and has become generally accepted, that this disposition of the fat and the lacteal contents in the rabbit is not invariable, indeed, that the appearances described by Bernard are seen not always, but only when the abdomen is opened at a particular time after the taking of the fat. Nevertheless, the observation did start Bernard on a fruitful inquiry into the action of the pancreatic juice, and with that instinct of genius which was one of his marked characteristics, he deserted the search into the differences between herbivora and carnivora for the new line of investigation which the observation in question suggested.

The new research was not, however, completed for some little time. The first notice of the results obtained were made known to the Société Philomathique in April, 1848, and a somewhat fuller account was presented to the Société de Biologie in February, 1849. Yet it was not until 1856 that he published the complete "Mémoire sur le pancreas et sur le rôle du suc pancréatique," which appeared as a supplement to the "Comptes Rendus" of the Académie des Sciences for that year.

At the time when Bernard took the matter in hand our knowledge of the action of the pancreatic juice, and indeed of intestinal digestion was of the scantiest. In Johannes Müller's great work, pages are devoted to gastric digestion, Beaumont's observations on Alexis St. Martin, the Canadian with the accidental gastric fistula, being dwelt upon at length; indeed, the changes undergone by the food in the stomach are treated as if they were almost identical with digestion as a whole. A good deal is said it is true about the bile; but the pancreas is passed over almost in silence. "The excellent researches of Tiedemann and Gmelin" are quoted as containing "all that we know with certainty relative to the changes which the chyme undergoes in the intestine;" and all that these authors have to suggest is the possibility "that the casein of the pancreatic secretion containing a large proportion of nitrogen yields a portion of this element to different ingredients of the alimentary substances which contain less nitrogen, so as to reduce itself to their standard in this respect, and to convert them into albumen."

Let this vague conception be compared with the knowledge which we at present have of the several distinct actions of the pancreatic juice, and of the predominant importance of this fluid not only in intestinal digestion, but in digestion as a whole, and it will be at once seen what a great advance has taken place in this matter since the early forties. That advance we owe in the main to Bernard. Valentin, it is true, had in 1844 not only inferred that the pancreatic juice had an action on starch, but confirmed his view by actual experiment with the juice expressed from the gland; and Eberle had suggested that the juice had some action on fat; but Bernard at one stroke made clear its threefold action. He showed that it, on the one hand, emulsified, and, on the other hand, split up into fatty acids and glycerine, the neutral fats discharged from the stomach into the duodenum; he clearly proved that it had a powerful action on starch, converting it into sugar; and, lastly, he laid bare its remarkable action on proteid matters.

Pointing out that the bile precipitates the products of the gastric digestion of proteid matters, and puts an end to peptic changes, he went on to show that the pancreatic juice acted subsequently as well on these precipitated matters as on those proteid constituents of a meal which had escaped solution in the stomach. "It is, in fact, the pancreatic juice which has the special property of completely dissolving these two kinds of material, for the digestion of nitrogenous substances is far from being completed in the stomach, though this is the accepted view. Two acts, perfectly distinct, take place in the stomach, and in the intestine, the one duly following the other. Gastric digestion is only a preparatory act." In carrying out the observations which supply the proof of this energetic and multiple action of the pancreatic juice, Bernard took a hint from Blondlot, who in

1843 had introduced the method of the artificial gastric fistula. He not only himself early and repeatedly used the gastric fistula, but, extending the method, brought into use the pancreatic fistula. Regnier de Graaf had, it is true, so early as 1662, succeeded in making some sort of pancreatic fistula, but in a very imperfect and fruitless manner. It was Bernard who made the operation really practicable and useful.

When we realise how deeply our present knowledge of the varied and powerful action of pancreatic juice has affected our present conceptions, not only of the digestive act, but also of the processes of nutrition, and when we remember that, making all allowance for the researches, subsequent to those of Bernard, of Corvisait, and especially of Kühne, on the proteolytic action of the juice, not only the foundation, but even the larger part of the whole edifice of that knowledge, is to be found in Bernard's memoir, we may be well prepared to commend the action of the Académie des Sciences, when in 1850 it awarded to the researches embodied in that memoir the prize of Experimental Physiology.

By the publication of that memoir, moreover, not only France, but men of science in all lands, were made aware that a young physiological inquirer of striking powers had arisen in Paris. Yet the merits of the research on the pancreas were soon to be eclipsed by results of a still higher order, and of far more commanding influence, reached by the efforts of the same brilliant investigator.

Before we leave these earlier labours, however, one more research must be mentioned, a research in which Bernard came to the aid of his master, Magendie. In supplying in 1822 the direct and complete proof of C. Bell's views as to the separate functions of the anterior and posterior roots of spinal nerves, and in later experiments of the same kind in 1829 and 1839, Magendie had sometimes found the anterior roots sensitive. He, however (or Longet, for this observer claimed the merit of the observation), also observed that this sensibility of the anterior root was in some way dependent on the posterior root, was a sensibility imparted to the anterior root and not inherent, like the sensibility of the posterior root in the root itself; and the sensibility in question accordingly received the name of "recurrent sensibility." The phenomena, however, were exceedingly inconstant; sometimes the anterior root was found to be sensitive, but more often the results of a search for sensibility were negative. So uncertain was the matter that Longet was ultimately led to deny altogether the existence of any such thing as recurrent sensibility.

Bernard, attending Magendie's course in 1839, and witnessing the inconstant results of the experiments on this recurrent sensibility, concluded, with the quick insight of a true inquirer, that the inconstancy must be due to a want of knowledge of the conditions under which the experiment ought to be conducted. And he set about to determine what those conditions were. A few years later he had laid hold of the conditions, and in 1847 he published two papers[1] in which, by showing what circumstances favoured and what hindered the development of

[1] "Soc. Philom.," 1847, p. 79, and "Comptes Rendus," xxv. pp. 104, 106, 1847.

recurrent sensibility, he rescued his master's discovery from the disrepute into which it had fallen. The experiments and arguments briefly summarised in the two papers just spoken of are more fully set forth in the lectures on the nervous system which Bernard published many years later. The question of recurrent sensibility does not possess to-day the importance which it seemed to have then; the researches of Bernard in relation to it are worthy however of special note, since they bear the marks of the same power to solve an experimental problem, which later on brought in such rich results.

IV

Glycogen

IMPORTANT as was Bernard's discovery of the action of the pancreatic juice, he soon came upon a far greater one. And the story of how he came upon it is worth telling in detail, since it illustrates in a striking manner how an alert, inquiring mind, seizing at once upon the hints which Nature gave, was led into a wholly new path, and, cautiously advancing step by step, as the way opened up, was enabled, by almost its own unassisted labours, to give to the world a new truth, not as a mere rough conception, but as a highly finished work.

At the time when Bernard began his physiological studies, the views which the great Dumas had brilliantly expounded in the "Essai sur la Statique Chimique des Êtres Organisés," written by himself and the agronomic chemist Boussingault, may be said to have been dominant among biologists. According to those views, animals and plants presented a complete chemical contrast, the one to the other. The chemical token of the plant was that out of the elements existing in the inorganic world it built up the complex organic compounds, the carbohydrates, the fats, the proteids, and the like, which formed the chemical basis of its body. The chemical token of the animal was that, by feeding, it received these ready-made organic compounds into its body, and destroyed them, resolving them again into inorganic constituents, and utilising that resolution for the needs of its life. The animal might modify the vegetable compounds and give them an animal character; but it never made anything anew. Matter passed through a cycle rising up through the constructive labours of the plant into the organic compounds of the living body, and falling back again by the destructive labours of the animal into the inorganic compounds of the world which was not alive. The contrast was held to be complete; it was asserted that the animal body never built up, never manufactured, either fat, or carbohydrate, or proteid; all of any of these present in the animal body had been brought to it in its food.

These were the dominant views, though the voice of heresy had already made itself heard. In the spring of 1843, the year in which Bernard published his first papers, the calm of the Académie des Sciences was broken by a lively discussion. Payen, a chemist who was an authority on the chemistry of food, communicated a paper by Dumas, Boussingault, and himself,[1] in which it was insisted that the fat already present in the fodder was, as shown by careful analysis, sufficient to supply the fat found in the body of the fattened beast. Liebig intervened in the discussion by a letter in which he quoted new experiments by Gundlach at Giessen, confirming Huber's old observations of 1780, that bees fed on sugar alone formed wax, and disproving the criticism that the bees had furnished the wax out of the previous store of fat in their own bodies. He further showed that

[1] "Recherches sur l'engraissement des bestiaux et la formation du lait."

ıe fat accumulated in the bodies of fattened ;ese far exceeded the supply furnished by ıe fat of their food, and while twitting ˡumas for refusing to believe that starch : sugar could be changed into fat, while ; assumed that mere waxy material could ; so changed, though the change was, om a chemical laboratory point of view, most as difficult, clenched his argument y showing that when a cow was fattened, ıe excreta during the fattening period ›ntained as much fat as the food taken. ˡumas and his friends remained apparently ıconvinced, though, by an irony of fate, the very next year Dumas announced › the Academy that observations con- ıcted by Milne-Edwards and himself ı bees fully confirmed the validity of [uber's old argument.

It was while men's opinions on the ıemistry of nutrition were in this con-tion that Bernard took up a research ı the physiology of sugar. His second ıblished paper, his thesis for the doc- ırate, on the action of the gastric juice, as the initial step in this research. His :tive mind, in turning from poetry to :ience, had lost nothing of its early nbition. The task which he had proposed ı himself was no less a one than to trace ıt the successive transformations which ıe food stuffs, the dominant substances f food, underwent within the animal body. Iis first result, embodied in his thesis, 'as that cane sugar was acted upon by astric juice, and underwent through the in- uence of that fluid its first transformation, amely, a change into dextrose (glucose) ; a necessary preparation for its being tilised by the tissues.

He had intended to study all the three reat classes of food stuffs, carbohydrates, ts and proteids; but he began with the 'st, and found the study of these so expan- ve that he never got beyond them. He :gan with sugars, partly because they ere the more simple, and partly because : had become early fascinated with the :oblems suggested by the disease diabetes; : was anxious to explain the cause of this ccess of sugar in diabetes, and so by good)rtune to find a remedy for it.

The plan of research which he marked ut for himself was somewhat as follows. 'iedemann and Gmelin had shown that ı the alimentary canal starch is converted ıto dextrose before being absorbed; he ad himself shown, as we have just said, ıat cane-sugar—sugar of the first species, ; he called it—is also converted into extrose, into sugar of the second species. ıll carbohydrates, then, may be considered ; passing into the blood as dextrose.

What becomes of this dextrose? What fate, what transformation, awaits it? Bernard proposed to himself to track out diligently the dextrose, introduced into the body from the alimentary canal, along the portal vein to the liver, from the liver through the right heart to the lungs, and then from the lungs through the left heart to the several tissues of the body. "At one or other of these stations I shall find," he said to himself, "that the dextrose disappears, is destroyed, or is in some way or other changed. If, having found the station of destruction, I am able to suppress the activity of this station, sugar will accumulate in the blood, and a condition of diabetes will be brought about. (He had already satisfied himself that the essence of diabetes was an excess of sugar in the blood.) If I can thus artificially produce diabetes, the way will be opened for the scovery of curative means." All this : tells us himself[1]; the great discovery : was about to make was no haphazard ve in Nature's full pocket; it was the :ward of a carefully planned enterprise. nd the crown fell into his hands in iis wise.

He fed a dog on a diet rich in sugar and igar-furnishing material, and, killing it at ie height of digestion, examined the blood aving the liver by the hepatic veins, to see if there were any destruction of igar in the liver. It is perhaps worthy notice as illustrating how "there is a me for everything," how many things, me of them little, contribute to a result, at the search for sugar in the tissues and iids of the animal body, to which Bernard d set himself, was just at that time ndered much easier by Bernard's friend

[1] "Nouvelle fonction du foie."

ıd fellow worker, the chemist Barreswil, ıving introduced the cupric sulphate test r dextrose, the test which in a slightly odified form we now use a Fehling's st. Bernard found abundant sugar in ıe blood of the hepatic veins. The ver, therefore, was not the sought for at of the disappearance of dextrose. But," said Bernard to himself, "how ɔ I know that the sugar which I thus ıd in the hepatic vein, is the same ıgar as that which I introduced into the ɔrtal blood through the food?" He :cordingly fed another dog on meat only, ı sheep's head, having previously satisfied mself that, under these circumstances ɔ dextrose was present either in the imentary canal or the portal blood, ıd again examined the blood of the ɛpatic vein.

To his great astonishment he found that

this case also the blood of the hepatic :in was loaded with dextrose.

Here came in the genius of the true .quirer. "Why!" said he, "if I have .ade no mistakes, I have in this experi-.ent come upon the production of sugar: ıe liver produces sugar. I need not bour at the long task which I had .arked out for myself, of searching for ıe seat of the destruction, the dis-)pearance of sugar, so that by suppressing ıat I might indirectly bring about the :cumulation of sugar. If the result hich I have just got is confirmed on :petition of the experiment, the liver is sugar-producing tissue, it manufactures ıgar out of something which is not sugar, ıd within it lies the secret of diabetes. urther, Dumas is wrong in saying that ıimals do not construct, that the liver does)t construct; the liver does construct, constructs dextrose. This is a big thing f which I have got hold. I must make ire that there is no mistake in the experi- ient, and then push forward as far as ɔssible the lead thus given me."

He set about to test his result in every ɔssible way. He took dogs which had een starved, and dogs which had been fed r some time on meat alone, and found both cases that sugar, while absent from ie alimentary canal and from the chyle, as present in the blood of the right heart id in the blood of the portal vein close the liver. He starved a dog for several ıys in order to get rid of the effects of a evious mixed diet, then fed it on meat, id found sugar as before in the hepatic in and right heart. Opening the body a dog killed in full digestion he placed gatures on the mesenteric veins at some tle distance from the intestine, on the eins from the pancreas, on the splenic ein, and on the vena portæ near the liver ; e found no sugar in the blood of the esenteric veins between the intestine and ie ligature, none in the blood of the ancreatic or splenic veins, but there was igar in the portal blood between the gature and the liver. Obviously, the igar in the latter case had regurgitated om the liver ; it was clear that the liver, id not the spleen or the pancreas, ny more than the food in the intestine, as the source of the sugar. And he und that a simple decoction of the liver ibstance invariably contained sugar. astly, he determined that the sugar in iestion was dextrose, was a sugar capable fermentation, and giving all the dinary tests for dextrose.

