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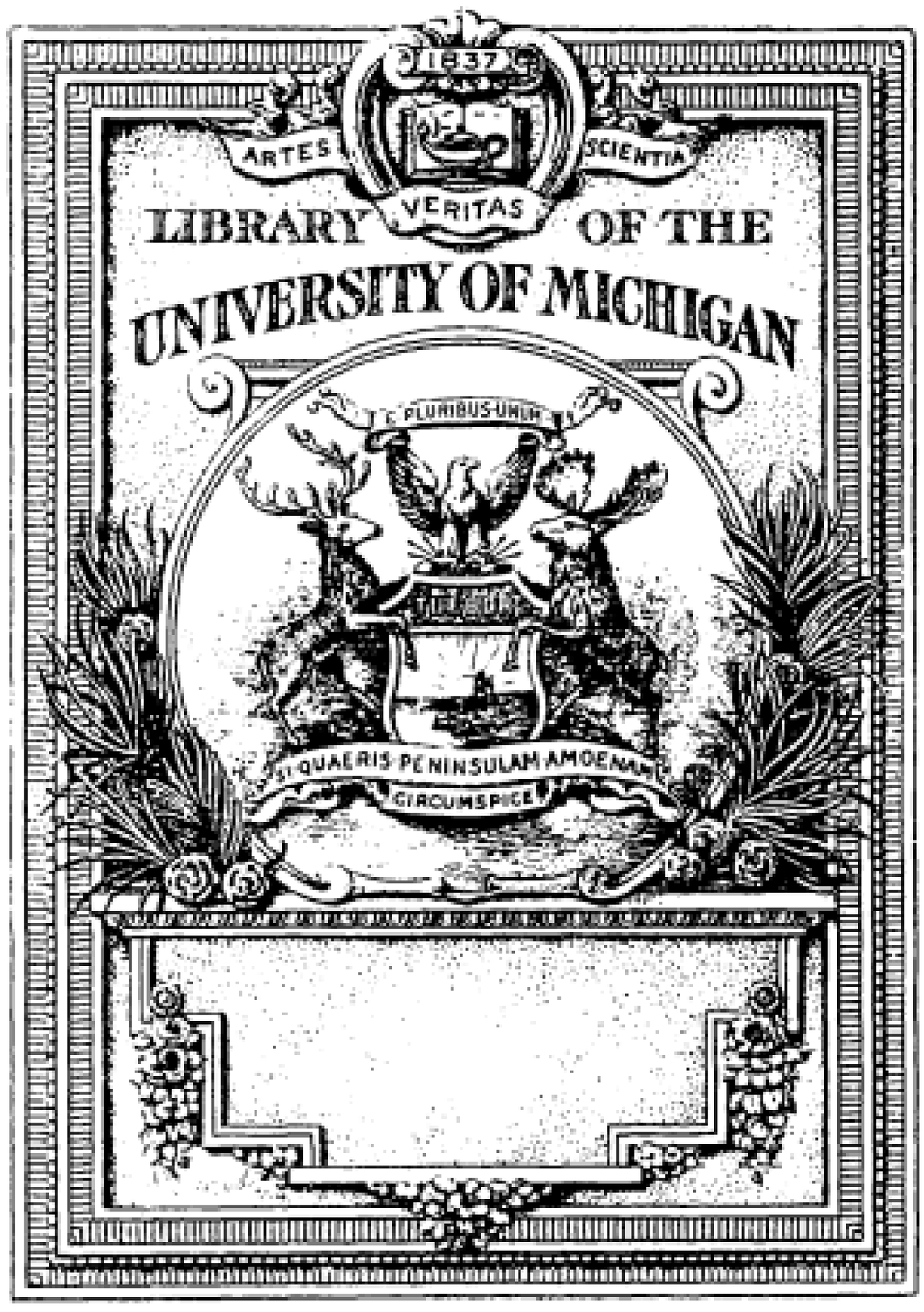

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TO

MEDICAL LITERATURE.

BY

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PREFATORY NOTE.

In selecting the following essays for republication, I have avoided all of a merely controversial character. The first one in the volume may, indeed, be considered somewhat an exception to this rule, but it treats rather of the principles involved, in what in Scotland would be called, the "great ganging plea," between the old and new schools of medicine, than attempts a full argument of the questions at issue.

The papers were all written for the 'British Journal of Homœopathy,' with which I was editorially connected for the first fifteen years of its existence, and which continues its career under the management of my late esteemed colleagues Drs. Drysdale and Dudgeon, whose labours for the advancement of the Art of Medicine will only be fully appreciated by its future historian.

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CONTRIBUTIONS

TO

MEDICAL LITERATURE.

CHAPTER I.

YOUNG PHYSIC.

THERE is a point in the development of a complex science, such as medicine, at which it seems to be overburdened by the multitude of its facts and the insufficiency of its generalizations. When it has reached this stage, and come to a dead lock, it gets no aid from those who have the reputation of being its most successful cultivators. For the very endowments which gave them renown at the time when the science required merely the accumulation of materials, or the critical selection of facts and their lucid arrangement, disqualifies them from the higher task of grasping the subject as a whole, and viewing it by the light of general philosophy, discovering the true centre from which it must be re-organized. This great achievement requires more than the highest powers of analytical acumen, it requires

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the intuition of synthetic genius. And when the voice of the great discoverer, who is destined to renovate his department of knowledge, makes known the central truth, there is none on whose ear it falls more repulsively than on that of the high-priest of the special science itself. Great discoveries belong to philosophy rather than to science; and no class of minds is less prepared to receive them than are those who have devoted themselves to the study of scientific details alone.

There is nothing more strange or more lamentable than the total absence of philosophical discernment displayed by those who are now the appointed guardians of medicine. While deploring its present uncertain and unscientific character, they do not seem at all aware of the real cause of its poverty, and propose remedies which would be as useless if got as they are impossible to obtain. Their inability to perceive what medicine stands in need of, arises from the same cause as their rejection of Homœopathy. If there be any thing more striking in Dr. (now Sir John) Forbes' memorable article[1] than this, that throughout the whole of it he speaks of theory in medicine as if it were quite an unimportant thing, and as if the whole end of the labours of the medical philosopher were attained if he made himself

[1] "Homœopathy, Allopathy, and Young Physic," first published in the 'Brit. and For. Med. Review,' of which Dr. Forbes was editor, it has since grown into a book, now in its second edition, entitled, 'Nature and Art in Disease.' The article well deserves the epithet "memorable," for it has produced a greater impression upon the mind of the medical profession than anything of the kind during the present generation. By many the writer was looked upon as having betrayed the secrets of the confessional, and was denounced with such clamour as to make him resign his editorial post; by others he was applauded; by all his influence was felt.