He now felt justified in making known the world that the liver was capable of 'oducing sugar not brought to it as sugar the food, that sugar made its appearance the liver itself by an act which seemed :ry analogous to the act of secretion by a creting gland, and which therefore might : spoken of as an internal secretion. On .e 28th of August, he deposited a sealed ıcket with the Académie des Sciences, ıd on the 15th of November following, : and Barreswil exhibited at the Académie specimen of alcohol obtained by fermention from sugar of the liver, their efforts crystallise the sugar having so far been ısuccessful. In 1849 he laid before the ›ciété de Biologie a fuller account of his searches,[1] and again in 1850 before the cadémie des Sciences.[2]

Continuing his researches, examining the

[1] "Mém. Soc. Biol.," 1849, pp. 121–133.

[2] "Compt. Rend.," xxi. p. 571; 1850, pp. 1–574.

vers of many different kinds of vertebrate
ıimals, and indeed of invertebrate
ıimals also, he found full confirmation
: his view that the sugar of the liver is
ɔt supplied directly from the food, but is
ırnished by the liver itself, through a
.echanism analogous to that of secretion,
the expense of elements of the blood
hich traverse the hepatic tissue. He
.rly recognised, however, that this
:patic sugar, though it did not come
rect from the food, was influenced as
gards its quantity by the nature of the
ıod. Thus he observed that it was much
minished, even to disappearance, by long
arvation, that it was very little if at
l increased by fatty food, but was very
.arkedly increased by gelatine or by carbo-
ydrates. This influence of food and other
fluences such as that of age, as well as
e action of the nervous system, he

more fully expounded in the thesis which he maintained for his Doctorate in Science, on March 17, 1853, and which was published as a monograph in the same year.

In the course of these researches he had come upon the remarkable fact that puncture of the fourth ventricle causes temporary diabetes. The first record of this is a note communicated to the Société de Biologie,[1] in which the author stated that he had previously shown the same effect in rabbits; no published account, however, of this appears. The same fact was also announced in a brief note to the Académie des Sciences.[2]

It is interesting to note the way in which Bernard was led to this striking result.

[1] "Compt. Rend. Soc. Biol.," 1849, p. 60 (April), "Chiens rendus diabétiques."

[2] "Compt. Rend.," xxviii. p. 393.

t first sight it has the appearance of :ing an accidental result ; and the fact at so early as February 3, 1849,[1] ernard had stated that section of the rebellar peduncles led to the appearance ' albumin and sugar in the urine, lends certain amount of support to this idea. ut, according to Bernard himself, he was d to it by a process of reasoning.

Regarding, as he had come to do, the ›pearance of the sugar as a secretion— ı internal secretion of the liver—he gued that this secretion, like other cretions, would be subject to the in- ıence of an appropriate nerve. Anato- ical considerations, as well as earlier ›servations on the vagus, and its relations digestion,[2] led him to suppose that .e nerve in question could be none

" Compt. Rend. de la Soc. Biol.," 1849, p. 14.
" Compt. Rend.," xviii., 1844, p. 995, &c.

other than the vagus nerve. And he was confirmed in this view by the fact which he had early ascertained that section of the vagus nerves did away with the formation of the sugar. He accordingly expected to find that galvanic stimulation of the vagus trunks would lead to an increase of the hepatic sugar; but in this he was grievously disappointed; all his results were negative. Remembering, however, some older experiments of his on the fifth pair, in which he had produced secretory effects—tears and saliva—by irritating the nerve in a special way, namely, by puncturing it at its origin in the brain, he conceived the idea of applying the same method to the vagus at its origin in the floor of the fourth ventricle. The experiment succeeded, the sugar-producing or glycogenic function of the liver was thereby highly excited; so much sugar as poured so rapidly into the blood ream that it could not be disposed of, id, like an excess of sugar in the blood ·oduced by artificial injection, made its ay into the urine. Bernard himself was ıe of the first to recognise that the ıeoretical view which led him to this :markable result was founded on error, ıat the vagus is not the channel by which ıe influences, started by the puncture of ıe fourth ventricle, whatever be their ıture, reach and affect the liver, that the ıgus is not the secretory nerve governing ıe secretion of hepatic sugar. Never- ıeless the wrong view led him to an ıportant truth ; and indeed it was one ˜ Bernard's characteristics that his experi- ental search after new facts was never haphazard prodding into unknown ·ound ; he was always guided by some ·econceived theory, sometimes right but more often perhaps wrong. No less a characteristic, and this was perhaps the one which led him so far, was his readiness to fasten on to the new fact, and to consider it by itself, regardless of the theory which had led him to it. In all his writings he insists on the value of imagination and preconceived theory in experimental research; but he knew how to use them and when to throw them on one side. He used to say to those who were working with them, "Put off your imagination as you take off your overcoat when you enter the laboratory; but put it on again, as you do the overcoat, when you leave the laboratory. Before the experiment and between whiles let your imagination wrap you round; put it right away from yourself during the experiment itself, lest it hinder your observing power."

ıese continued observations, con-
ıs they speedily were by investi-
lsewhere, notably by Lehmann in
though criticised and controverted
were by others, more especially
in France itself, Bernard had
ed the glycogenic function of the
e had proved that the liver pro-
ıgar by means of a mechanism
ıs to that of secretion at the
of the elements of the blood
ıg the hepatic tissue.

e did not stop here.

ɔon came to the conclusion that
.r was not formed at one step out
:lements, whatever they might be,
ıe blood brought to the liver, but
sugar came from some substance
ın the hepatic tissue, some substance
of being converted into sugar,
glycogenic substance." He was

led to this conclusion in the following way.

Taking a liver fresh from the body, he sent a stream of water through it until the wash-water issuing by the hepatic vein contained neither albumin nor sugar. He washed out of the liver all the sugar previously present in it, and a decoction of the liver so washed out contained no sugar. But if the liver thus washed out and sugarless were allowed to remain, especially in a warm place, for some time, say for a few hours, a subsequent stream sent through the vessels was once more loaded with sugar, as was also a decoction of the liver substance. The sugar had been washed out by the first washing, but not the glycogenic substance, and this latter had subsequently given rise to fresh sugar.

He next found that the conversion of

is glycogenic substance into actual sugar as arrested by the hepatic tissue being ıbjected to the temperature of boiling ater. Aware of the profound changes hich proteid matter undergoes when ıbjected to the above temperature, his st idea was that his "glycogenic substance" was of a proteid nature, and that ; conversion into sugar was prevented by ıe changes of composition induced in it / the high temperature. But he was on set right by the observation that decoction of boiled liver, though of self it remained unchanged, producing) sugar, readily gave rise to sugar when small quantity of an infusion of fresh, ıboiled liver was added to it. He saw once that the conversion of his glygenic substance was brought about by kind of fermentation, that the glycoenic substance itself was of the nature

starch. By straining the liver de-
)ction free from blood-vessels and con-
:ctive tissue elements, he was, by
ıbsequent washing with alcohol and
her, able to prepare, in the form of a
:y powder, a glycogenic substance, which,
)t itself giving the tests for dextrose,
as readily converted by fermentation
to dextrose. These important results
: communicated to the Académie des
:iences on Sept. 24, 1855.[1]

His powdered liver, however, was still
very impure substance, and it was not
ıtil March 23, 1857,[2] that he could
:scribe the complete isolation by the
)w well-known potash-alcohol process,
ıd the definite characters of the sub-
ance which he now felt justified in

[1] "Compt. Rend.," xli. p. 461, "Sur le mécha-
sme de la formation du sucre dans le foie."

[2] "Compt. Rend.," xliv. p. 578, "Sur le méchan-
ne," &c., (suite).

lling *glycogen*. He obtained this in a fficiently pure form to enable Pelouze, the result of an elementary analysis, to sert its carbohydrate nature. He had, rlier in the year,[1] given some account ' these results to the Société de Biologie; d it may here be mentioned that ensen[2] had independently been led to complish the isolation of glycogen.

In the above memoir Bernard describes e technique for the extraction and puri-cation of glycogen, and gives an account its reactions, including that towards dine. He adds some valuable reflections the whole subject, pointing out that hile the formation of glycogen is a vital t, that is to say, takes place only ıder conditions of life, the conversion of ycogen into dextrose, by a process of

[1] "Mém. Soc. Biol.," 1857, pp. 1–7.

[2] "Verhl. d. Phys. Med. Gesell. Würzburg," l. viii., 1856, s. 219.

rmentation, is independent of life. He
ılls attention to the fact that the blood
ntains in itself a ferment capable of
nverting glycogen into dextrose, and
ggests that the nervous system, in giving
se to an increase of sugar, as in the
abetic puncture, probably acts in an
direct manner by modifying in some
ay the circulation. He further draws
interesting comparison between gly-
gen and sugar in the liver, and starch
d sugar in a germinating seed; in
e one glycogen, in the other starch, is
rmed and deposited in the cell by virtue
the living activities of the tissue, in
th the carbohydrate so formed is con-
rted into sugar by the action of a
rment.

This brings to a close the first chapter
the history of glycogen. It was
rnard's good fortune not only to have

ɟun but to have completed the dis-
ʳery. Though the whole investigation
ɔk several years to accomplish, though
m the very outset the matter ex-
ed great interest throughout the whole
entific world, and many other hands
re put to the work, it was Bernard
nself who, following steadfastly the lead
ʳen by his initial observation, through
cessive steps, each new one reached by
als based on sound reasoning suggested
its forerunner, arrived at the final goal.
ıough he never shrank from making
own each new result as he came upon it,
had not the mortification, which some-
ıes falls to a pioneer, of seeing his leading
ıceptions realised by experimental proof
the hands of others before he himself has
l time to furnish the decisive evidence.
ıe whole story lies in Bernard's own
itings. To the account which he gives

his own researches, the rest of contem-
ɔrary literature on the subject, whether
ꜫ consider the corroborative writings of
ehmann and others, or the opposing views
fered by Figuier, Sanson, and others,
ɔpears as a mere unimportant fringe. Nor
n much importance be attached to the
ere fact that Hensen was prior to Bernard
publishing an account of the isolation of
ycogen, since Bernard had practically
fected the isolation some time before.
ernard started the fox, was at the head of
e pack through the whole run, and was
st in at the death.

Every discovery in physiology of any
arked magnitude has a double bearing.
n the one hand, it is a link in a chain, or
ther a network, of special problems; it
rves as a starting-point of new inquiries,
id fills up gaps in, or it may be supplies
ɔrrections to, old ones. On the other hand,

influences more or less deeply, according
ı its nature, general physiological concep-
ɔns. Bernard's discovery of the glyco-
:nic function of the liver was powerful in
ɔth these directions. As a mere contribu-
on to the history of sugar within the
ıimal body, as a link in the chain of
ɔecial problems connected with digestion
ıd nutrition, its value was very great.
ven greater, perhaps, was its effect as a
ɔntribution to general views.

The view that the animal body, in con-
ast to the plant, could not construct,
ɔuld only destroy, was, as we have seen,
ready being shaken. But evidence, how-
ver strong, offered in the form of statis-
cal calculations, of numerical comparisons
etween income and output, failed to pro-
uce anything like the conviction which
as brought home to every one by the
emonstration that a substance was actually

ırmed within the animal body and by the :hibition of the substance so formed.

No less revolutionary was the demon-ration that the liver had other things to ɔ in the animal economy besides secreting le. This, at one blow, destroyed the then ɔminant conception that the animal body as to be regarded as a bundle of organs, .ch with its appropriate function, a con-:ption which did much to narrow inquiry, nce when a suitable function had once :en assigned to an organ there seemed no :ed for further investigation. Physiology, :pounded as it often was at that time, in .e light of such a conception, was apt to ave in the mind of the hearer the view ıat what remained to be done consisted ıiefly in determining the use of organs ıch as the spleen, to which as yet no defi-te function had been allotted. The dis-ıvery of the glycogenic function of the ver struck a heavy blow at the whole ıeory of functions.

No less pregnant of future discoveries as the idea suggested by this newly found ıt action of the hepatic tissue, the idea ıppily formulated by Bernard as "internal :cretion." No part of physiology is at the ·esent day being more fruitfully studied ıan that which deals with the changes hich the blood undergoes as it sweeps ırough the several tissues, changes by the ıreful adaptation of which what we call ıe health of the body is secured, changes ıe failure or discordance of which entails sease. The study of these internal secre-ɔns constitutes a path of inquiry which ıs already been trod with conspicuous ccess, and which promises to lead to un-ld discoveries of the greatest moment ; e gate to this path was opened by Ber-rd's work.

With the demonstration of the actual bstance the first chapter of the story of ycogen is, as we have said, closed. By it e mode of inquiry was profoundly changed ıd a new chapter begun. The search for dications of the appearance of sugar was placed for a search for the substance ycogen. And Bernard himself was the ·st to contribute to the new chapter. He ıd quite early in his investigation come :ross and appreciated the importance of .e fact that sugar is formed in the liver ˙ the embryo after a certain stage of intra- :erine life and that sugar is present in the ımiotic and allantoic fluids. He was now 'med with two new methods of inquiry. ı the first place, he could quantitatively :termine the amount of glycogen present this or that tissue under these or those rcumstances ; this gave a precision to his :sults which could never be gained by a ere estimation of the production of sugar. . the second place, he early recognised that e colour reaction of glycogen towards dine, the port-wine colour which glycogen ıowed when treated with iodine under vourable circumstances, enabled him to udy the ways of glycogen not only by ıemical but also by histological investiga- ɔn, the one method of investigation con- ·ming or checking the other.

In purely histological inquiries Bernard as not "at home"; but in the micro- opical search after glycogen he was able › avail himself of the skilful help of a ɔung German then studying under him, ıe who was already becoming known / a remarkable research in the physiology ̄ muscle, and who has since achieved a remost place among the physiologists of .e day, Willie Kühne, the distinguished ·ofessor of Heidelberg. In saying that

ernard did not seem to have the same
.cility in histological as in other physio-
gical inquiries, this must be understood
apply to the technique only. In grasp-
g the meaning of histological facts, he
owed the same quick power which
aracterised him in all his work. He,
r instance, early recognised the signi-
ance of the granules in the secreting
lls of the pancreas; and indeed Kühne,
whom, after Bernard, so much of our
owledge of the pancreas is due, gave
these granules the name of "Bernard's
anules."

The fruits of the new method he made
own to the Académie des Sciences in
59.[1] In a series of papers he gave
account of the presence of glycogen
the one hand in the maternal pla-
nta, and, on the other hand, in various

[1] "Compt. Rend.," xlviii. pp. 77, 673, 884.