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sure of a sufficient number of facts, it is the constant recurrence of the expression "philosophic practitioner," —whose philosophy seems to consist in doubting much and in doing nothing. Unsparingly as he condemns Homœopathy as tending to degrade physic by making its practitioners artisans, the position he would appoint to medicine is singularly opposed to the usual requirements of an art and a science. Science is certain knowledge, giving the power of prediction; art is the application of that knowledge. But his science consists in knowing that we can know nothing, and the art he recommends us to practice is the *dolce far niente!* He speaks of Homœopathy as "an ingenious system of medical doctrine, tolerably complete in its organization, tolerably comprehensive in its views," and says that "it is as good and rational a theory as most of our medical theories." With the great leader of philosophic practitioners it seems a very light matter whether a theory be right or wrong. He seems to regard it very much in the light of the shell of an oyster, useful only for containing a nutritious body of facts. It is not surprising that, considering theory to be so very insignificant a matter, he lightly observes, with an inconsistency which might be culpable in one who took a more serious view of the value of theory, "that we may indeed have sufficient proof to satisfy any reasonable mind that the theory, or doctrines, or principles of Homœopathy are false," although he does not enter into any such proof, but waives it till a more convenient season. He turns away contemptuously from the consideration of the theory, doctrines, or principles, and eagerly asks what the *facts* are worth.

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The facts arrest and startle him. He finds that under the guidance of this theory practitioners of medicine restore to health more patients than under the orthodox method, which disdains, as an infringement upon the liberty of the faculty, to acknowledge obedience to any theory whatsoever. This fact of the greater success of Homœopathic treatment, is a hook in the nose of Allopathy, which must in some way or other be extracted. The problem is this:—By the most authentic returns, more cures are effected in certain dangerous diseases under Homœopathy than under Allopathy; how, then, is the inference to be prevented that, therefore, Homœopathy is good, and the position to be established that it is radically bad? This is done in the simplest and most ingenious way. Good and bad are relative terms: it is the same thing to say that a man is better than a monkey, as to say that a monkey is worse than a man. If we say Homœopathy is better than Allopathy, we suppose some good in the former—more good at least than in Allopathy. But we have only to say Allopathy is worse that Homœopathy to get rid of the dilemma. And this is the position our great antagonist assumes! But it is urged, if Homœopathy be bad, and Allopathy still worse, how do you retain your allegiance to Allopathy? To this he answers: it is bad *in esse*, but good *in posse*; it contains the germs of its entire renovation, and it only requires proper attention to foster the latent rudiments of good into vigorous growth. How comes it, a sceptic might inquire, that you, who have so long held a chief place in the cabinet of Medicine,—you, her appointed, acknowledged, respected minister, should, till now, never have whispered your be-

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lief that the system you have been directing requires a thorough reformation? How is it that this momentous truth has been forced from you by the doings of so insignificant a body as the Homœopathists? Surely, in the history of our art, among the triumphs of Homœopathy will be recorded,—"It forced Dr. Sir John Forbes to give his candid opinion of the state of medicine. It forced him to make confessions during his life, which otherwise he might have reserved for his testament."

But let us follow out the process of Young Physic, and see exactly what it is, and to what it leads:—Allopathy and Homœopathy are both bad. In their hospitals many patients die, although more recover. The average mortality in both is much alike; the kind of diseases which kill them is much alike. It is plain that, as there is no great difference in the amount of recoveries under the two systems, they must both derive their efficacy from the same cause. Now, as the methods of treatment are wholly opposite, the benefit must accrue from something beyond or outside of the treatment altogether. The only thing beyond that, common to both, is the natural power of recovery. Here, then, is the real explanation of the cause of the equality in the mortality and recovery in the two sets of hospitals:—that those who are able to stand the treatment and the disease, get well; and those who are unable, die. The practice of medicine (*medendi*, curing) turns out to be nothing. All that the physician can do, is to open his wards and see fair-play between Nature and Death. If the struggle between them end in favour of the latter, he has the satisfaction of confirming his diagnosis; if in favour of the former, of dismissing his patient.

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Is it, then, really come to this—that, after two thousand years of observing disease, we are no further advanced in its treatment? In what respect is Young Physic superior to Hippocrates? They both advise the same thing: Watch, say they, the progress of disease, but do not interfere. Young Physic, however, remarks, that all who have deviated from this rule have done mischief. All that his greater experience has taught him is, to have greater confidence in the canon of Hippocrates. How long is this new cycle to run? Are we to stand, like sentinel-stars, for other two thousand years, silently watching the course of disease, while ever and anon an eccentric man of action suggests some practical innovation, till, at the expiry of this double millennium, another Forbes arises, and with his critical crowbar demolishes the systems of these practical men, and dooms his fellow-mortals to endure all the pangs of sickness, with nothing but the consolations of philosophy to alleviate them? In short, what is the germ of Allopathy on which Dr. Forbes rests his hopes? The only positive recommendations we can find are—carefully to watch the progress of disease, to employ all proper hygienic measures, and *to use the numerical method in noting down the effects which have been or may be observed to follow the administration of the medicines.* The first two recommendations, however excellent, can hardly be considered as at all differing from those given by Hippocrates; and as Dr. Forbes evidently expects much good to accrue from the last, it is to it we shall now direct our sole attention: and if we can establish that the only results of such a method are increasing perplexity, rank error, or total disbelief in the powers of

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medicine, we must acknowledge the incapacity of Young Physic to be of any use,—except in the way of a pioneer.

"*Life is short and the art is long; the opportunity quickly pàsses; experience is fallacious, and decision difficult:*"[1] how, then, can any man, in the short term of his life, discover for himself what remedies are certainly useful in the all but infinite variety of diseases he has to treat? Suppose even that he has the advantage of a large hospital, is it possible for him to make experiments in such a way as to arrive at certain knowledge? The Young Physic school, which prides itself upon its philosophy, recommends the use of the numerical method, that, by noting down the result of each medicine in each form of disease, we may at length arrive at positive data to direct us in future cases. Let us inquire what philosophers of established reputation think of this plan.

M. Comte scouts with the same severity as Sir John Forbes, all ontological speculations, and founds his system on the ascertainment of positive phenomena alone, leaving the mystics to pry into the causes of these phenomena. He has, moreover, a profound respect for mathematical science, places it as the first and fundamental science in his scheme of human knowledge, and is most anxious to carry the numerical spirit as far into general science as it will legitimately go. He would apply the method of numerical notation to every department of human knowledge, where it could be of any avail. Surely if any philosopher of note is likely to countenance Young Physic's brave attempt to reform medicine by introducing into it the accuracy of arith-

[1] Hippocrates' first Aphorism.

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metic, it must be M. Auguste Comte. His opinion is given in the following words:

"*Indeed the spirit of calculation tends in our day to introduce itself into this study,* (Physiology) *especially into that part of it relating to medical questions, by a much less direct method, under a much more specious form, and with infinitely more modest pretensions. I wish to speak of that pretended application of it which is called the statistics of medicine, from which many savans* (*e. g.*, Young Physic) *expect wonders, and which, from its very nature, can lead only to profound and direct degradation of the medical art,* (*reduced by it to a blind enumeration.*) *Such a method, if we may be allowed to call it by the name of method at all, cannot, in reality, be any thing else than absolute empiricism, disguised under the frivolous garb of mathematics. Pushed to its extreme logical consequences, it will tend to make all rational medication radically disappear from medicine, by conducting the practitioner to make chance trials of certain therapeutic measures, for the purpose of noting down with minute precision the numerical results of their application. It is evident, on principle, that the continual variations to which all organism is subjected, are necessarily even more pronounced in a pathological, than in a normal state, as a result of which, the cases must be even less exactly similar, whence results the manifest impossibility of making a judicious comparison between two curative methods derived from data furnished by statistical tables alone, independent of some sound medical theory. No doubt some direct experimentation, restrained under proper limits, might be of great importance to medicine as well as to physiology, but it is precisely under the strict condition that it shall never be simply empiri-*

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cal, but that it shall always attach itself either in its institution or in its interpretation to an entire system of corresponding positive doctrines. (A l'ensemble systematique des doctrines positives correspondantes.) *Notwithstanding the imposing aspect of the forms of exactness, it would be difficult to conceive of an opinion in therapeutics more superficial and more uncertain than that which rests solely on the easy computation of fatal and favorable cases, to say nothing of the pernicious practical consequences of such a manner of proceeding, where one could not beforehand exclude any kind of attempt.*

"*It is really deplorable that geometricians have sometimes honoured with some kind of encouragement, such a profoundly irrational aberration, by making vain and puerile efforts to determine, by their illusory theory of chances, the number of cases sufficient to make these statistical results legitimate.*"[1]

We must again remind Young Physic that M. Comte is one of the first of living mathematicians, and one who, more than any other philosopher, insists upon the prolongation of the mathematical method as far forward into the more special and complex sciences as it can be made to go.

There can be no greater contrast than that presented by Sir John Forbes, as a destroyer, and as a builder. Nothing can be clearer, more precise, more acute than his objections to the systems or practice of others; but when he makes his own system known, he becomes vague and obscure in the extreme, and falls into declamation about the medical profession being "grand and

[1] 'Cours de Philosophie positive,' par M. Auguste Comte. Tome 3me. pp. 418, 420.

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glorious in its essence, aims, and aspirations!" Wearied, it would seem, with the work of demolition, he sinks into the state described by Horace, when he says, "*Auditis? an me ludit amabilis insania?*" and in the dim perspective he sees the future progeny of Young Physic flit past like Banquo's shadowy race, but cannot catch or paint their lineaments.

The system which is thus faintly suggested in the article of Sir John Forbes is more fully expounded by Dr. A. Combe in his letter "On the Observation of Nature in the Treatment of Disease." This letter is very remarkable for its candour and high moral tone. It contains also much truth, and shows that the writer, to some extent, perceives the real cause of the backward state of medicine when he says that, "notwithstanding the ardour and success with which facts are sought for, yet, inasmuch as the ultimate facts remain unknown, the others lead to no useful result." If by ultimate fact, Dr. Combe means the largest possible generalisation, and this is the only idea that a disciple of Bacon can attach to the phrase, then we entirely agree with him, but we look upon the homœopathic principle as being that ultimate fact which he seeks. Although to a certain extent Dr. Combe feels the necessities of medicine, and truly says it is by the ascertainment of an ultimate fact, that is, a general law, that we can advance our science, yet the means by which he strives to reach the point of vantage are as vague as his general conception of what it would be, when gained, is just and exalted.

The system which Dr. Combe expounds is founded on a radical error in philosophy. His view is this:—If we observe the progress of disease, we shall find that it runs

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a determinate course; it has its origin, its growth, and its decline; in these it obeys certain fixed laws given to it by the Creator. It is for man to learn these laws—to interpret these laws, but not to interfere with them. Nature alone cures, not man. It is presumptuous in man to usurp the prerogative of Nature, and attempt to improve upon her efforts. Man is the interpreter, not the master of nature. Such seems the general drift of his opinions, which, lest we misstate them, we shall give in his own words;—"The grand object of medicine is to preserve and restore the healthy action of all the different organs and functions of the human body, so as to insure their efficiency, and fit the individual for the successful discharge of the duties devolving upon him as a created being and a member of society. Here, then, the first step to be taken is obviously to become acquainted with the mechanism of the body, the structure of its constituent organs, the conditions or laws under which these act, the purposes which they respectively serve in the animal economy, and the relations in which they stand to each other, and to the external agents by which man is surrounded and acted upon from the moment of conception down to his latest breath. In other words, the first step towards rational principles of cure must consist in a knowledge of the laws of the healthy functions. The second ought to be the observation of the manner in which the various disturbing causes act upon the different functions, and the kind, course, duration, and termination, of the morbid action which they produce. Having investigated these points, we become qualified to inquire, in the next place, what circumstances will best favour the intentions of Nature, and remove the obstacles

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which may have arisen to impede or thwart her efforts. To succeed in these aims, or even to make a rational attempt at succeeding, we must be profoundly impressed, or, I may say, saturated, with the great principle or truth, that all the operations and actions of the living body, whether healthy or morbid, taking place according to fixed and discoverable laws, and that God has left nothing to chance. With this grand fact before us, it becomes palpably evident that we can do nothing rational in the way of either prevention or cure, except in so far as we act in accordance with those laws. Many medical men have, however, a very different impression from this. A good physician will always seek to be, and never aim at being more than, Bacon's 'servant and interpreter of Nature.' A greater than he created man, and ordained the laws of his being, and no surer road can be found than that traced by the hand of his Creator. Overlooking this truth, and viewing disease as an entity, ungoverned by any definite laws, and not destined to run through any definite course, many medical men talk as familiarly of their 'curing' and 'arresting disease' as if they had an absolute control over the whole animal functions, and could alter their laws of action at pleasure. To my mind, no clearer proof of presumption and philosophic ignorance can be found than this usurpation of the prerogatives of Deity; and its results are often very unsatisfactory,"

The great error contained in this view consists in confounding the so-called laws of Nature with the laws of a moral Creator. The tap-root of the false school of philosophy to which Dr. Combe seems to belong, and which has recently been fully expounded in many popular works,

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is confounding the real obligation of man to obey the moral laws of his Creator with his assumed submission to the laws of the creation. The so-called laws of the creation have no existence out of the mind of man. They are but the summary expressions of his knowledge; they are but the ultimate facts or laws he has arrived at. "Ultimate laws," says Mr. Mill, "are observed uniformities of Nature which cannot be resolved into more general laws."[1] All that they express, in any case, is the constant relation of certain facts to certain other facts. This is all that gravitation expresses. Because gravitation is a law of Nature, would any sane man argue that therefore it ought not to be interfered with? What is the whole active life of man, but a struggle with this great law of Nature? What is death but a sinking under it—becoming obedient to it—being let down, pulled down, we might say, in submission to this grand natural law? It cannot, then, be argued that because a process occurs in obedience to a law of physiology or pathology, that on that ground alone we ought not to interfere with it. It must be shown that it would be injurious to the patient if we did. This, which is the only real point at issue between us and the Naturalists, Dr. Combe does not attempt to touch. He shows, indeed, that in certain cases it is very hazardous to use the ordinary depleting measures; but he does not even attempt to show that there are not, or may not be, means which are innocuous, yet potently beneficial. He does not show that there may not be a science of therapeutics, or curing; but he implies, from the position he at first assumes, that there cannot.