ɜtal tissues, calling attention especially ı the relatively enormous quantity of ycogen present in developing striated uscles. With characteristic breadth of ew he dwells on the light which the ·esence of this carbohydrate in tissues hile they are struggling to put on their ɔpropriate structure, throws on the ıture of the processes of nutrition.

This may, perhaps, be considered as ernard's last important contribution to ιe history of glycogen. He, it is true, ɔntinued to work on the subject to ιe end of his life. In his last year, 1877, he contributed three papers ɔon it to the Académie des Sciences, ιving in the interval between that and 359 published other papers, and more pecially made known the results of fresh periments, and developed general views his lectures delivered at the Collége de rance and elsewhere. The volume of ctures on Diabetes ("Leçons sur le iabète") published in 1877 may be ›nsidered as his last testament touch-g the subject.

All these later writings, however, are ıiefly occupied in expounding or defend-g his views on the nature and purpose ˙ the glycogenic function, or in criticising e opinions on the same subject expressed ʳ others.

Not a little space in them is from time time devoted to a severe criticism on ıat he called the vitalistic view put for-ırd by some, and more especially by his :pil Pavy, teaching that the appearance of gar in the liver is a *post-mortem* phe-ımena. Bernard, as we have seen, had ‹en up a definite position in relation to .estions of so-called "vitalism"; and ever he was tempted to abandon the tone

' calm and dispassionate attitude in which
: discussed most questions, it was when
: had to deal with vitalistic theories.
[is very last paper but one, that in the
Comptes Rendus" of 1877, deals tren-
iantly with this view of the *post-mortem*
iaracter of the appearance of hepatic sugar;
id in his lectures on Diabetes he cannot
:sist the temptation to be sarcastic when
iscussing it, pointing out that according
› it "a diabetic patient is a walking
›rpse; a truly droll idea."

The reader who reads these various later
ritings in the light of the knowledge
hich we possess to-day cannot but be
ruck with the reflection that if we put
iide the discovery of share taken by the
ancreas in determining the part played by
igar in the animal body, all that has
nce been added by others to Bernard's
wn results, amounts, compared with them,

something relatively small. It has rarely llen to the lot of any one, who made the ginning of such a wholly new line of search, to carry it forward so far towards mpletion with his own hands as Bernard d the glycogenic function of the liver. he views which he left behind him in 377 have, on the whole, not been largely odified by subsequent inquiry. Much, r instance, has been done since that in termining the influence of carbohydrate od on the storage of hepatic glycogen ; it it is worthy of notice that Bernard rly recognised this, and that in spite of e fact that the corner-stone of his whole scovery consisted in the proof that sugar the hepatic vein was independent of gar in the alimentary canal.

So also in many other details the kernel what we are discussing to-day may be und in some sentence or other of

ernard's. It has been the fate of many
ther men in many matters to have merely
id a foundation on which other men have
uilt. He, in the matter of glycogen, not
nly laid the very first stone, but left a
ouse so nearly finished that other men
ave been able to add but little.

V

Vaso-motor Nerves

THE discovery of glycogen was Bernard's greatest achievement; next importance to this, and, indeed, hardly ss than it, was his discovery of the vaso-otor system. The part which he played this latter discovery, however, was very fferent from that which he played in ie former. As we have just said, he ɔt only began but carried out and, we ay almost say, completed the discovery glycogen by his own researches; the ɔntributions of others in his own time ere almost insignificant as compared with is. He, moreover, at the very outset rasped the full and almost the exact ıeaning of what he had laid hold of.

In the case of the vaso-motor nerves, was others rather than himself who rst recognised the importance of his ırlier result, the vaso-motor function of ıe cervical sympathetic. In the case of ıis, as in the case of glycogen, he was ›oking for something else when he ›und it. But, unlike his attitude in ıe glycogenic research, he did not at ıce turn aside and give himself up to e new result. It would almost seem if he did not at first see its im-›rtance, and was inclined to continue ı the line of inquiry which he had iginally laid out for himself; and, in-·ed, to this he clung to the end, though e interest which others manifested in the tercurrent vaso-motor phenomena led m, in spite almost of himself, to develop .is part of the inquiry.

Before proceeding to speak of Bernard's vestigation, it will be well perhaps to call mind what was the condition of our ıowledge at that time of the relation of .e nervous system to the blood-vessels.

The great German physiologist, Johannes Iüller, in the 1838 edition of his classical ork on Physiology, the English transla- on of which by Baily appeared in 1841 ıd 1843, recognises two kinds of muscle, .e striated muscles, the muscles of the unk and limbs, and the non-striated uscles of organic life found in the intes- ıes, the uterus, the bladder, and the iris. [e also describes the contractile "cellular" "connective" tissue, as we now call it; this the dartos of the scrotum serves his characteristic example. He dis- ıguishes this from muscle by the fact

' its yielding gelatine, whereas muscles, : says, are fibrinous. He discusses at ·eat length the question whether arteries ›ssess muscular contractility, and de-des firmly in the negative; they possess ıysical elasticity, but not muscular con-actility. He admits, however, the possi-.lity that the contraction observed in small ›ssels upon the application of cold, as in-sted upon more especially by Schwann, ıay be a manifestation of that which he, in ıe language initiated by Bichat, speaks of ; "insensible organic contractility," and hich was supposed to be the basis of the tonus" not only of the tissues of organic fe, but even of the skeletal muscles. Iüller obviously was wholly unprepared ›r vaso-motor nerves, even within a few ›ars of Bernard's discovery of them.

The fact, however, that the sympathetic ›rves were in many places traced to blood-

:ssels was leading men to suspect .at the nervous system must in some ay govern the blood-vessels. In 1840 [enle, discoursing on the physiology of sympathy," and putting to himself the iestion why do sympathetic fibres, ›parently motor in nature, go to such ructures as arteries if these, as supposed, e devoid of muscles, was led to the con- usion that the middle coat of the arteries really in part muscular in nature, though .e muscular tissue in them is of a kind mewhat different from that, not only ' the skeletal muscles, but also of such uscles as those of the intestine. And in .e same year Stilling, in a work on "Spinal ritation," had introduced the word "vaso- otor." Arguing on theoretical grounds : had come to the conclusion that there ere motor nerves not subject to the will it capable of being put into action by nsory impulses, nerves which determined e movements of the blood, and which he erefore proposed to call "vaso-motor :rves."

A little later on the whole question of the uscular nature of the blood-vessels and her allied tissues was made clear through ie discovery by Kölliker in 1846 of the ct that plain muscular tissue, whether :curring in masses or in a scattered shion, was made up of minute spindle iaped cells aggregated together.

The way was now open for the clear :oof of the existence and action of vaso-iotor nerves; but no one supplied this ıtil Bernard came upon it. And his scovery was made in this way.

He proposed for himself the study of ie influence of the nervous system on ıimal heat; and he began by attempting to certain in an exact manner how far the mperature of a part of the body was fected by the section of the nerve or erves distributed to it. Of the three inds of nerves, at that time distinguished om each other, motor, sensory, and sym-athetic, he began with the sympathetic, eing led to this choice by the consideration iat the sympathetic fibres, since they so ften accompany the blood-vessels, are robably specially connected with the iemical changes between the blood and ie tissue which determine the develop-ient of heat and so the temperature of ie part.

Accordingly, choosing the cervical sym-athetic as a sympathetic nerve easy of cess, he divided that nerve in the neck. Iolding the preconceived idea that the ifluence of the nerve, if any such existed, as in the direction of bringing about iemical changes involving the setting

ee of heat, he expected to find that the
ction of the nerve by removing that
ıfluence would lead to a lowering of
mperature. To his surprise he obtained
contrary result. When in a rabbit or
her animal he divided the cervical sym-
ıthetic on one side of the neck, the
mperature of that side of the head and
eck, instead of falling, rose, the rise being,
ıder favourable circumstances, very con-
lerable, several degrees Centigrade, and
adily appreciated even by the hand. A
milar rise, or even a more marked one,
llowed the removal of the superior
rvical ganglion on one side. At the
me time he observed an increase in the
nsibility of the side of the head operated
ı; and the title of his first communica-
on on the subject, read at the Société de
iologie in December, 1851, was "In-
ıence du grand sympathique sur la

:nsibilité et sur la calorification." A ıbsequent communication on the same ıbject to the Académie des Sciences on the 9th of March of the following year[1] bears similar title, "De l'influence du système ɜrveux grand sympathique sur la chaleur ıimale." Not a word in the title of ther paper about vascular effects. Yet in oth papers, though they are very short, ɜ describes the changes in the blood-essels. In the latter he says, "All the art of the head which becomes hot after ıe section of the nerve becomes also the ;at of a more active circulation. The :teries especially seem fuller and appear to ulsate more forcibly; this is very dis-nctly seen, in the case of the rabbit, in ıe vessels of the ear." He reserves ɔr further consideration the question whether the vascular changes are the

[1] "Compt. Rend.," xxxiv. p. 472.

cause or the effect of the rise of temperature."

Bernard published the account of this experiment as a contribution to our knowledge of animal heat; but it will ever remain as the first clear and decided experimental proof of what we now call the vaso-motor functions of the nervous system. I say "the first clear and decided proof," for not only, as we have seen, had previous observers drawn inferences, chiefly from pathological phenomena, concerning the influence of nerves on the blood-vessels, but vascular changes had been observed in connection with the cervical sympathetic nerve itself. Thus so long before as 1727 Pourfour du Petit had observed redness of the conjunctiva in the dog after section of the cervical sympathetic, and the same effect had been noticed by subsequent observers, such as

Dupuy, Brachet, and John Reid. B
attention of these and other inquire
been almost exclusively concentrat
the remarkable effects of the ce
sympathetic on the pupil; in wat
the constriction of the pupil
followed section of the cervical sy
thetic, they neglected attendant p
mena. Even in the remarkable m
by Budge and Waller, presented t
Académie des Sciences in the same y
that with which we are now dealing,
in which the pupil-constricting fibr
the cervical sympathetic are traced t
spinal cord, and in preparing whicl
authors must have repeatedly come a
the phenomena to which Bernard calls a
tion, there is nothing which can be
sidered as in any way forestalling Berr
discovery. They were looking at
pupil and saw, so to speak, nothing

Indeed Bernard himself tells us that from the very beginning of his experimental studies in 1841, he had repeatedly divided the cervical sympathetic without observing the phenomena which he saw for the first time in 1851. In these previous experiments his attention, like that of others, had been directed to the pupil; it was not until the day that he looked for changes in the face and ear that he saw them. He was, it was true, looking for animal heat, but he saw also the vascular changes, saw them and spoke of them in such a way that never afterwards could they be ignored. With his experiment and not with any of those made by his forerunners does our knowledge of the influence of the nervous system on the blood-vessels really begin.

That Bernard's observation had the significance which we are claiming for it

is indeed clear from the fact that i
diately attracted great attention
out the whole scientific world.
August of the same year, 1852, t
of it had crossed the Atlantic,
find Brown-Séquard, then sojou
America, publishing in the *Phi*
Medical Examiner of that month,
in which the following words appe

"I have found that the re
phenomena which follow the se
the cervical part of the sympatl
mere consequences of the para
therefore of the dilatation of th
vessels. The blood finding a la
than usual, arrives there in great
tity; therefore the nutrition
active. Now the sensibility is
because the vital properties of th
are augmented when their nutritic
mented. . . . I base my opinion

on the following experiments : If galvanis: is applied to the superior portion of tl sympathetic after it has been cut in tl neck, the vessels of the face and of tl ear after a certain time begin to contract their contraction increases slowly, but last it is evident that they resume the normal condition, if they are not eve smaller. Then the temperature and tl sensibility diminish in the face and in tl ear, and they become in the palsied si the same as in the sound side. When tl galvanic current ceases to act, the vesse begin to dilate again, and all the ph nomena discovered by Dr. Bernard r appear."

Brown-Séquard thus supplied what v may call the second half of the vaso-mot proof; and it will be observed that he h: none of Bernard's hesitation as to t interpretation of the phenomena. T rise of temperature as well as the inc of sensibility were to him simply the e of the greater blood supply, due t(dilatation of the vessels.

A little later, in November of the year, Bernard quite independently of apparently in ignorance of Brown-Séqu results made known [1] that galvanisin upper portion of the divided sympat produces effects diametrically oppose those of section; "the circulation being active becomes feeble, the junctiva, the nostrils, and the ears, v were red, become pale." He ther himself also supplied the second ha the vaso-motor proof.

Still a little later Waller, appar ignorant alike of both Brown-Séqu and Bernard's results, announced communication to the Académie

[1] "C. R. Soc. Biol.," 1852, p. 168.

Sciences [1] that galvanism of the cervic
sympathetic produced constriction of tl
blood-vessels of the head, and at tl
very same time Budge [2] showed that tl
fibres in the cervical sympathetic goveri
ing the blood-vessels, like the fibres fo
the pupil, took origin from the spin
cord.

Up to this time all Bernard's commun
cations on the subject had been extremel
brief, but in December 7th and 21st (
1853, he read a longer memoir before tl
Société de Biologie, [3] in which he moi
fully developed his views. It is worth
of note that though in the first sectioi
historical in nature, of this paper, he stat
that he had shown that galvanism of tl
upper end of the divided cervical syn

[1] "Compt. Rend.," 1853, xxxvi. p. 378, dat Feb., 1853.

[2] Ibid., p. 377.

[3] "Mémoires de la Soc. de Biol.," 1853, p. 77

pathetic "caused all the troubles proc
by the section of the nerve to disapp
he devotes the last and longest secti
a discussion "on the relations which
between the vascularisation and the c
fication of the parts after the division c
great sympathetic." In this section, re
mainly on the fact that when the c
lation in the ear is arrested by lig
of the two veins, the rise of temper
may still be observed upon division o
sympathetic, he argues that "the incr
warmth cannot be explained by a
tended paralysis of the arteries whic
virtue of a passive enlargement all
larger quantity of blood to circulate."
he insists on the fact, as indeed h
in his very first paper, that very
on the day after the section, thoug
vessels have returned to their n
condition, the rise of temperature

sists. "In a word," he concludes, vascular phenomenon which follows the section of the sympathetic ner active, not passive; it is of the nature as the vascular turgescence w occurs in a secreting organ on its sage from a condition of rest o feeble activity to one of great activi

Four years later, in a lecture at Collége de France in June, 1857 expounds the same views, employin times the very words of his original munications; he still maintains that vascular phenomena cannot be ref "to a paralysis pure and simple of arteries." In this lecture it may remarked he insists, as indeed he done in his earlier communicat that it is section of the sympat fibre alone which produces a ris temperature, section of the se or motor fibre giving rise tc lowering of temperature; the ris temperature which is observed after section of a mixed nerve, such as sciatic, is due to the sympathetic 1 present which have joined the n peripheral to its spinal roots.