[1] 'Mill's Logic,' vol. ii, p. 3.

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Miserable as are the conclusions Dr. Combe arrives at, we believe they are the logical consequences of working out Sir John Forbes' system; and for this reason we attach much value to his letter, independently altogether of the high and generous tone which pervades it, and which gives great weight to its influence over others. If ever Medicine suffer utter degradation, it will be brought about by means of this new school. The leaders of the profession are engendering a scepticism in Medicine which, unless counteracted, will blight it to the core. Practitioners of physic will become divided into two classes,—the one believing nothing, but yet acting as if they did, and giving the countenance of their high talents and acquirements to the rankest hypocrisy; the other class believing anything or everything, but having no substantial grounds for their belief: cold, cultivated sceptics will be the aristocracy, and ignorant but energetic dupes of their own credulity the democracy, of this noble and glorious profession. No wonder that one of Sir John Forbes' correspondents exclaims, "What more melancholy fact can be presented to the mere prescriber, when he first enters upon the duties of his benevolent profession with the enthusiasm of unsoured philanthropy, than the continual assurance of the Nestors of the profession, that the greater our experience the more positive our conviction that we can *do* nothing? And it only proves the immense force of habit that, with such convictions, we do not see men quit a profession which, under such circumstances, requires a constant exercise of hypocrisy and a constant sacrifice of principle."

While the great majority of those who have expressed their opinions on the state and prospects of medicine

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agree in the main with Sir John Forbes and Dr. Combe, yet a few avow their dislike at being thus consigned to the negative pole of usefulness. The best exponent of the views of this class seems to be Dr. Bartlett, who says, "the seat, the character, and the tendencies of the disease being known, the next thing to be done is to find out the best means of preventing, modifying, and of curing it." To do this he goes on to say, "we must know the effects and influences which all substances and agencies in nature are capable of producing upon it, and this we can know only by direct observation of the effects themselves." That is, we suppose medicine improved by a system of experimentation with individual drugs upon individual diseases. This is what another writer likewise proposes. Let us again apply to some philosopher of acknowledged reputation, to ascertain what may be expected from such experiments when not conducted in the light of some previous theory.

It most fortunately happens, that the very case in point is used as an illustration by Mr. J. Stuart Mill, in his work upon Logic. The great modern authority upon the subject writes thus:—"Let the subject of inquiry be the conditions of health and disease in the human body, or, for greater simplicity, the conditions of recovery from a given disease; and in order to limit the question still more, let it be confined, in the first instance, to this one inquiry,—Is, or is not, a particular drug, Mercury, for example, a remedy for that disease? . . . . When we devise an experiment to ascertain the effects of a given agent, there are certain precautions which we never, if we can help it, omit. In the first place, we introduce the agent into the midst of

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a set of circumstances which we have exactly ascertained. It need hardly be remarked how far this condition is from being realized in any case connected with the phenomena of life; how far we are from knowing what are all the circumstances which pre-exist in any instance in which mercury is administered to a living being. This difficulty, however, though insuperable in most cases, may not be so in all; there are sometimes (though I should think never in physiology) concurrences of many causes in which we yet know accurately what the causes are. But when we have got clear of this obstacle we encounter another still more serious. In other cases, when we intend to try an experiment, we do not reckon it enough that there be no circumstances in the case the presence of which is unknown to us, we require also that none of the circumstances which we do know of shall have effects susceptible of being confounded with those of the agent whose properties we wish to study; we take the utmost pains to exclude all causes capable of composition with the given cause; or if forced to let in any such causes, we take care to make them such that we can compute and allow for their influence, so that the effect of the given cause may, after the subduction of those other effects, be apparent as a residual phenomena. These precautions are inapplicable to such cases as we are now considering. . . . . *Anything like a scientific use of the method of experiment in these complicated cases is therefore out of the question. We can, in the most favorable cases, only discover, by a succession of trials, that a certain cause is very often followed by a certain effect.*" [1]

We find, then, that logic as unceremoniously discards the

[1] 'Mill's Logic,' vol. i, p. 529.

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experimental method proposed by Dr. Bartlett as philosophy condemned the statistical method sanctioned by Sir John Forbes. Indeed, the two methods are very nearly allied, the one almost involving the other; and if they both be abandoned, and if young physicians be not content with being the mere spectators of disease and superintenders of diet and ventilation, and revolt at the idea of hospitals for the cure of the sick being nothing but museums for the study of morbid natural history, and registries of mortality, to what side are they to turn for escape from the alternatives presented to them? How shall they be followers of nature and yet energetic? How shall they know all that is to be known, and not be sceptical; and do all that is to be done, and not be dangerous?

If the simple observation of the natural course of disease; if the application of the numerical method to all recorded cases; if experiments with various medicines upon various diseases, are each and all fallacious modes for the improvement of medicine, what conceivable method yet remains by which it may be raised from its present uncertainty to become amenable to known laws, by which it may be worked? The answer to this we have already anticipated, at the commencement of our article. The accumulation and complexity of facts in medicine rendering the inductive process inapplicable, it is absolutely necessary, for the regeneration of the science, that some successful hypothesis be made which shall express the law of relation between the curative and some other discoverable property of a drug, by which we may know beforehand what we are to select in a given case of disease. There has been but one such

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attempt in medicine, (for we need not stop to show that the old Galenic maxim, "*Contraria contrariis opponantur*," is not such an hypothesis, involving, as it does, the previous ascertainment of conditions as impossible to recognise as they are incapable of being tested ;) that is, *that medicines tend to cure diseases similar to those they tend to produce*.[1] This is a perfect hypothesis for the circumstances ; because it embraces all the circumstances coming within the range of medication, and it is possible to establish the truth or falseness of it by experiment. How vain and ignorant it is of those who profess themselves philosophic practitioners, to reject this proposition, and to stigmatize the practice thence flowing, on the ground of its being at the first hypothetical, is shown from the fact that all complex sciences have become perfect by means of some successful hypothesis. What was Kepler's law of planetary motion, which has introduced such accuracy into astronomy that the accession of a new member to our system can now be infallibly predicted, but an hypothesis verified by calculation? What was Dalton's atomic theory, which has given such systematic beauty to chemistry, but an hypothesis verified by calculation? What was Torricelli's grand discovery of the pressure of the atmosphere, which opened a new era in physics, but an hypothesis verified by an experiment? It is the same with Hahnemann's great law of medicine —it is an hypothesis proved true by its results.

Had Sir John Forbes and his followers been alive to the necessity of such a discovery, had they perceived

[1] We believe this is the most accurate way of stating the law of homœopathy, and it would obviate many objections if this mode of expressing it were generally adopted.

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that *it was not a Bacon, but a Newton, that medicine stood in need of*, they would have formed a very different estimate of the grandeur of homœopathy; they would not have harped with puerile pertinacity upon the assertion that Hahnemann was mistaken in supposing that the symptoms of ague he felt stood in relation of cause and effect to the doses of Cinchona bark he had previously taken. They would have perceived that it was of very little consequence what gave rise in the mind of Hahnemann to an hypothesis which, if substantiated, would make medicine ever afterwards a deductive science, and not an empirical one; and they would have directed all their energies to ascertain whether this hypothesis be true or false; for if true, it must change the whole character of the art.

There were three possible ways open for disproving the truth of homœopathy: first, by showing that it was opposed to some already established natural law; second, by showing that the facts on which it rested its claims for belief were either false or too few; third, that when tried as a guide in practice, it led to failure.

The first way was never attempted. No one has yet ventured to assert, that the proposition that a medicine tends to cure diseases similar to those it tends to excite, is radically opposed to any general fact. On the contrary, were this the place for it, it would not be difficult to demonstrate, that this primary law of homœopathy presents many striking and interesting analogies with some of the most important doctrines that have recently been advanced by the greatest writers in the various fields of physics, ethics, and psychology.

Neither Sir John Forbes, nor any of his school, have

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devoted themselves to the task of undertaking the second way, and showing that there is not a marked similarity between the curative and noxious effects of very many drugs. Nay, Sir John Forbes himself admits, in these words, that there is such a resemblance. "Indeed, it is supported by several strong analogies, afforded both by pathology and (allopathic) therapeutics."

The only other way which remained was to show, that where this law had been applied for the cure of diseases, it had entirely failed to effect its object. How far our opponents have succeeded in this, we leave those to determine who have carefully and critically studied Professor Henderson's letter to Sir John Forbes, as well as the evidence from which his arguments are drawn.

Hitherto we have contemplated homœopathy as an abstract scientific truth, "won from the void and formless infinite," by the genius of its discoverer; a truth which would remain the same, although disease were to disappear, and there were no occasion for its application to human affairs; and we have considered the reason why the value of this truth has not been appreciated by the foremost men in our profession. But between such an abstraction and the requirements of daily life, there lies the province of art. The truth might be revealed, but might remain for ever unprofitable to us, if we were not instructed in the mode of its application. Art is to science what action is to thought. The discovery of the law of homœopathy made medicine as a science perfect; it required another discovery before an element of perfection was imparted to medicine as an art. Discoveries in art are of a wholly different kind from those in science.

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They are simply empirical rules obtained from observations or experiment, and proved true by experience. They never can have the same absolute value as fundamental scientific principles, nor are they susceptible of the same amount of proof. The rules of art occupy, as it were, a middle point between science and its application. They rest upon a surface of fluctuating observations, and they derive their coherence and stability from a source different from that of their origin. That source is the abstract scientific truth to which they are united. In estimating the value of these rules of art, it is necessary always to keep in view this their double connection; and it would be as unfair to judge of them, separated from the scientific principle which gives them support, as it would be to insist upon a child living in its mother's womb after the umbilical cord had been divided. This is the injustice which has been practised upon the rule of art connected with the science of homœopathy;—the rule that medicines when administered in accordance with the principle "*similia similibus*," should be given in minute quantities. If the proposition had been advanced that medicines, in doses infinitely minuter than had ever hitherto been imagined, were capable of curing diseases, as a simple fact of observation, it might fairly have been met with the ridicule that has assailed it. But when advanced under the protection of a more general proposition, it ought to have been always viewed as related to this general fact; and in that light, whether it be true or false, it ceases to be ridiculous.

While, then, we charge Sir John Forbes with committing a fundamental error in philosophy, by not appreciating the necessity of some general hypothesis by which

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the science of medicine may be made deductive, we charge him and his followers with a second error in judging of a rule of art without taking into consideration the cognate scientific principle from which it derives half its force. This misapprehension of the proper point of view, from which the system of giving small doses ought to be regarded, on the part of all English writers, is the less excusable, since those who have adopted their side of the controversy in Germany, have admitted and given full prominence to the fact, that the dose must vary according to the principle by which the medicine is selected. "On the other hand," observes Dr. Jörg, "medicines operate most powerfully upon the sick when the symptoms correspond with those of the disease. A very small quantity of medicinal arnica will produce a violent effect upon persons who have an irritable state of the œsophagus and stomach. Mercurial preparations have in very small doses given rise to pains and loose stools, when administered in an inflammatory state of the intestines. Yet why," he exclaims, "should I occupy time by adducing more examples of a similar operation of medicines, since it is in the very nature of the thing that a medicine must produce a much greater effect when it is applied to a body already suffering under an affection similar to that which the medicine itself is capable of producing?"[1]

Another great error which the various writers upon the subject have fallen into, when treating of minute doses, is confounding the amount of force required to originate a series of changes in the animal system, and that which

[1] Materiellen zu einer künftigen heillmitellehre durch versuche der Arzneien an gesunden Menschen gewonnen und gesammelt von Dr. Johan C. G. Jörg, p. 16.

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is required to modify those changes where already going on. It is, if we may so express it, applying to a question of dynamics, calculations derived from data afforded by statics. Contented as the disciples of Young Physic are with allowing morbid processes to proceed without any intervention, and simply supporting the powers of life, that the system may not sink under the continuance of the disease; and accustomed as they and all allopathic physicians are to look upon therapeutic agents as operations beyond, or outside of the actual morbid forces, (the derivative system of medicine,) they do not perceive that if it be possible to introduce a force within the actual sphere of diseased action, that force will be incalculably intensified in its operation by acting on other forces already in a state of preternatural and violent activity. A breath of air will deflect an arrow from its course, although shot from the bow of Apollo. We do not intend to dwell upon the point last mooted, partly because our space is exhausted, and partly because it has already been fully and ably handled by various writers, especially by Dr. Henry Madden, in a work entitled 'Reformed Medicine.'

Before we close this fragmentary paper, the design of which has been rather to discover, if possible, and to indicate the chief sources of the errors of our opponents, and in this very search to point to the refutation of those errors, than to expose, by argument, the numerous fallacies which have marked their winding course throughout the controversy, we wish to do full justice to Sir John Forbes. We look upon him as the ablest exponent of the sceptical era in medicine. He expresses for the Medicine what others have done for the Philosophy of

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our period, which has been characterised as "an age of unbelief, and yet afraid of scepticism." By giving expression to prevailing scepticism in medicine, Sir John Forbes has rendered an immense benefit to the profession; *he has brought out and made curable the hitherto latent psora.* The element of faith which he would fain mingle with his confession of general unbelief is too foreign to the rest of the system to have any influence upon his disciples. The reform he has so powerfully advanced will soon become a revolution beyond his power to control. His influence for the future will be purely destructive. But destruction of the bad must precede construction of the good; and for having with great talent and boldness attacked and shaken a dynasty whose speedy termination is with certainty predicted by this revolt of its chief supporter, as the doom of the Corn Laws was pronounced by the leader of the party which had maintained them—for having rendered this service, and opened the gate of scepticism for the admission of truth, we feel deeply indebted to him, and we have no doubt that his school will prove to many the halting-place between old physic and homœopathy; and the title of his great article should be, not 'Homœopathy and Young Physic,' but HOMŒOPATHY *via* YOUNG PHYSIC.

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CHAPTER II.

ON DIET.