In Bernard's mind the importanc his experiments on the sympatheti in the proof which they afforded the nervous system did act dir on the chemical changes in the tis and so intervened in the dev ment of heat; the vascular pheno he regarded as of secondary in tance. Dwelling on the fact, v by that time had become to be reg as established, that the warmth of blood is supplied to it by the dev ment of heat in the tissues through v it passes, rather than by generatic

heat in itself, and arguing that the t
of the face, and even of the ear, contr
to the blood their quota of heat
virtue of the chemical changes goin
in them, he was inclined to think
the tissue changes formed the pri
object of the nerve supply, and tha
vascular changes were rather the
than the cause of the rise of tempera

He was, however, himself soon led—
this is a marked instance of how alwa
his inquiries he conscientiously followe
teaching of his experimental facts in
of his preconceived opinions—to fu
an instance in which chemical pheno
were obviously the result of the vas
changes, and at the same time to
the second great advance in our k
ledge of the vaso-motor system.

In the very first scientific paper w
he published, that in 1841, on the ch

tympani, Bernard had been led to
that this nerve has any influence ove
secretion of the submaxillary gland.
the very same year, 1851, that Bei
had discovered the vaso-motor func
of the sympathetic, Ludwig publishe
classical paper on the secretory func
of the chorda tympani. That great
siologist did not, however, observe
at least did not describe, any of
attendant vascular phenomena.

In the January of 1858, Bernard, v
ing on the submaxillary and other gl
announced to the Académie des Scie
that when a gland is actively secreting
blood which issues from it along
veins, is not as in the case of the l
issuing from an active muscle, dar
colour, but is bright red, in fact art
In the next month, in a short co

[1] "Compt. Rend.," xlvi. p. 159.

nication to the Société de Biol(
followed by a longer one in the succe(
August to the Académie des Sciences
made known that this feature of the ve
blood of the submaxillary gland onl)
peared as a result of stimulation of
chorda tympani nerve ; when the (
nerve which supplied the gland, na
the sympathetic, was stimulated, the ve
blood issuing from the gland was (
even darker than usual. The gland i
was under the dominion of two kin(
nerves, the one giving rise on stimul
to a bright and the other to a dark ve
blood, the flow in the former case l
full and rapid, in the latter scanty
slow. He showed that the same
nomena of two antagonistic nerves r
be observed in other glands ; and he

[1] "Compt. Rend. de la Soc. d. Biol.," p. 29. [2] "Compt. Rend.," xlvii. p. 245

plied the true explanation of the phenor
He argued that the bright red ar
colour of the venous blood issuing
the gland upon stimulation of the cl
and the dark colour of the same blood
the sympathetic is stimulated could n
due to the direct action of the nerv
the blood ; " there must be interme
conditions, and these are supplied b
different mechanical modifications br
about in the capillary circulation
the two nerves respectively."
chorda tympani dilates the vessels,
brings about so rapid a circulation
the blood has not time to lose its ar
colour in passing through the capill
The sympathetic constricts the ve
impedes and slackens the flow, an
permits the gaseous exchange to be
gerated. " The sympathetic nerve i
constrictor nerve of the blood-vessels

tympanico-lingual (chorda tympani) is dilatator."

This is the first announcement, tl the statement of the discovery, of · constrictor and vaso-dilator nerves.

To Claude Bernard, then, we owe foundations of our knowledge of vaso-motor system. He made kı to us the existence of vaso-motor ne and he also made known to us vaso-motor nerves are of two k vaso-constrictor and vaso-dilator.] are the two fundamental facts of motor physiology ; all else supplie many others is built up on these.

It is also worthy of note, as indicati the spirit of the true inquirer, that Beı came upon both these truths while h in each case looking for something In his research on the sympathetic mind was fastened on the relation o

nerves to animal heat; in his researc
the submaxillary gland he was tryir
make out the differences in the colo
the venous blood according as the g
was active or at rest. In each case h
the genius to appreciate the value o
new truths which thus incidentally,
were, came to the surface. A
ordinary observer, with his mind
solely on his main theme, might
neglected these so to speak side is
It was Bernard's characteristic, and
secret of his success as an inquirer,
he was ever ready to turn aside and gr
truth thus presenting itself by the way

Though Bernard admitted, and in
himself supplied the mechanical explan
of the change in colour of the blood a
mere result of the widening or narro
of the arteries, he never even up to
end abandoned the position which h

at the first taken up, that the rise of perature which follows section of sympathetic fibres is not to be expl as the mere result of the fuller rus blood through the widened blood-ve He insisted to the last, that there wa that there might be, a direct action of nerve on the tissues changes which fo the local source of heat. In his "Leçor la chaleur animale," delivered in 1872 published in 1876, little more than a before his death, we find expressions c views on this question cropping up time to time. Thus, p. 222, "The cal phenomena depend on actions of two k on a vascular action and on a concom chemical action." Again, p. 288, " nervous system seems at first sight to l about calorification only by the inter tion of the circulation. It is to a motor action alone that one at first r

the modification of animal heat. Alth
this may be true to a certain exten
cannot, however, consider it as an ade
cause, we cannot refuse to admit an a
of the sympathetic different from a p
vaso-motor action, an action which ha
a result a local increase of activity i
chemical changes of the tissues attende
a direct production of heat. It is not
by dilating the vessels, by increasing
local circulation, by bathing the tissues
fully with hot blood, that the section c
sympathetic brings about a rise of
perature; it acts also by increasing the
combustions or chemical metabolism.
vaso-motor action is accompanied
chemical action on the nutrition o
tissues. . . . Conversely, it is not
because it constricts the blood-v
that the galvanisation of the sympa
produces cold, it is because it checks

slows at the same time the chemical m
ment of nutrition. So long as one l
upon the lowering of temperature as
result simply of the constriction of
vessels, one may confine oneself to spea
of the sympathetic as a constrictor n
of the blood-vessels. But if one admi
I do, the independence of the two effe
special name is wanted for each. One
say that, apart from its vaso-motor ac
the sympathetic exerts a thermic influ
Stimulation of it produces a frigorific ef
section or paralysis of it produces a cal
effect. It is not only a constrictor n
of the vessels, it is also a frigorific ne
Again, p. 443, dealing with fever he s
"The phenomena of nutrition are of
kinds: the one kind is that of destruc
of splitting up, of material disorganis
or combustion; the other is of organis
or organic synthesis." The latter

nomena are under the influence of frig
nerves which belong more especially t
sympathetic system ; the phenomer
combustion are more specially govern
the vaso-dilator or calorific nerves w
belong more particularly to the cer
spinal system. "Now fever is essen
an exaggeration of the action of
calorific nerves and not merely a par
of the vaso-constrictor nerves."

It is almost impossible to exagg
the importance of these labours of Be
on the vaso-motor nerves, since it is a
impossible to exaggerate the infl
which our knowledge of the vaso-r
system, springing as it does from Berr
researches as from its fount and o
has exerted, is exerting, and in wid
measure will continue to exert, on al
physiological and pathological concep
on medical practice, and on the co

of human life. There is hardly a ph
logical discussion of any width in v
we do not sooner or later come upon
motor questions. Whatever part of
siology we touch, be it the work
by a muscle, be it the various kin
secretive labour, be it the insurance o
brain's well-being in the midst of the hy
static vicissitudes to which the chang
daily life subject it, be it that mainter
of bodily temperature which is a cond
of the body's activity : in all these,
many other things, we find vaso-m
factors intervening. And if, passing
insecure and wavering line which
health from illness, we find ourselves
ing with inflammation or with fever
with any of the disordered physiolo
processes which constitute disease, we
find, whatever be the tissue specially affe
by the morbid conditions, that vaso-m

influences have to be taken into acc
The idea of vaso-motor action is w
as a dominant thread into all the
siological and pathological doctrine
to-day ; attempt to draw out that tl
and all that would be left would appe
a tangled heap.

All this dominant knowledge has c
as does a full stream from the s
which is its source, from Bernard's i
experiment on the cervical sympat
This is one of not a few instanc
which a simple experiment on a l
animal, has brought suddenly a
light in a field where men had
groping in vain with the help of
clinical observations. Before this s
experiment attention had again and
been drawn to cases in which there se
to be some connection between va
changes and affections or conditio

nerves; but in none of these did
come to light any clear teaching a
what that connection really was; all
uncertain and obscure. The resul
the experiment was the first clear
which broke upon the subject; an
was the following up of the teachin
the experiment which supplied the i
pretation of the hitherto obscure cli
facts.

And it may be well here to
that the experiment in question was
is called a vivisectional experiment
experiment which Bernard, had he
in this country and in our day, n
have been prevented from doing
work might thus have been strai
at its very birth. Some, in whom
ment is stronger than knowledge, are
of declaring that all such experiment
useless and needless, since the know

gained by them might be come at in
ways. The unbiassed inquirer in
genesis of scientific truths and concep
may be ready to admit that in the c
of time experiments of Nature's ma
not of man's, might have suggest
some quick mind that nerve-fibres a
blood-vessels, and might even have h
how they act. And haply to the
quick mind, or to others following
him, duly impressed with what had
thus suggested, there might after
at some time or other, by fort
occurrence, have come other like ex
ments of Nature confirming the sugg
and establishing it as a proved truth.
unbiassed inquirer will admit this ; b
will also acknowledge that up to the
of Bernard's experiment all the ex
ments which a seemingly cruel Natur
carried out year after year, and day

day, on suffering mankind and suff animals, passed before the eyes of obs after observer, quick to see and eag note, without suggesting anything than the dimmest and shadowest of such an action of nerve-fibre on bl vessel. And he will also admit that stroke of Bernard's knife — a st bringing a pain which shrinks t vanishing point compared with the which it has been the means to spa laid bare a truth, which all Nature's strokes had during long years been ur to bring to light.

During the latter half of the cer which is drawing to its close, the p of the healer to cure or lessen dis and to prevent or soften pain, has gr with a swiftness which is in a mea marvellous, and that in spite of the g helplessness which is still all too c

witnessed. That power is, as we
just said, in part the outcome of t
wider views of vaso-motor action;
whatever we may say about the r
have been, there remains the plain hi
cal fact that those wider, truer views
had their origin in Bernard's initial ex
ment on a living animal.

VI

Other Discoveries

THE discoveries of glycogen, of motor nerves, and of the actic the pancreatic juice form Bernard's gre claims to fame; but he also enri physiology with a large number of re: of value less than that of any of the al though of varied importance. We dwell on two or three of these onl

In the quite early years of his caree just missed the opportunity of associ his name with a discovery, the inflı of which on the progress of physi(has been not much less than that o:

discovery of glycogen or that of the covery of vaso-motor nerves.

If the views accepted and expound physiologists at the present time, espe perhaps those relating to the actions t place within the central nervous syste analysed, it will be found that the do of inhibition plays a very important It is not less dominant, it is perhaps more dominant, in pathological views in the application of physiology to m practice. Now the doctrine of inhil had its origin in an experiment made by the brothers Ernst Heinrich and E Friedrich Weber orally, at Naples, i fall of 1845, and by means of pri 1846; the experiment, namely (no well known), of the stoppage of heart's beat by stimulation of the nerve. That experiment, which sti mains the typical inhibition experi was the first clear proof that a ne: impulse, instead of giving rise as in r and indeed in ordinary cases, it to an expenditure of energy, may c expenditure and by banking up er increase in this tissue or in that potential store. It has been the star point of a clearer insight into the mole changes of the tissues, and into the r of working of the nervous system.

Now in 1846, in the very same ye: which the brothers Weber published discovery, Bernard had quite indep ently come upon the same result. himself tells us[1] that in that year ausculted a dog while the vagus n was being stimulated, and that he " served with the greatest ease that every galvanisation the heart stopped the sound ceased, recurring again so :

[1] "Leçons sur le système nerveux," ii. p. 38

as the galvanism was removed." mentioned the result in his private co and it was published by one of his p a Dr. Lefèvre, in a thesis which app in 1848.

But Bernard never grasped the bearing of the result which he observed. Though he returned tc subject in a communication to the S de Biologie in 1849 [1] he never follow up; and indeed, though in his lectur the nervous system, as well as in his writings, he expounds or refers tc facts of inhibition, these never see have largely or deeply occupied thoughts, never tempted him on to culations as to the nature and of action of inhibition, such as largely exercised the minds of many man and other physiologists. Indee

[1] "C. R. de la Soc. de Biol.," 1849, p. 1

late as 1858, he speaks of the stoppa the heart by the vagus as "a sin experiment of which several interpreta and explanations have been offered."

Bernard was very early attracted t(study of poisons. He recognised in t as he himself has said, physiologica struments of greater delicacy than mechanical means at the disposal o physiologist, instruments capable of lysing, of dissecting as it were, anatomical elements of the body this is yet alive. He looked upon as true "vital reagents." Studying from this point of view, rather than the desire to compile the com toxicological or physiological histor any one of them, he was, on the hand, led to important general siological conclusions, and, on the

hand, enriched his science with val
new methods of inquiry. His succ
this direction was conspicuous in the
of curare and carbonic monoxide.

Curare, otherwise spelt as urari, a
many other ways, an arrow poison c
South American Indians, was first br
to Europe by Sir W. Raleigh from G
in 1595. It had been described by
authors, its botanical origin and its ge
chemical features had been studied,
several authors including Brodie the
English surgeon, and Charles Wate
the traveller, had experimented wi
But Bernard was the first to analyse
accuracy its physiological action.

He tells us that in 1844 he received
his friend Pelouze, a supply of the p
being some which Goudat had br
over from Brazil, and immediately
to experiment with it. He did not,

ever, publish anything of the results v
he had obtained until 1850, in v
year he made a communication tc
Académie des Sciences on October 15
to the Société de Biologie in Decem
In the former longer paper he hardly
more than that the poison kills rapidly
out convulsions and at once renders
nerves inexcitable; he does not disting
between different kinds of nerves, in re
to its action; and indeed the greater
of the paper is taken up in showing
the poison does not diffuse from the
rior of the alimentary canal into the b
and hence is harmless when swallc
though a minute quantity introduced
a wound is rapidly fatal. In the se
communication he says that the p
abolishes reflex actions, destroying ra

[1] "Compt. Rend.," xxxi. p. 533.
[2] "C. R. de la Soc. de Biol.," 1850, p. 1[...]

and completely the motor and se
properties of the nervous system; ar
especially insists that, while it at once re
the nerves inexcitable, it leaves the mu
fully excitable, in this respect afford
marked contrast to nicotine, which
troys the irritability of the muscles a
causing death brings about convul
This marked effect on the nerves in
absence of any effect on the muscles
recognised by Bernard, and perhaps
more distinctly by others, as a convin
proof of the correctness of the view
the irritability which muscular tissue
plays is an independent property o
own, and not merely one conferred
by the nervous tissue supplying
matter which at the time served a
occasion of lively controversy.