WHETHER it be true, as some historians affirm, that Napoleon lost the battle of Leipzic owing to his having eaten a bad dinner, is certainly questionable; but there can be no doubt whatever that bad dinners and bad food generally are at the bottom of much domestic and national misery and disaster. And there is no subject which more imperatively demands special attention than the one we are about to enter on. For from the promulgation of the system founded and taught by Hahnemann, numerous specialities in diet have been inculcated with more or less rigour by himself and his followers, and so prominent have these innovations become to the eye both of the public and the medical profession, that while the former frequently seem to suppose that if they only consume a sufficient quantity of homœopathic cocoa, they will be entitled to all the benefits of the new system of medicine, the latter discovers with its usual sagacity, that although the success of our treatment is beyond dispute in very many cases, yet that we owe our superiority, not to the total difference of the principle according to which we select our remedies, but to our better judg-

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ment in the regulation of the diet of our patients; although the subject of diet has been one carefully studied and ably handled, since the time of Hippocrates, and although by this assumption our opponents pay us the highest possible compliment, by yielding us the palm of victory in the field common to us and them. In fact if they are right in this explanation, it would be tantamount to saying, that as there is no law by which we profess to be guided in our choice of food analogous to that which directs us in the choice of the proper medicine, each individual who practises homœopathy displays a higher amount of intelligence than his allopathic brethren, and is in their sense of the term the better physician of the two. This is a fatal argument for young physic, which professes rather to cure diseases by diet and regimen, than by medicines. They acknowledge themselves thrown in their own ring: how strange is their inconsistency! It is considered disgraceful to admit the superiority of homœopathic medicine, a novelty which old physicians might pardonably plead ignorance of, and they prefer to proclaim their inferiority in the only branch of the physicians' art on which they build their reputation; while on the other hand we find among ourselves a pertinacious adherence to certain dietetic formulas, which rest on no authority but that of accidental prejudice, and which derive no support from the enlarged experience that we have now within our reach.

There are but two methods by which we can hope to arrive at any thing like unanimity in our dietetic creed; the one is to assume Hahnemann's writings as our Leviticus, and to ban all he banned, and admit all he admitted; or to inquire into the principles by which he

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was directed in the rules he lays down, and to follow out these principles, if they be sound, into their full development by all the lights which modern science and observation have shed upon the subject.

As Hahnemann did not assume the infallibility of inspiration, we may safely decline accepting any Leviticus at his hands, and examine with that care and respect which all his writings deserve, what they contain upon the subject of diet, and we shall be satisfied that if in some instances he was led into extravagance and error, yet that the principles of his doctrine are eminently sound and sagacious.

In endeavouring to form a just valuation of Hahnemann's dietetic rules, we must bear in mind that before he had discovered his grand formula for the selection of medicines, he had already written extensively upon various topics connected with diet and regimen, and that he carried over into the new province of homœopathy, where he so long ruled supreme, some of the old adopted notions which he had already too vehemently expressed to admit of his recanting them. The most striking illustration of this was his antipathy to the use of coffee. "The cold, considerate earnestness of our forefathers," such are Hahnemann's words, "the firm stedfastness of will, of resolve and of judgment, the endurance of continued and yet powerful exertions of the body, adapted to the object in view, that used to constitute the original national character of the Germans, the whole sublime stamp of our descent disappears before this medicinal beverage, and changes into over-hasty disclosures, hurried resolves, immature judgments, frivolity, changeableness, talkativeness, irresolution, too

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easy mobility of the muscles without any endurable result, and theatrical behaviour."[1] The extravagance of this passage makes it read like a temperance lecture. In fact what truth is in it is certainly applicable to the abuse not the use of coffee. It would be interesting if we could discover the origin of this excessive hostility to coffee, which belongs to the family of narcotics, and is a sort of foster-brother of tobacco, that "foul and loathsome weed," as an irreverent lecturer calls it, which Hahnemann is said to have loved, if not wisely, at least too well. It may have been an idiosyncratic dislike, or more probably he fell into a common opinion of the time in which he wrote, and which was strongly shared by the philosopher Kant,[2] and by many French physicians, that coffee was an injurious beverage, and he afterwards rested his opinion upon the basis of unsound experiment. That it was an unsound deduction from the effects which follow the use of coffee when taken in a way to produce derangements in the system, to forbid its use in all circumstances, is obvious, if we reflect that we could all make ourselves as ill as many of our patients, if we were only to take dinner at time of breakfast, and tea at dinner time for a few days.

We cannot, however, dismiss the subject of experiments in diet, without some further remarks. And the first is, that although in the particular example we have quoted, Hahnemann committed a great mistake, yet that on the whole he is perfectly sound, and does not seek to lay down any positive rules about diet, but acknowledges that food must be regulated by the special appetites and

[1] Lesser writings, Dudgeon's translation, p. 458.
[2] See De Quincy's 'Last Days of Kant.'

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instincts, as well as circumstances of those who take it; and he gives in his 'Friend of Health' several illustrations of the superiority of the popular over the scientific judgment upon this point.

In the whole of his intellectual career he seems to have been looking for some law of relation between diseases and their curative specifics, but nowhere does he hint that there can be any such law to guide us in the choice of food; and as an obvious corollary to this tacit conviction, while he from the first insists upon the necessity of instituting a series of rigid experiments to discover the effects of a drug upon the animal economy, he nowhere suggests that similar experiments should be made in regard to food. He seems to have recognised the important difference between that kind of knowledge which we reach by experiment, as in chemistry, and that other kind which we gain by observation, as in geology, where nature has made the experiments for us, extending over incalculable surfaces of space and periods of time. The feeding of the human race has been going on in every conceivable circumstance for many thousand years. In the language of Schleiden "The Gaucho who in the wild pampas of Buenos Ayres, managing his half-wild horse with incredible dexterity, throws the lasso or bolus to catch the ostrich, the guanacho or the wild bull, consumes daily from ten to twelve pounds of meat, and regards it as a high feast day, when in any hacienda he gains a variety in the shape of a morsel of pumpkin. The word bread does not exist in his vocabulary. The Irishman, on the other hand, regales himself in careless mirth on his potatoes and point, after a day of painful labour—he who cannot help making a joke even of the name he

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gives to his scanty meal. Meat is a strange idea to him, and he is happy indeed, if four times a year he can add a herring to season the mealy tubers. The hunter of the prairies lays low the buffalo with sure bullet, and its juicy, fat-streaked hump, roasted between two hot stones, is to him the greatest of delicacies: meanwhile the industrious Chinese carries to market his carefully fattened rats, delicately arranged upon white sticks, certain to find a good customer among the epicures of Pekin; and in his hot smoky hut, fast buried beneath the snow and ice, the Greenlander consumes his fat, which he has just carved, rejoicing over the costly prize, from a stranded whale. Here the black slave sucks the sugar cane and eats his banana—there the African merchant fills his wallet with sweet dates, his whole sustenance in the long desert journey—and there the Siamese crams himself with a quantity of rice from which a European would shrink appalled. And wheresoever over the whole inhabited earth we approach and demand hospitality, in almost every little spot a different kind of food is set before us, and the daily bread offered in another form."[1]

After perusing such a passage as this, we naturally ask, what conceivable advantage could the world derive from the experiments of Dr. Stark, who contrived to kill himself in about six months, by a succession of simple dishes?