From 1850 to 1856, Bernard publ
no formal account of the resea

which he continued to make on cı but from his paper communicated tı Académie des Sciences in 1856[1] as as from his "Leçons sur les effets substances toxiques," published in ı we learn that already in 1852 he arrived at and made known, : rently in his lectures at the Collég France, further remarkable results, ı in the succeeding years he continuı extend and complete.

In 1852, having observed that muscles of a frog poisoned with cura far from being less irritable, seemed more irritable, than normal muscles, being aware that the individual differ existing between frogs rendered the parison of the muscles of one frog those of another more or less inexact at least inconclusive, he was led to m

[1] "Compt. Rend.," xliii. p. 825.

comparative experiment on the musc
one and the same animal, by tyin
blood-vessels of one leg, so as to sh
the blood stream from the tissues of
leg before he introduced the curare
the circulation. He found his
corroborated; the muscles in the
supplied with blood and so with p
were more irritable to direct
tric stimulation and remained so
longer time than did the muscles i
leg from which the blood and ther
the poison had been cut off by the
ture. But in making the experimen
attention was arrested by another fact,
the leg, protected from the curare b
ligature not only remained sensitive, s
it was moved when it was stimulated
also that movements took place in it
the skin in the parts of the body to
the poison had had access were stimul

that is to say, stimulation of the
which produced no reflex action i
poisoned moiety of the body, could
about by reflex action movements o
muscles in the unpoisoned leg. H
once grasped the meaning of this, na
that while motor nerves were ren
inactive by the poisons, the sensory n
and the central nervous system rem
intact. Here again we have an ins
of how Bernard's genius led him to
aside from an inquiry which he had b
in order to follow up a hint which,
were, accidentally presented itself. W
he made the experiment his mind
wholly directed towards the influen
curare upon the muscles; but he at
left these to seize upon the new fact
cerning nerves, which had always esc
him in his previous observations.
similarly devised experiments, now

common demonstrational experimer the lecture room and the laborator supplied proof that curare acts upoı motor nerves, the abolition of their tions being peripheral, not central, anc not only the motor nerves to the sk muscles, but other efferent nerves, as the vagus fibres to the heart, are l adequate dose of the poison similarly lysed.

This fundamental fact he had observ early as 1852, but in this case as in so ı others he did not rush forward to pu it. In contrast to many of his own tim since he had a dread of making knowı new result, however important and sı might seem, until he had had opportuni work the matter thoroughly out. Ir he complains of how against his will l times gave to the world a discovery in aı finished condition, because the insuffici of his laboratory and of other means o perimenting left him no hope of perfe it as he desired. " I am only too far with the regrets of the investigator simply by reason of the lack of ma means is prevented from carrying ou experiments which he has devised, a driven to abandon researches which h in his head and is led to make kno discovery while it is as yet a mere r sketch, not a completed work."[1] It wa until 1856 that he gave a formal accou what he had discovered four years be and he was led to speak then be Kölliker at Würzburg, in that year lished a paper showing that he had pendently arrived at the same main re The priority however clearly belon Bernard, though Kölliker's investig was in some respects more complete

[1] "Physiologie générale," p. 209.

exact. Since then curare has becom
instrument in the hands of the physiol
of the same order as that supplie
anæsthetics ; by enabling him to ab
temporarily the movements of the sk
muscles it has enabled him to carr
observations which could not have
made at all or could not have been
satisfactorily without such aid. An
knowledge which has thus been gain
indirectly due to Bernard.

In many of the instances in which i
writings Bernard has dwelt on the a
of curare, he has in an almost dra
manner insisted upon its leaving i
the sensory elements of the ne
system. And indeed there is every
son to believe that with a certain do
the poison an animal may be killed
out its sensations being affected unti
nervous system is poisoned not by the

but by the blood ceasing to be oxyger owing to the paralysis of the respir pump. But Bernard himself came to r nise that the exact limits of the action (poison were largely determined by the that while a small quantity in the blo any one time affected only the motor n of the skeletal muscles, a larger quanti terfered also with the vaso-motor ne and Kölliker was nearer the truth Bernard when, even in his first comn cation, he maintained that the poison act on the central nervous system a affect sensation if present in the blo adequate quantity.

Of hardly less value than his wor curare was Bernard's analysis of the pl logical (poisonous) action of carbonic oxide gas. The extremely poisonous n even in small quantity, of this gas, s to appear when combustion, as in st is imperfect, had long been known ; a also the fact that though death throu seems to be a kind of "suffocation, blood, during the poisoning and after is not as in ordinary suffocation almost black, but bright red in cc as bright or even brighter than ord arterial blood. But how the gas actec how it caused death, was unknown Bernard took up the matter. In re to this also, as to curare, he had solve problem some time before he for announced the solution. His formal munication to the Académie des Sci was not made until September, 1858 ; he there says that he had made his observations ten years before and expounded his conclusions in his lec of 1853, and again in those of

[1] "Compt. Rend.," xlvii. p. 393.

Indeed a brief account of these appea
Atlee's notes of his lectures, publish
Philadelphia in 1854; and he hi
included a somewhat fuller description
results in his "Leçons sur les effet
substances toxiques," published in
In any case he had solved the pro
before 1857, and the value of
solution will be apparent if the sta
knowledge concerning the gases o
blood be kept in mind.

Lavoisier, in making known the
damental truth that the central fa
respiration is the disappearance c
certain quantity of oxygen from
inspired, and the appearance of a ce
quantity of carbonic acid in the ex
air, had, at the beginning of the cen
propounded the further view that
oxygen goes and the carbonic com
consequence of a subtle substance con

ing carbon and oxygen being sec
from the blood into the air passages
being there oxidised. This view, th
it came to be regarded as years wer
with increasing doubt, was not w
abandoned until Magnus in 1838
lished his researches on the gases o
blood. Those researches markec
epoch in the Theory of Respira
Magnus showed that both arterial
venous blood contained both oxyger
carbonic acid, that arterial blood
tained more oxygen and less car
acid than did venous, and that re
tion consisted in the blood ga
oxygen and losing carbonic acid i
lungs, and losing oxygen and ga
carbonic acid in the tissues. That
a great step; but Magnus maint
that both gases were simply dissolv
the blood, the quantity of each p

being determined by the law of pres
This view held its position for
years. Though contested by Liebig, i
the dominant view at the time Be
began the observations with which
are dealing. Not until after he
arrived at the conclusion of whic
are about to speak, namely in 1857,
Magnus's views overthrown by obs
tions carried out by L. Meyer on Mag
lines, but leading to the conclusion
the gases of the blood are not w
present as merely dissolved gases,
exist in some loose unstable combin
from which they can, under approj
circumstances, be set free. This M
very distinctly showed to be the conc
of the oxygen in the blood, but he
to discover the substance or substanc
the blood with which it so comb
The red corpuscles were suspecte

be concerned in the matter ; but tl crystals, of what we now call h globin had been described by Rei Funke and others, hæmatin was spoken of as the colouring matt the blood, and all Meyer's attemp connect with hæmatin the absor of oxygen by the blood failed. knowledge on this matter was not on a proper footing until Hoppe (H Seyler) in 1862 and Stokes in published their spectroscopic obs tions.

Meanwhile, before 1857, Bernard got at the truth by help of car monoxide. He had certainly made fundamental experiments in 1855, according to his own account had al made some of them in 1848, h observed the peculiar colour of the l in carbonic monoxide poisoning so

as 1842, at the very beginning alm
his career.

He observed that blood taken fron right side of the heart in animals poi by carbonic oxide and exposed to an sphere containing, like ordinary a known quantity of oxygen, did not, a normal blood, take up oxygen. further observed that when normal was exposed to an atmosphere of car monoxide it gave up oxygen at the time that it absorbed carbonic monc and that the volumes of oxygen so up and carbonic monoxide so abs were equal. Reflecting that the diff colours of arterial and venous blood be due to the behaviour of the corpuscles in relation to the gases o two kinds of blood, he jumped, as it to the conclusion that the red corp were concerned in the retention bot

oxygen and carbonic oxide by the b
This was confirmed by the further c
vation that the beneficial effects oi
transfusion of blood, which are obvi
due to the red corpuscles, since s
free from corpuscles has no such ef
were absent when the blood used
carbonic monoxide blood. This gave
the key to an understanding of the
in which carbonic monoxide poisons
animal body. "I was thus led," say
"to find that this gas rapidly pc
animals, because it instantly displace
oxygen of the red corpuscles and ca
itself be subsequently displaced
oxygen." "The animal dies becaus
red corpuscles are, as it were, para
and circulate as inert bodies devoid o
power of sustaining life."

Some of the details of the experir
on which Bernard based this view m

open to criticism, but it remains n
theless true that by these researche
secured a threefold gain. He expl
the mode of action of carbonic oxide
so opened the way for rational rem
measures. He introduced an easy
ready method of measuring the qua
of available respiratory oxygen in
given quantity of blood, a method w
both in his own hands and in tho
others, has proved of great service.
by a sort of inspiration, he anticipate
conclusions arrived at in a more labo
way by his German brethren, and rea
almost in one step, a correct view o
nature of the respiratory process.
view, moreover, assisted by the me
enabled him to push forward his inq
into the functions and behaviour o
blood as the great "internal med
on which the tissues live, and thus

dentally led him, as we have seen, to discovery of the distinction between v constrictor and vaso-motor nerves.

There are many other results w Bernard arrived at, such as the so-c paralytic secretion, the supposed r activity of the sympathetic ganglia, others ; but on these we need not dwe detail, though they are often quotec physiological literature.

One observation of his, however, per deserves notice. In the "fifties" b gists, especially perhaps in France, engaged in a fierce controversy a spontaneous generation, a controversy to rest in the main by Pasteur, and well nigh forgotten. To that controv Bernard made a notable contributio 1858.[1] He showed that the gro which readily appear in a solution

[1] "Ann. d. Sc. Nat.," ix., 1858, p. 360.

taining gelatine and dextrose exposed the air, do not appear if the solution supplied exclusively with air which passed through a red-hot tube.] argued that the growths which usua appear are not spontaneous in origin l have their source in air-borne germs, a the red-hot tubes killed the germs on th way to the solution. Much of Pasteu refutation of the spontaneous generati theory was on the lines of Bernar experiment.

VII

His Later Writings

FROM the preceding chapters it will b seen that all, or nearly all, Bernard' great achievements were accomplishe during that period of his life which ende with the year 1860. Looking mor closely we may see that the essential result of his two greatest discoveries, the glyco genic function of the liver and the vaso motor system, were gained so early a about 1850, within the first ten years o his career as an investigator.

The great truths, which it was given t him to lay bare, were not reached by th help of easy circumstances and ampl opportunities for inquiry such as fall t lot of at least most of the young m science of to-day. I am not speakir the attendant difficulties of earning the bread; these are known, perhaps one say happily known, to almost all y inquirers of all times. I am contra Bernard's surroundings at the comm ment of his career with those of the y man who at the present time desir devote himself to scientific research. latter, at least in most cases, finds in way or other access to a well-furn laboratory, in which the path of inve tion is made smooth, perhaps in some too smooth, for him, in which ade apparatus is placed at his disposal, a which he is at once trained in the tech of inquiry and guided in his though the sympathetic and suggestive word a wise and experienced master.

None of these things came to Ber
He had, it is true, encouragement ar
example in Magendie, but hardly n
and even this was not much. So :
example went, beyond a strenuous
to put everything to the test of e:
ment, there was little of intellectual
ing which Magendie could impa
Bernard. Indeed, it could not have
long before the pupil began to feel, a
stronger man of the two, his n
enthusiasm for his master checked
growing conviction that the master's
was often pointing the wrong way.
in the details of experimental exe
was there much for Bernard to l
indeed, it was for the most part the
way. After the third or fourth le
at which Bernard assisted as *prépai*
Magendie, struck with the superl
with which Bernard had conducte

lecture demonstrations, said, in his gruff way, as he left the lecture-roo the close of the lecture, "You are a l man than I am."

In the way of material advantage research Magendie had little to offer t assistant. As Bernard himself (" Pł logie générale," p. 203) has told u: perimental physiology was then lc down upon. Natural history was ii ascendant. Botany, zoology, and ge had their museums. The great and (nant Cuvier mocked at experiment living animals, maintaining that were simply sources of erroneous ' Chemistry was rising fast and was provided with adequate laboratories. for physiology nothing was being On the contrary, it was being perse and reviled. "In those days the pł logist had need of a real passion fc science, and in order to ward off fatal discouragement had to possess his soul of high courage and great patience. So soon as an experimental physiologist was discovered he was denounced; he was given over to the reproaches of his neighbours and subjected to annoyances by the police."

In the Collége de France, devoted though it was to scientific inquiry, no better place as a laboratory was found for Magendie than a corner, spoken of as "a damp and dark lair," a hiding-place for a wild beast. And Bernard himself has said: "I had ample opportunity, while I served as his assistant, to note the continual obstacles which the administration of the college put in his way."