Before his promulgation of homœopathy, Hahnemann, with the exception of his disapproval of coffee, did not differ in his views on diet from other writers of his age and country. "There is not," he says, "and cannot be

[1] The Plant—a biography, in a series of popular lectures, by M. J. Schleiden, Professor of Botany in the University of Jena, &c.

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anything, which, as a general rule, is absolutely wholesome, or unwholesome, * * * * None of the general maxims of the dietist can be accounted good: such as, veal is the most wholesome butcher's meat, &c. * * * * 'There is a time for everything, says Soloman, and to my mind he speaks much more sensibly than most of the dietists."[1] There is much more to the same effect in his early writings.

It is a very striking fact, that whereas, before he had discovered and taught the great law of healing, his position in regard to medicine, was very similar to that of young physic of the present day, and he relied much upon sanitary and dietetic rules; after he had made good his grand discovery that the obstacles to certainty in medicine were not insuperable, the power of diet as a method of cure, and its general importance seems to have assumed a very insignificant value in his eyes. What before had occupied whole treatises, now shrinks into this solitary observation in the 'Organon,' (p. 308.): —"Considering the minuteness of the doses necessary and proper in homœopathic treatment, we may easily understand, that during the treatment everything must be removed from the diet and regimen which can have any medicinal action, in order that the small dose may not be overwhelmed and extinguished, or disturbed by any foreign medicinal irritant." In a note he adds, "Coffee, fine Chinese and other herb teas, and many other things ought to be avoided by patients, as they should avoid excesses in eating and drinking, sitting up long at night, damp rooms, penurious living, and so forth." The note concludes with the following remark.

[1] Lesser Writings, page 227.

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"Some of my disciples seem needlessly to increase the difficulties of a patient's dietary, by forbidding the use of many more tolerably indifferent things, *which is not to be commended.*" Here we have the common sense rule laid down, that patients are to avoid things hurtful to themselves, and likely to interfere with the operation of a minute dose of medicine.

On this note of things to be avoided, have been built, with a somewhat slavish adherence to the text instead of the spirit, the majority of those homœopathic diet tables, which are in such large circulation, although some of the most acknowledged authorities have expressed an opinion in favour of a more liberal interpretation of Hahnemann's doctrines upon this subject. Thus Hartmann remarks,[1] "Every intelligent physician will at once perceive, that every patient cannot be subjected to such a rigorous diet, and that a man of sixty years old, for instance, who had been in the daily habit of taking coffee, tea, wine, brandy, or of smoking tobacco and using snuff, cannot be suddenly deprived of those things without detriment to his organism. Hahnemann allowed smoking and snuff; he forbade wine, brandy, and coffee with great severity. This seems inconsistent, and leads to suppose that in a more advanced age we may except some other things from the general rule of abstinence." While in this country[2] there is undoubtedly a disposition to enlarge the table of our patients, in America our energetic friend Dr. Hempel seems inclined to surpass Hahnemann in the rigour of his regulations—one of his golden rules, as he styles them, running thus:—"Avoid all stimulating drinks, brandy, beer, wine, and content

[1] 'Acute Diseases,' vol. i, p. 82.

[2] See Laurie, Chepmell, and others.

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yourself with cold water, milk, light and unspiced chocolate, weak black tea, and syrups made of currants, raspberries, strawberries, or other kinds of wholesome and unmedicinal fruits. Never use tobacco in any shape, except for medicinal purposes."[1] We suspect that beyond the confines of the State of Maine Dr. Hempel's gospel will hardly find believers in the land of liberty and tobacco. Setting aside all extravagances, the subject assumes a very simple form, and may be divided into two questions.

1st.—Is it possible for our patients to avoid all medicinal substances in their diet; and

2d.—Is it conducive to the better action of the medicine, to enforce a very rigorous diet.

At present, let us observe, we are considering the question solely in the view of its relation to homœopathic medicine, not to general health.

1st.—Is it possible to confine our patients to a non-medicinal diet? Suppose we restrict them to a prison fare, and desire them to drink a tumbler of water, and eat four ounces of bread for a meal, and repeat this three times a day. This would appear simple enough with a vengeance. We shall suppose the scene London, and just inquire into the character of the bread and water diet we had prescribed. To begin with the water, it is an incontestible fact, that, in the words of Dr. Hassall, "The waters at present in use in this metropolis are all hard, and have all the disadvantages of hard water; they are moreover river waters, and for the most part contami-

[1] Hempel's 'Organon,' p. 31.

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nated to a great extent with organic matter, dead and living; add to these points the fact of their further deterioration by contact with lead cisterns, and by the accumulation and growth of animal and vegetable productions, which take place in these receptacles, and the case is proved against the whole present supply of the metropolis."[1] It is indeed impossible to contemplate the world of monsters contained in a drop of London water, without being satisfied that a score of globules would go but a little way in affording them a satisfactory repast. Besides these living impediments to the action of our globules, and besides the lead derived from the pipes and cisterns, which will be a stronger dose of Plumbum than our rigorous dietists ever employ, there are no less than from 20 to 30 grains of inorganic matter, carbonate of lime, of magnesia, &c., in every gallon of London water.[2]

So much for our patient's morning draught of pure water—now for his slice of bread.

Dr. Hassall examined twenty-four samples of bread, obtained indiscriminately in various parts of London: his verdict is, that the whole twenty-four samples were adulterated with alum. But perhaps it may be said, the quantity is trifling; on the contrary, according to another observer, Mr. Mitchel, in ten loaves there were no less than 819½ grains of alum discovered. And the reason for the presence of alum heightens the enormity of the pollution. It is put there to enable spoilt flour to be mixed with sound flour!

[1] 'Food and its Adulterations,' by Arthur Hill Hassall, M.D., p. 52, et seq.

[2] Johnston's 'Chemistry of Common Life,' p. 38.

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With such facts staring us in the face, would it not be absurd to insist upon bread and water as a simple diet for any unhappy patient who has the misfortune to fall into our hands. Unless we could keep our patients in a conservatory, and prepare by artificial means the air they breathe, the water they drink, and the food they eat, it would be absolutely impossible to prevent them being assailed by so-called medicinal influences every hour in their lives. In fact, all nature would be in a conspiracy against them, to avenge the sentence of outlawry we should thus have pronounced upon her. This is the answer to the second question. For if we strive to attain as near as possible a supernatural immunity from the influences at all times powerfully acting upon the human organism by any method of isolation or abstinence, we thereby intensify in a corresponding degree the sensitiveness of the frame, and our patients reared in a hot-house fade away before the first breeze, which had they been of out-door growth, would have invigorated instead of destroying their tender natures.