It was not in Magendie's power to offer his assistant opportunities for research, and Bernard, even after his appointment to the

assistantship, had to carry on his ex
mental inquiries in some temp
laboratory of his own, or in that o
of his chemical friends, such as in th
Pelouze. We have already (p. 12)
something of the circumstances amid w
he made his first researches; and
following story which he tells in
"Physiologie générale," illustrates
nomadic character of his experim
installations, and the difficulties
which he prosecuted his early inqui

"About 1844 I was studying
digestive powers of gastric juice by
help of the method introduced by B
lot, namely, that of collecting gastric
by means of a cannula, or sort of silve
fitted into the stomach of a live d
such a way that the health of the a
does not in the least suffer thereby.
then the celebrated surgeon of B

Dieffenbach, was on a visit to Paris : hearing of my experiments throug friend Pelouze, he was anxious to w the operation of the introduction c gastric cannula. Informed of his v hastened to gratify it, and performe experiment on a dog in the ch laboratory which Pelouze then had Rue Dauphine. After the operatic animal was shut up in the yard c laboratory in order that we migh amine it again later on. But o morrow it was found that the dog in spite of precautions, escaped, ca still in its belly the accusing cann a physiologist. Some days after quite early in the morning, before got up, I was visited by a persor came to tell me that the police co sioner of the quarter of the Écc Médecine wanted to speak with m

requested me to call on him. In
course of the day I presented myself a
police office in the Rue du Jardine
found there a little old man of a
respectable appearance, who receivec
very coldly, and at first said not
Then, taking me into an adjoining r
he showed me, to my great astonishı
the dog on which I had operate
Pelouze's laboratory, and asked me
admitted having placed in the dog
instrument which he had in his bell
replied in the affirmative, adding t
was delighted to find my cannula a
for I had given it up as lost. My an
however, instead of satisfying the
missioner, appeared to anger him, fo
addressed to me an admonition couch
the severest terms, accompanied by tl
at my audacity in having taken his d
experiment on. I explained that it

not I who had taken his dog, but t
had bought it of the men who were i
habit of selling dogs to the physiolc
and who stated that they were emp
by the police to collect stray dog
added that I regretted having beer
involuntary cause of the pain whicl
misfortune of his dog had caused
that the animal would not die of it
that there was only one thing tc
namely, to let me take back my
cannula and for him to keep the
These last words at once made the
missioner change his manner of spea
and completely appeased his wife
daughter. I removed my instru
and on leaving promised to call a
And, indeed, I visited the Rue du Jar
several times. In a few days the dog
completely cured, I became a frien
the commissioner, and could hencefor

count on his protection. Indeed thi
me to establish my laboratory withi
district ; and for some years, until, in
I was appointed deputy to Magend
the Collége de France, I was enabl
continue my private courses of ex
mental physiology under the licence
protection of the commissioner, wh
I was saved many disagreeable incide

In 1847 Magendie's increasing inf
ties led to Bernard's being appointe
Deputy at the Collége de France, a
career now seemed to be secured to
He could henceforward work in an o
laboratory ; and it was, as we have
during the succeeding few years tha
most brilliant work was done.

Yet in 1851, at the very time th
was unlocking the gates of Fame, cons
though he must have been of the
value of the truths which he was b

ning to make known, he was despa
of the future. The path of the ex
mental physiologist seemed so dou
the difficulties so many and so pre
the hopes of success so few an
shadowy, that he at this time had se
thoughts of abandoning a career of sc
and of devoting himself to active pr
as a surgeon. His domestic rela
probably had much to do with his
couragement; for he had, unhap
married a wife who was in no w
helpmeet to him. Hers was a n
too commonplace to rise to any sym
with his intellectual aspirations; she
not prepared, as he was, to live labc
days on narrow means, in order tha
world might be the richer for the t
which he by patient toil might reap.
desired that the ability, which she le
from those around her gave promi

being of the kind which men call ge
should find immediate reward in
"which all women admire," and sh
change hardships and self-denials into
and affluence. She thought that the
of a successful practitioner, riding to
in his carriage and to-morrow forgo
was far more to be desired than the
of a student whose present lay in obscu
and whose future could not be foretolc

Happily Bernard had in him that w
armed him against the temptation to l
the plough to which he had put his h
When in 1847 he gave his first lectu
deputy for Magendie, he began with t
words: "Scientific medicine, gentle
which it ought to be my duty to t
here, does not exist." He said this kn
ing that he who was speaking had l
the ideas and the skill to realise t
ideas, such as would go far to create

lacking science. With this conscio of his power he could not but g Nor had he long to wait before assu came to him that he was not mistak to the path which he had marked o himself. Whispers ran round amon Olympians of science that a young ph logist was doing remarkable work, wh which soon strengthened into loud of unstinted praise. And speech before long followed by action. Mag still lingering on, Bernard's positic the Collége de France was the ins and insufficient one of a mere de whose term of office was dependent o life of another. Recognising this, recognising also that the physiology Bernard was advancing and expoun was not a mere technical adjunct tc art of medicine, but was of the kind v claimed to be considered as a bran

knowledge, of value for its own sal
a constituent part of philosophy
authorities created in the faculty of Sc
of the University of Paris, at the Sorb
a new chair of General Physiology
placed him in it as the first occu
Though the post carried with it ne
laboratory nor assistants, it at least
Bernard an honourable position. Fu
recognition came in the same year i
form of election into the Academ
Medicine and Surgery, in succession t
great surgeon Roux. And in 1855,
the death of Magendie, Bernard was wi
hesitation called to the vacant chair
made full Professor at the Collèg
France. Henceforward there could l
question of his turning back from sci

During the next few years his ac
was enormous. He extended and
pleted his great discoveries, and w

seventeen years from the beginning
career as an inquirer, while he was
some three years short of fifty ye
age, he had brought to the scien
the sake of which he had deserted
ture, all the wealth of riches on whi
have dwelt in preceding chapters. A
same time he was delivering lectures
at the Collége de France and at the
bonne, a course of forty lectures a
in each place. These lectures we
deliverances of academic platitude
formal expositions of acknowledged
trines, composed once for all, and
without great labour, and repeated
but little change year after year. A
Collége de France, as we have seen
hand was free; he could lecture on
he liked and how he liked. At the
bonne, the regulations for the chair
scribed lectures of a more didactic k

but Bernard, ignoring the regula
lectured there also in the way whi(
thought best, and no one gainsayed
In both places he chose some subject
course, and treated it as an inquiry,
developed as the course went on b
help, not only of old, but also of
experiments. Each course which he
was as a mere course a burden to hin
took no pleasure in exposition, this i
was only weariness to him ; it was inc
and inquiry alone, which satisfied his
In fact, he largely used his lectures
means of making known new result
new or corrected and extended views
many instances the communication in
he made known some new result t
Académie des Sciences or to the Socié
Biologie, seems all too brief and impe
in fact, sometimes almost bald. One l
look to the published Leçons, in whic

matter is more fully treated of, in
to get the author's fuller expositic
to learn his more mature view. M
Bernard's results, indeed, can oı
found in these Leçons, and it is tc
that we have to look for an ad
exposition of many of his ideas.
scientiously reported by one or ot
his talented pupils, and carefully ı
by his own hand, the series of lı
thus delivered and published con
Bernard's great contribution to p
logical literature.

In these he developed more fu
views on the questions involved in h
researches, and his conceptions of g
physiological truths. Taken togethe
constitute his testament in which n
found at once an account of hi:
labours, an exposition of the aim
methods of physiology, and a vind

of the value of the science to whi
devoted his life.

The series begins with the "Leço
Physiologie expérimentale," the
volume of which, published in
contains the course delivered at
Collége de France in the winte
1854–55, and deals chiefly with the
siology of sugar and with the glyco
function of the liver, while the se
volume which was published in the
year is devoted to digestion, and inc
an account of the new results abou
pancreas.

Next followed, in 1857, the "Leçor
les effets des substances toxique
médicamenteuses," delivered in 1856
at the Collége de France, in which he,
have seen, expounded his views on
action of curare and carbonic mono
though dwelling also on strychnine, nic

ether, and other chemical sub
The dominant idea of the book
develope the value of a drug as a
of physiological analysis.

In the next year, 1858, he publis
"Leçons sur la physiologie et la pat
du système nerveux," delivered
Collége de France in the winter of 18
The first volume is of the nature
general exposition, though three l
are devoted to the diabetic punctur
second deals with the physiology
several cranial nerves, with which,
pupil of Magendie, Bernard was thor
conversant, and on which he himse
worked; and the last two lecture
stitute an exposition of the new
as to the sympathetic nerve.

In 1859, appeared in two volum
"Leçons sur les propriétés physiolo
et les altérations pathologiques des li

de l'organisme," in which developin pregnant idea of the blood as the "int medium" on which the tissues liv discoursed of the physiology of that other fluids of the body in health a disease. These seven volumes of lect all delivered at the Collége de Franc some years later formally presented t Academy of Sciences.

They were followed in turn by "Leçons de pathologie expérimen "Leçons sur les anesthétiques et l'asphyxie," "Leçons sur la ch animale," "Leçons sur le diab "Leçons sur les phénomènes de la and lastly by the "Leçons de Physio opératoire." They form altogether octavo volumes. He who reads the v series consecutively will naturally upon much repetition, especially in exposition of ideas; but that is ins able from the circumstances of the delivery of the lectures and of their publication. The more important of the topics dwelt upon in these several courses of lectures have been referred to in their proper place in the preceding chapters. It may, however, be worth while to point out the especial value of the lectures on animal heat, not only in that they contain, as we have already indicated, an exposition of Bernard's views as to influences of nerves on the chemical changes of the tissues and so on the development of heat, but also and perhaps especially in that they embody the results of his observations on the topography of bodily temperature, on the differences of temperature presented by different organs and parts, and on the causes of those differences. For Bernard largely extended, or perhaps rather established, our knowledge of this subject.

In the long bibliographical list of Bernard's writings, a break is found in the year 1863. While from 1843 onward, each year is marked by some and generally by many utterances, he published nothing between the fall of 1862 and the spring of 1864; in 1863 he was wholly silent. The incessant labours of the preceding years had told upon him; and in the winter of 1862–63 he wholly broke down. The state of his health gave great anxiety to his friends, they feared that the brilliant inquirer was prematurely to be taken away from them. The exact nature of the malady which attacked him was in many ways obscure. It was an abdominal affection, apparently an abscess, giving rise to periodical attacks of fever and acute pain, with intervening remissions. An exact diagnosis was never made, but it seems not unlikely that the illness would nowadays have been recognised as appendicitis. He suffered from it at intervals until 1866–67, when, after a most severe attack, accompanied by intense fever, during which his life was despaired of, the abscess appears to have discharged into the bowel. He then slowly recovered his health, and by 1869–70 had become once more sound and strong.

The early attacks of the malady led him in 1862–63 to desert his laboratory and seek for restoration to health by a long sojourn in his ancestral home at St. Julien. The rural abode with its surrounding vineyards had not passed into other hands. Bernard, since the death of his parents, had kept possession of it, and thither he had been wont to return each autumn, restoring his mind and body during his vacations with the pure air of the country and the quiet occupations of a rural proprietor.

Here, in the soothing autumn days, he went to and fro, tending his garden, caring for his fruit trees, many of which had been grafted by his own hands, and above all watching over the vintage, the produce of which appeared upon his table at Paris.

In this quiet retreat he spent the greater part of the year 1863; but as health and vigour came back, at least in part, to him, the daily round of rural pleasures and occupations soon began to be too small to fill his mind. Away from his laboratory and instruments, experimental inquiry was impossible for him; but the science of his adoption was ever present to his mind, and he made use of this enforced leisure and his returning vigour to develope in a systematic manner those views on the nature of the true methods of physiological inquiry, which from the very first had guided him in his investigations, and of which he had from time to time, as occasion presented, given out fragmentary utterances. The result was a volume, published in 1865, entitled "Introduction to the Study of Experimental Medicine," and intended to serve as the introduction to a larger work with the title "Principles of Experimental Medicine;" this latter however never saw the light.

In this "Introduction" Bernard expounded that conception of biological inquiry to which he gave the name of "determinism." When he began his medical studies he found, as we have already said, the opinion very common—his teacher Magendie himself holding it—that really vital phenomena were not subject to law, and therefore were beyond the pale of scientific investigation by experiment and observation, this being applicable only to the "physical phenomena of life." Bernard's position was that the manifestations of the properties of living bodies are bound up with the existence of certain physico-chemical phenomena, and that the latter furnish the conditions of the former. He insisted that in living, no less than in non-living bodies, natural phenomena are rigorously dependent on conditions, and that in the case of both the object of scientific inquiry is to lay bare the connection of the phenomena with the conditions. He expressed himself somewhat as follows: "In both biological and chemico-physical studies the inquirer meets with a double set of conditions. He has to consider, on the one hand, the body in which the phenomena occur, and, on the other hand, the external circumstances, or the 'medium' by which the manifestations of the phenomena are determined or provoked. In both studies the rigorous determination of the conditions is possible because matter itself is devoid of spontaneity, in both studies the limits of our knowledge are the same, in both studies in order to arrive at the 'determinism' of the phenomena, it is necessary to bring those phenomena into experimental conditions as definite and as simple as possible. The experimental physiologist knows nothing either of spiritualism or of materialism; such words belong to an effete philosophy. We do not know and never shall know either spirit or matter. First causes do not belong to the domain of science; they will ever be beyond our grasp, whether we are dealing with living or with non-living things. The true experimental method has no part whatever in the chimerical search of the 'vital principle;' there is no more a vital force than there is a mineral force, or, if one prefers to say so, the one exists quite as much as the other."

To push forward a vigorous and exact analysis by physical and chemical means of the phenomena of living bodies, was Bernard's conception of the task of the physiologist, the analysis being carried out either by simple observation or that "provoked observation" which is called an experiment. This led him, in the work in question, to caustic remarks on systems and doctrines. "Experimental medicine, that is physiology, belongs to no medical doctrine and to no philosophic system."

It is worthy of note that in his various writings Bernard repeatedly uses the phrase "experimental medicine" as identical with "physiology." To his mind it was perfectly clear that the treatment of disease was simply the practical application of pathological truths; and it was equally clear that all distinctions between pathology and physiology, as those between health and disease, were artificial, or of the surface only. To him the phenomena of the living body presented the same fundamental features, and had to be studied by the same canons of inquiry, whether the body in which they appeared were called sick or called sound. But, though he never doubted in the progressive power to solve the problems of disease of a growing physiology, which increasingly laid hold of the deeper and more general laws of life, he saw the dangers which beset the premature application to practical needs of unripe and superficial physiological views. He says at p. 348 of the work of which we are speaking—

"But no one in the present state of biological science can pretend that physiology is able to supply complete solutions of pathological problems; we must ever strive to solve those problems by physiological inquiries, for that is the true scientific path; but we must carefully guard against the illusion that we have already gained the solution. Hence, the prudent and reasonable course at the present moment is to explain all that part of disease which can be explained by physiology, and to leave that which we cannot so explain to be explained by the future progress of biologic science. This kind of successive analysis, which, in its application to pathological phenomena is carried only so far as the progress of physiological science permits, isolates little by little, by way of elimination as it were, the essential element of the disease which is thus being studied, lays hold of its characters with greater exactitude, and allows therapeutic efforts to be directed with greater certainty. Besides, with progressive analytic advance, the proper character and physiognomy of the disease are preserved. But if, instead of this, some delusive approach of physiology and pathology gives rise to the ambition to explain prematurely at one step the whole of the disease, then one loses sight of the patient, one gets a wrong idea of the disease, and by a false application of physiology, experimental medicine is hindered instead of being assisted in its progress."

No less clear than his views of the relation of physiology to medicine were his conceptions of the philosophical aspects of physiology. In many popular writings may often be found the idea that the progress of the experimental sciences, such as physiology, is the result of a combination of the labours of two kinds of men. In such writings it is taught that the new facts of science are gathered in by a laborious set of men who make experiments and observations, and thus bring to light new things; but who, humble in nature, and lacking the power of insight into deep truths, leave the truths thus discovered to be dealt with by higher minds, who, relieved from the tedious labour of collecting facts, can spend all their energy in the elaboration of great generalisations. The former are, in the eyes of the popular writers of whom we are speaking, men of science in the general acceptation of that term; such men are happily abundant. The latter, much more rare in their occurrence, are spoken of as philosophers. The former are mere labourers, delving after little truths; the latter hold the lamp which lights the others to their work. The writers in question point to Francis Bacon as such a philosopher, such a holder of the lamp, and seem to think that to the light shed by him, the great advancement of natural knowledge which marked his time, and the times which followed, was mainly due.

Such was not Bernard's view. He says, "men of science, as Maistre has said, make their discoveries, work out their theories, and build up their science, without the aid of philosophers. They who have made the most discoveries in science are those who have never known Bacon; and those who have read him and meditated upon him have, like Bacon himself, had but little success as inquirers."

It may be added that no one who has any knowledge of the development of sciences will do otherwise than agree with Bernard as against the popular writers. Indeed, the characteristic trait of scientific inquiry, in whatever branch, is, that it is even in its humblest efforts "philosophy." Moreover, not only is it well-nigh impossible to reach the solution of even the smallest scientific problem without finding that it bears on some truth greater than itself; but it is also, and even more so, impossible to gain a true insight into larger scientific verities without a strict and often a prolonged apprenticeship, in what to the popular writer seems the hodman's part of scientific inquiry. Only by letting the spirit which dwells in each branch of science soak, as it were, in the mind by repeated and almost daily converse with its facts and simpler truths, can any one hope to get a real grasp of its higher teachings.

The meditations on method to which Bernard was thus led in his enforced retirement, though they were merely the fuller development of what had been in his mind from earlier days, made themselves felt during the whole of the rest of his life. Even in his earliest papers, short as they often were, he frequently turned aside from the narration of an experiment, and the discussion of the conclusions which might be drawn from it, to point a moral as to the excellence of this or that method, and to insist on the criteria of the true spirit of inquiry. But from this time onward deliverances on method became, in all his writings, longer and more frequent. Indeed, it may almost be said that the rest of his life was in the main devoted to the completion of his earlier labours, and to an exposition, richly illustrated by instances new as well as old, of the principles which ought to guide the investigator into biologic problems. Though never a year was passed, though never a course of lectures, or even a single lecture, was delivered without his bringing to light some new fact, or placing some old fact in a new light, he never again made known results of such supreme importance as those which his earlier labours had brought to him. He was led more and more into general views, and the problems which he attacked in his lectures took on more and more a general nature.

These features may be observed in the contribution which he made in 1867 to the "Recueil des Rapports sur les progrès des lettres et des sciences en France," and which was republished in 1872 under the title of "De la Physiologie générale." They may also be seen in his various later lectures. But they are conspicuous in his "Leçons sur les phénomènes de la vie communs aux animaux et aux végétaux." Indeed, in his later days he became more and more drawn towards those fundamental properties of living bodies which may be observed alike in animals and in plants.

In this latter line of inquiry he was a leader. Every inquirer, it is true, into physiological problems who, whether in these latter days or in the old times, has reached the truth in some special investigation, has looked with wistful eyes at the deeper, more general, questions which lie below the special ones, and which are, as it were, laid bare by the solution of the special problem. Those general questions are such that in discussing them the superficial differences between the plant and the animal seem of insignificant moment. To-day, perhaps, when so many special problems have been successfully solved, men's minds are becoming more than ever busied with such general problems presented by all living beings, whether simple or complex, whether called animals or plants. And Bernard, in being drawn especially towards such problems, was a pioneer on a line of inquiry which is now engaging many active minds, and which seems likely to be energetically pursued, and to bear fruit even in the near future.

VIII

Latter Years

IN the summer of 1864, soon after his return with at least temporarily restored health to active life and the joys of laboratory work he made his first acquaintance with the Court. In his private life he had ever shown a retiring disposition; nothing could be more repugnant to his nature than any wish to push himself into the notice of those whom others might consider as great and influential. He had his own idea of what true greatness was; he knew, too, his own worth; and he only cared to be thought well of by those of whom he himself thought much. He felt honoured when the great men of mind praised him, and counted it a great thing when the Academy took him into its select fold. But he had no desire for, and indeed shrank from, social distinctions and from marks of favour at the hands of those in high places. Had he wished otherwise, the way would have been easy for him. With Duruy, now high in political position, he had been familiar while the politician was as yet a humble professor. With Henry St. Claire Deville, who at that time was on terms of intimacy with the Emperor, he had close personal relations, being indeed much attached to him. These, or others, could readily have brought him into the circles of high society. For a long time, however, he held aloof. Not that he had any very strong political feelings, but he simply did not care for social distinctions. One day, however, the Emperor Louis Napoleon, always anxious to secure the goodwill of eminent men of science, invited Bernard to take part in the festivities at Compiègne. Bernard accepted and paid the visit in company with Pasteur, who was also one of the many invited guests. The Emperor, as is well known, had pronounced spiritualistic tendencies; and it may be readily imagined that when he began to talk with Bernard he found he had come across a mind able to tell many things which were to him of a new and startling nature. So fascinated was he with what the physiologist had to say concerning the problems of life and the proper attitude of mind in which to approach them, that the talk instead of being limited to a few courtly remarks and polite rejoinders, was prolonged, to the envy of others, into a lively discussion of some two hours length, which the Emperor concluded by saying, "You are a great man of science, and I want you to be pleased with me." Calling to his side M. Duruy, the then Minister of Public Instruction, he said to him, "You know M. Claude Bernard see that he has all that he wants."

A few days afterwards the Minister sent for Bernard, and asked him what could be done for him. "For myself," said Bernard, "I want nothing; but my science is in great want of proper laboratories." Up to that time his only laboratory had been the one at the Collége de France, for the chair which had been created for him at the Sorbonne carried with it neither laboratory nor assistant. As the result of the interview at Compiègne two well installed laboratories were established, one at the Sorbonne, the other at the Museum d'Histoire Naturelle at the Jardin des Plantes, the latter in connection with a chair of Physiologie générale, which was being instituted for him there, and into which he entered in 1868, relinquishing the chair at the Sorbonne to his pupil, Paul Bert.

Thenceforward his life, though it continued to be one of labour, was one of well-being and honour. In 1868 he was admitted into the Académie Française, and made one of the "Immortals," replacing Flourens; he took his seat on May 29th in that year, pronouncing, according to custom, the Éloge on his predecessor.

In 1869 the Emperor made him a Member of the Senate; but he never took his seat, and the events of 1870 deprived him of it. He had conquered the Emperor, but the Emperor had also conquered him ; and though in earlier days he might have been considered as an Orleanist, not so much from personal conviction as because many of his early associates belonged to that party, he at this time might have reckoned among the Imperialists. For this reason, perhaps, but also and perhaps still more because he felt no real interest in such things, he held aloof for the rest of his life from politics, though he became friends with many of the men of the Republic, more especially with Gambetta.

During these latter days he lived in comparative comfort, in adequate apartments on the first floor of No. 40, Rue des Écoles, just opposite the chief entrance of the Collége de France. He had by this time quite recovered from the illness of which we spoke in the last chapter. Indeed his health had become better than it ever had been before ; from being thin, worn, and pale, he grew to be somewhat stout, and a healthy colour was to be seen in his cheeks. He lived alone, for the dissensions with his wife, to which we referred some time back, had led to an early separation, and his two daughters, his only children, were also estranged from him. One of these, who is, or a short time ago was, still alive, was so far removed from sympathy with her father's labours, that she spent much of the means which fell to her in founding hospitals for dogs and cats, with the view of atoning for what she considered the crimes of vivisection which her parent had committed. He lived alone, attended by an old servant maid, who, devoted to him, and a skilful cook, took all possible care of him, so far at least as was consistent with the fervent performance of her religious duties. When on Sunday afternoons she was away at vespers, he had himself to open the door to any one who called; and, on one occasion, somewhat displeased at having to do this, petulantly said to his visitor, "It is not for the sake of 'le Bon Dieu' that Marie has gone out. These vespers serve as an opera to servant maids."

He went very little into society, visiting only at a few houses, for the hours which he spared from the laboratory were fully occupied with various labours. Besides the two lectures, which at least he gave weekly, he was very constant in performing his duties towards the societies to which he belonged, the Académie des Sciences, the Académie de Médecine, and the Société de Biologie.

Yet had he pleased, he might have been much sought for. He had charmed the Emperor, and indeed he charmed every one who had the good fortune to meet him. Tall in stature, with a fine presence, with a noble head, the eyes full at once of thought and of kindness, he drew the look of observers on him wherever he appeared. As he walked in the streets passers by might be heard to say, "I wonder who that is; he must be some distinguished man."

And his talk was brilliant, when he was moved to speak. Scattered through his lectures may be found many pithy and epigrammatic sayings; and many others fell from him in friendly intercourse. When one day he said to Gambetta, "It is that which we do know which is the great hindrance to our learning that which we do not know"—the acute politician declared it was even more true in politics than in science.

In his later years his closest personal

friends were perhaps Berthelot, the chemist, and Renan, the philosopher, both his colleagues at the Collége de France; their friendship was one of some thirty years' length; and very often at the close of the day's work the three met in Bernard's laboratory and held together a brief, but genial and witty talk, to the great delight of the young assistants present. He was also very intimate with Davaine, and followed with great interest that inquirer's first bacteriological investigations; indeed he at a very early epoch grasped the great importance and significance of microbic life, and he watched the development of Pasteur's great researches with an attention and appreciation born of a clear insight into their surpassing value.

In his early days he was much attached to the distinguished physician and pathologist, Rayer, whose influence over him at that time was perhaps second only to that of Magendie, and who was of great help to him in his early struggles. Towards his old master, Magendie, his affectionate attitude was almost that of a son, so soon as he had overcome the initial dislike which the former's rough nature and abrupt manners had engendered. Though as his own intellectual character grew he could not help seeing more and more clearly Magendie's failings as a scientific inquirer, he as it were shut his eyes to much; in many respects he followed the lead of his master with almost the obedience of a child. He was especially influenced by Magendie's opinion of the worth of various workers in science. An investigator whom Magendie held in light esteem, or denounced as a mistaken opponent, Bernard made no effort to draw near to; and hence in early days he kept aloof from many men, from Poiseuille, for instance, to know whom would have been of great advantage to him. Indeed it was not until long after Magendie's death that Bernard wholly freed himself from his old master's influence.

As for his pupils, these simply worshipped him. Some great men in spite of their intellectual force, in spite also of the possession of a wholly upright and open character never succeed in gathering round them a body of young men, bound by the ties of personal attachment. Such men are masters in their writings only, not in themselves; the bonds between them and their pupils are of the incorporeal intellectual kind, and have nothing of that body which is fed by love of and esteem for the man. It was not so with Bernard. He had, it is true, pupils in all countries, pupils who had never seen his face and who called him master only because they knew the worth of what he had done. But over and above these he had a closer band of personal pupils, not men of Paris or of France only, but of other lands as well, who had heard his voice, and had watched his hand in the laboratory, and who knew him as a man no less than as a scientific worker. All these loved, admired, and indeed venerated him, not only for the great things which he had done in science, not only for his quick intellect and for the wide grasp of his mind, but also, and perhaps no less, for his charming character and his moral worth.

In 1877, the last year of his life, Bernard made three contributions to the Société de Biologie. One in April was on his old theme, Animal Heat; in this he advanced nothing very new, but dwelt on the topography of the temperature of the blood, and on the view that heat was produced not in one special seat, but in all the tissues, in proportion to the chemical changes of nutrition taking place in them. Once more, also, and for the last time, he insisted on the difference between thermic and vaso-motor nerves. A second in May, and a third in June, were both on gastric juice; thus in these he again returned to the subject of his earliest researches. He also contributed three papers to the Académie des Sciences: one on May 7, a second on May 28th, and a third on September 10th, all three dealing with the old subject of the glycogenic function of the liver. The first is a mere brief note, accompanying the presentation to the Academy of his Leçons sur le Diabète; in this he remarks on the importance of studying pathology, from a physiological point of view. The second and third constitute a more elaborate exposition of his views of the glycogenic function of the liver, during life and after death. He maintains that the latter is simply the continuation of the former, and concludes with the pregnant observation that, while the mechanism of the production of sugar out of starch and out of glycogen, by means of a ferment, is completely parallel in animals and in plants, the question whether a like parallelism holds good with regard to the formation of starch and of glycogen, yet remains to be seen. This is a problem with which he is occupied, and he trusts before long to have something to say about it. That something, alas! was never said.

The last course of lectures which he delivered was one at the Collége de France during the last months of 1877, the subject chosen by him being the technique of physiological experimentation. It was an extended and developed repetition of a course which he had given years before, in 1859–60, at the same place. He recognised himself how much of his success as an investigator had been due to his manual dexterity. He further recognised that this was no accidental condition; on the contrary, he saw that the exact and vigorous analysis of physiological phenomena was in the highest degree dependent on operative skill. Two things, he insisted, were needed for a successful physiological experiment: a clear idea suggesting the experiment, and skill to put the idea to the adequate test. As he said in his "Introduction à l'étude de la médecine expérimentale" (p. 8): "To be worthy of the name, the experimentalist must be at the same time theoretical and practical. He ought, on the one hand, to be completely master of the art of establishing the experimental facts which serve as the materials of science, and, on the other hand, to have a firm grasp of the scientific principles which guide our reasoning in the midst of the widely varied results of the experimental study of natural phenomena. You cannot separate these two things, the head and the hand. A dexterous hand without a head to guide it is a blind tool. A head without a hand to realise its wishes, is an impotent nothing."

In the many lectures which he had given, with the exception of the course just referred to, and in the numerous memoirs which he had written, though he had never failed to give from time to time directions about the hand, he had dwelt chiefly on the head and its ideas. This last course of lectures he proposed to devote entirely to the hand. Nothing he felt was too small, too humble, too insignificant to leave unnoticed. He knew only too well that the success of an experiment might turn on what seemed to be the merest trifle. And he laid out for himself the task of embodying the vast experience of his life in the fullest and minutest exposition of the details of physiological experimentation as it ought to be carried out in order to ensure the greatest result. Thus in the course of lectures in question, after spending some time on the exposition of general considerations, he descends to the lowest details of the laboratory, beginning with precise instructions as to the handling of an animal and as to the administration of anæsthetics in preparation for an experiment.

The course was published after Bernard's death, under the title of "Leçons de Physiologie opératoire." But, alas, only the first five of the lectures were revised by Bernard himself, the last on his very death-bed.

Drawn in his latter days, as we said, more and more to ponder over the fundamental properties of living matter, his mind dwelling more and more on the phenomena which are common to all living things, whether animals or plants, he could not but be led to meditate often on the changes in living beings brought about by the actions of so-called ferments. These he had come across in his very earliest researches. They had been present to him in this research or that, during his whole life ; and while he had been making France famous by the discoveries on which we have dwelt in former chapters, his friend and colleague, the illustrious Pasteur, had been adding like increments to that fame by his researches on alcoholic fermentation.

As is well known, Pasteur had come to the conclusion that while some changes, often spoken of as those of fermentation, such as the conversion of starch into sugar, can be carried out by means of the so-called soluble, unorganised ferments or enzymes, the change of sugar into alcohol and carbonic dioxide is the direct act of the living yeast cell, needs the immediate intervention of a vital factor, and is therefore by its very nature wholly removed from the category of ordinary chemical reactions. As is also well known, he explained this special vital action involved in the formation of alcohol, as a contrivance on the part of the cell to obtain for its ordinary processes of nutrition a supply of oxygen under circumstances in which no supply of free oxygen was present in the medium in which it was living.

To Bernard this calling on the cell as a *Deus ex machinâ* to serve as an explanation of what could not otherwise be explained, was in flagrant discord with the principles of biological inquiry on which he had again and again insisted; and, though his great friendship with Pasteur probably led him to abstain from open criticism, he seems to have marked the problem as one towards the solution of which experimental inquiry might be directed. In 1876 he had said in a letter: "I have in my head ideas which I must above all things work out;" and this possibly was one.

At all events during his holiday stay at St. Julien, in the autumn of 1877, he made a large number of experiments on the fermentation of the juice of the grape. On his return to Paris he continued these experiments in his laboratory during

November and December. But he said little about them, no one had a very clear conception of what were the ideas which he was putting to the test, and as we shall presently state, a fatal illness brought the inquiry to a premature end. While prostrated on what was to prove his death-bed, he said one day to his pupil d'Arsonval, "I believe that I have obtained some results which will put alcoholic fermentation in a wholly new light; but I am too tired and weak at this moment to explain them to you." The strength and clearness of mind needed for the explanation never, alas, came back to him, and whatever views he might have had went down with him into the grave. The thought that he was not to live to give to the world the full exposition and convincing proof of his conception saddened his dying hours. "It would," said he, "have been grand to have ended with that."

After his death, when his effects in the cottage at St. Julien were being examined, a number of very rough and brief notes were found hidden away, and these seemed to indicate the line on which he had been working. They were certainly not in a condition fit for publication; and it is perhaps to be regretted that the enthusiasm of his pupils led to their appearing with an explanatory note in the "Revue Scientifique," on the ground that nothing which the great master had written, however incomplete it might be, ought to be lost.

The publication of these notes gave pain to Pasteur, who saw that their effect was to destroy the theory on which he had insisted so much; he could not bring himself to believe that his old friend who was always wont to exchange ideas with him most fully and freely, could have thus been so long working so to speak against him, without saying so much as a word of what was in his mind. And some words on the matter passed between Pasteur and Berthelot in the Académie des Sciences.

It would be unjust to lay any great stress on these rough notes of a set of experiments obviously tentative and incomplete, or, from abrupt sentences written down here and there, to infer that Bernard thought that he had solved his problem. Still it seems very clear that he did think that he was at least on the road to the proof that alcoholic fermentation could be carried out by means of a soluble ferment, working outside the cell, and so apart from any direct action of the cell, a ferment capable of performing its task amid the free access of oxygen.

Had he lived to complete this inquiry of his, Bernard would, by some twenty years, have anticipated Buchner, his successor in this line of inquiry; his very last research, fragmentary as it was, was a parting proof of how far ahead the light of his genius threw its rays.

During November and December of 1877, Bernard was busy in his laboratory at the Collége de France, chiefly occupied with these fermentation experiments; but on the last day of the year he was seized in the laboratory with a chill, and left it to return to it no more. The chill marked the onset of a grave illness, an acute affection of the kidneys. After lingering for some time he finally passed away on the 10th of February, 1878.

Even on what was to prove to be his death-bed he could not be wholly idle.

The lectures on "Physiologie opératoire," to which we referred above, as being delivered at the Collége de France during the preceding winter and spring, were being prepared for publication by his pupil, Mathias Duval; and Bernard strove to the end to give these a final revision before they saw the light. But only the proofs of the first five lectures thus felt the touch of the master's dying hand; his strength then failed him, he could do no more. The last words which came to the public from him who had wrested such great truths from Nature by experimental inquiry, were words devoted to counsel as to those minute details of the conduct of a physiological experiment, by which the fruitfulness or barrenness of the experiment is so much determined.

At his death all Paris wept. In the

Chamber of Deputies the then Minister of Public Instruction, M. Bardoux, proposed that the great man whom France had just lost should be laid in his grave with all the pomp and ceremony of a public funeral, at the expense of the State. Gambetta, acting on the occasion as the Reporter of the Budget Commission, supported the proposal in a speech in which, speaking not only as an admirer but as a friend, he dwelt on the greatness and goodness of the man who had passed away, insisting that, among other marks of an exalted mind, he possessed this great one, that he had never let himself be led away, either by party spirit or by the dogmas of a school or by private feelings. Up to this time France had given such a token of national esteem to none but princes, statesmen, or soldiers. Bernard was the first man of science or of letters who was thus laid to rest with the display of a great procession and a solemn function in the draped cathedral of Notre Dame.

Dumas, Vulpian, P. Bert, Moreau, and others spoke at the grave in the name of the several societies and institutions with which Bernard had been connected. A year later, in February, 1879, Paul Bert, who followed Bernard in the chair at the Sorbonne, and was, perhaps, his favourite pupil, delivered a discourse upon him in a Conference at the Sorbonne; and a little later, in April, Renan, in succeeding Bernard at the Académie Française, pronounced his Éloge.

In 1886, a bronze statue was erected to him in the court of the Collége de France, the seat of his so many brilliant labours; and in 1894 another statue was erected at Lyons, in the great court of the Faculty of Medicine and Science.

IX

Conclusion

IN the preceding pages we have attempted to show what Claude Bernard did for Physiology, and to indicate, in some detail, the workings of his mind by which he was led to lay hold of the truths which he lay bare to mankind. If we go a step further and attempt to analyse his genius, if we put to ourselves the question what were the qualities of Bernard's mind and character (for the two cannot be separated in an investigator), by which he stood above the barren or even the ordinary industrious inquirer, by virtue of which he, instead of groping long and wearily in dimness if not in darkness, as it were, rapidly or even suddenly diving into the obscure, brought out the truth at once into light, we shall find three conspicuous traits. I say nothing of his conscientious adherence to exact truth, of his refusing to think he saw that which might be expected to appear but was not to be seen, of his never being willing to look upon the "almost" or "very near" as good as "quite." This he had in common with many other observers whose results have nevertheless been of mediocre value. And without this he too, in spite of all else he had, would have been barren or worse. But, over and above this essential condition of all successful inquiry, he had other prerogatives which are not often found in one man. Of these perhaps the most important was an imagination ever on the alert. In this respect he presented a strong contrast to his master Magendie, whose way was somewhat that of a prospector, prodding and digging in all directions, in the hope that the precious ore of a new truth might at times be turned up. Bernard, on the contrary, always worked under the guidance of some leading idea. "He," said he, one day, "who does not know what he is looking for, will not lay hold of what he has found when he gets it." And his fertile mind was ever ready to supply him with a clear idea by which to work. He has himself in his "Introduction à l'étude de la Médecine Expérimentale," given us an admirable description of the genesis and growth of a successful experimental inquiry. To the observer brooding over the phenomena presenting themselves to him there comes the thought that if a certain state of things were supposed to exist, or if a certain sequence of events were supposed to take place, the occurrence of the phenomena must necessarily follow ; and he forthwith set about to seek for evidence whether the things so supposed do really exist or no. Observation starts a hypothesis, and experiment tests whether the hypothesis be true or no. Such is a research reduced to its simplest terms. The experiment once devised must be carried out in accordance with acknowledged rules and precepts ; there is little or no scope here for differences in intellectual power between one inquirer and another. But in the origin of the hypothesis out of the observation, and in the framing of the needed experiment, there is room for all the difference between genius and stupidity or foolishness. It is in the putting forth the hypothesis that the true man of science shows the creative power which makes him and the poet brothers. His must be a sensitive soul, ready to vibrate to Nature's touches. Before the dull eye of the ordinary mind facts pass one after the other in long procession, but pass without effect, awakening nothing. In the eye of a man of genius, be he poet or man of science, the same facts light up an illumination, in the one of beauty, in the other of truth; each possesses a responsive imagination. Such had Bernard, and the responses which in his youth found expression in verses, in his maturer and trained mind took on the form of scientific hypotheses.

An hypothesis may be good or may be bad, may be fruitful or may be barren. This may, on the one hand, depend on the very nature of the hypothesis, which may, even at the outset, in its very origin, be worthless and wrong. On the other hand, failure or success may depend on the framing of the experiment by which the hypothesis is tested. Here, too, the imagination comes into play. The man who constructs a hypothesis without supplying an adequate programme for its trial by experiment, is a burden to science and to the world; and he who puts forward hypotheses, which by their very nature can not be so tried, is worse, for he is a purveyor of rubbish. We can never know what rejected ideas passed through Bernard's mind, ideas rejected so soon as born, because they were unfitted for trial; probably to his as to other like fertile minds, there came many thoughts which he buried at their birth, letting live only those which seemed to him of promise. But this we may say that the force of his imagination was as conspicuous in the framing of experiments to test his views as in the quickness with which new views sprang up in his mind.

In the framing of experiments but not in the carrying of them out. In this we may recognise a salient difference between the foolish and the wise investigator, between the false scientaster and the true inquirer. In the case of the former, imagination, even though, as sometimes happens, it may have been dull and sluggish in building up the hypothesis and planning the experiment, awakens into riotous activity while the experiment is going on; it sees visions and dreams dreams; it sees in the results of the experiment things which never were, is blind to things which stare it in the face, and comes away with a distorted and lying picture of what has taken place. In the case of the latter, imagination, knowing that its work is done so soon as the experiment begins, stands aloof during the whole time that it is going on, making way for calm, frigid observation which, in its perfect action, while it lets nothing escape it, sees nothing but what really is. Such was Bernard's way when he came to the experiment which his imaginings had prompted. Active before and after the experiment, during the experiment itself his imagination was, as it were, dead.

Another conspicuous trait of Bernard, on which we have already dwelt, also the product of his quick imagination, was the readiness with which he turned aside from an inquiry on which he was already engaged, to follow out a new line of inquiry suggested by some intercurrent fact. To divine when thus to turn aside and when not to turn aside but to go straight on, regardless of side issues however tempting, is perhaps the chiefest sign of genius in inquiry. The man who, refusing to take heed of any beckoning by the way, plods doggedly on along the path which he has marked out for himself, may miss a golden opportunity. On the other hand, the man who is always ready to leave the main track in order to follow out the bye-paths, which in almost every inquiry open out from time to time on either hand, runs the risk of losing his way in blind alleys and of coming late to his real goal, or it may be never reaching it at all. Bernard, in nearly every one of his inquiries, was led to turn aside from the road which, at starting, he had marked out for himself; his instinct guided him to leave the road at the right turning, and to follow a bye-path which brought him to a great result.

Lastly, Bernard's success was in no little measure due to his remarkable manual dexterity. His hand was promptly obedient to his mind. His facility enabled him to put sharply and clearly the question which an experiment embodied; and hence the answer came to him sharp and clear. His old pupils still speak with admiration of the almost marvellous celerity and directness with which he would perform a most intricate and difficult operative experiment. Without haste and without hesitation, taking step after step swiftly and in due order, he would with exact strokes lay bare and isolate a delicate structure by disentangling it, with the utmost neatness, from its perplexing surroundings, and would complete a difficult operation in time needed by others for mere preliminary preparation. It is told of him that sometimes, urged by the pressing need to get an immediate answer to some question with which his mind was stirred, he would come suddenly into the laboratory, call for an animal, and then and there, without so much as removing his hat, perform an experiment, it may be, of no little difficulty. A false worship of intellectual supremacy has led some to ignore the value of bodily attributes as aids in the pursuit of truth, and as elements in the composition of genius; and indeed in some branches of learning a failing eyesight and a clumsy hand do not present themselves as serious obstacles to success in reaching the inner secrets of the nature of things. In experimental science it is otherwise. Here great truths are for the most part come to by treading a flight of steps, each step an experiment resting on the one below, and leading to the one above. If any one step goes wrong the whole ascent is stopped; and the experiment will go wrong if there be bungling in the execution of it, if the details, even the minutest ones, fail to be deftly carried out. The clumsy experimenter may count himself fortunate if his clumsiness only leads to loss of time, if, through lack of the needed dexterity, he simply fails to carry out the experiment exactly as he wished, and therefore has to try it all over again. The much more common occurrence is that the want of skill mars the experiment, by introducing something, which enters unnoticed into the result and leads, without the experimenter being aware of it, to a wrong or imperfect answer being given to the question asked. A clumsy experiment is in most cases a bad experiment, leading to a wrong conclusion; and the evil which the clumsiness thus entails is all the greater, the more acute and the more active the mind which is guiding the clumsy hand. Hence it comes about that in experimental science skilfulness of the hand, no less than quickness of the mind, must be counted among the attributes the possession of which gives a man the power to pierce swiftly and surely into the secrets of nature, the power which his fellow men seeing in him, speak of him as having genius. Such a skilled hand had Claude Bernard.

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The Gresham Press,
UNWIN BROTHERS,
WOKING AND LONDON.