Is there then to be nothing peculiar in homœopathic diet, and are there no rules to be enforced? The only rule, and we may enforce it with peculiar rigour, that I can conceive as universal, is, that every one should eat and drink what his own experience has taught him agrees best with him, and we on our part need not be at all afraid of the efficacy of our medicines being seriously impaired by what contributes to the well-being of the individual. It may seem very strange to order a dose of Capsicum to a man who dines upon curry. But is it more strange than ordering a dose of Calcarea to a man who drinks a thousand times the amount of the dose in

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every glass of water? These are both strange, but life is stranger still, and we are utterly unable to unravel the complex influences on which its integrity depends, and we therefore cannot understand how powerfully substances act in their curative sphere of operation which are quite inert in their nutritive. If we were to throw out a conjecture, it would be that the medicinal action of a substance resembles a distinct impulse upon the nervous system like a note of music, and as an impulse, that is purely dynamically, takes effect, whereas the same substance conveyed through the system for the purpose of nutrition, does not secure the attention of the nervous system at all. Be the explanation what it may, fortunately for the possibility of our method of practice, the fact admits of no doubt, that infinitely minute quantities of a body produce important effects, while at the same time large masses of the same are quite inoperative, and therefore it is not necessary on the theoretical ground, to insist upon abstinence from all stimulants and condiments as the indispensible initiative to a successful course of homœopathic treatment. I am aware that these views will be considered by many as somewhat heretical, and perhaps as too speculative, but although I would now rest them upon the reason of the thing, they were forced upon me slowly by my own observations in practice. It is now many years ago since I had a curious illustration of the possibility of a minute dose succeeding, even although the system was at the time being stormed at by massive ones. A patient of mine who had long been liable to megrim, was always cured by a few globules of Pulsatilla. She was also subject to ague, imported from India, which I

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could not cure. So she was put under the old system, and ordered full doses of the Sulphate of Quinine. While taking this, she had an attack of her old headache, and took a few globules of Pulsatilla, but expecting nothing from it; for I had told her, so long as she was taking her Quinine it was impossible homœopathy could do her any good. Homœopathy, however, showed itself more liberal than its practitioner, and after a single dose of Pulsatilla she got better. This case made a deep impression on my mind, and I often thought from that period that perhaps our diet code was too rigorous. Then during a long course of observation, extending over some eighteen thousand cases treated at the Edinburgh dispensary, I found that it was on the whole impossible to interdict successfully the use of tea and spirits, and that notwithstanding these infractions, and a thousand other disadvantages, the proper medicine produced its expected result with so much exactitude as to reduce to an insignificant point the deranging influence of these condemned enjoyments. And quite lately I had under my care a gentleman who took a wineglassful of Laudanum daily, and yet was as speedily relieved by the proper medicine when attacked by pain and spasms of the heart, from which he suffered in consequence of organic cardiac disease, as if he had been an absolute teetotaller.

These facts, and many more such could be adduced, ought at least to make us pause before we lay down any imperative rule against the use of coffee, tea, tobacco, and other excisable articles, as being necessarily destructive to the efficacy of homœopathic medicines. For my own part I seldom now make any change in a patient's diet out of respect to homœopathy. And with these

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observations we pass on to the more general part of our subject, and consider the principles by which we are likely to be assisted in advising our patients in reference to the most difficult affair of regulating their food and drink so as best to lead a long, healthy, and happy life.

The difficulty that meets us at the very threshold of our investigation, and which we cannot get rid of throughout its whole course, and which seems to be strangely overlooked by most professed writers on the subject of the food of man, is, that man occupies an exceptional position in the animal kingdom: although in it, he is not of it. The fundamental instinct of animals is the preservation of their life, and the escape from death. The idea of manliness is contempt of death. The preservation of life is frequently a secondary feeling in man. The reason is, that upon the animal life of man is erected a life of sentiment, emotion, and imagination, which always modifies his animal instincts, and sometimes entirely supersedes them. Hence come the long fasts and vigils of the saints of old, and of the modern Fakirs in the east. Fasts implying a continuance of abstinence from all food, which modern physiologists, deriving their knowledge from the effects of starving dogs and birds, and from the enforced abstinence of shipwrecked mariners and buried miners, insist upon ignoring, as they are so opposed to the conclusions of their experiments and observations. But the testimony in favour of these long spontaneous fasts is as trustworthy as any in favour of the other class, and certainly deserves attention, especially as it shows us a little farther into an every-day occurrence, that is, the power of abstract thinking, or very strong emotion, to

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arrest more or less the animal functions. It is the most common observation, that thought is incompatible with violent muscular exertion. Who ever heard of a mathematician trying to solve a problem in the hunting field? If he were found there at all, it would be to escape from thought. When thought is both very prolonged and very intense, and confined to one object, it may induce a kind of trance, in which there is an entire cessation of all other vital action; but long short of this, there can be no doubt that the over-active brain monopolizes the vital energy, and paralyses to a greater or less extent, the other portions of the nervous system, and cuts off the supply of cerebral stimulus required for the nutrition of the body; that is, which enables the capillary vessels to exercise their elective affinities as to what they shall absorb, and what they shall reject.

Nutrition then, or the continual action and reaction of the living world within upon the chemical one without, depends upon two factors, the one the nature of the materials presented to the body to be incorporated into it, to increase its bulk by growth, or to repair its decay, and the other the reception given by the body to these substances presented by the hand of external nature. The character of this reception depends, among other modifying circumstances, upon the state of the mind, and the whole doctrine of the fitness of aliments for their proposed end, must begin by assuming that end to be known. Hence the grand difficulty of laying down laws upon the subject, for the ends of life are so dissimilar in different men. The scale runs from the sensualist, whose avowed end is corporeal enjoyment, to the ascetic, whose object is bodily morti-

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fication. And it is the duty of a physician to appreciate all these varities, and to administer advice, not according to his own preconceived notions of the proper ends of life in general, but the most healthful, or least injurious method of securing for each person his peculiar, if innocent object of desire. If we were practising in the millennial times, we might tell men to avoid all excess, of eating, drinking, loving, hating, sleeping, waking, working, talking, &c.; perhaps when such advice can be followed, it will not be needed; certainly to talk so to men as they are now, would be in many cases simply an impertinence, or an affront. What would become of a popular preacher if he could not fill a large church with his voice for an hour together, or a popular alderman, if we forbade him to taste turtle? In short we must make the best of our materials, accommodating our rigging to the state of our vessel, and sometimes too thankful if we can secure a jurymast to save our ship from impending wreck. After these preliminary remarks, let us consider how our bodies are built up from day to day, and of what materials the structures consist.

"According to Quetelet, a full grown man weighs on an average 154 lbs., and if we substract the great quantity of water which runs through all parts of our body, keeping them supple and pliant, some 38 lbs.; 14 lbs. of this comes from the bones, and 24 lbs. from all the remaining parts. The former contains about 66 per cent., the latter 3 per cent. of earthy constituents which are left behind after combustion. Man consists, therefore, in more than a third part, of inorganic substances which are necessary to his existence, and which he must there-

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fore receive with his food. He must in fact as the evil spirit says in Faust, 'feed upon dust.'"[1]

It is to supply this dust for the stony skeleton of the fabric, that birds instinctively swallow sand and gravel, and not to assist the comminution of their food merely. Chossat found that birds deprived of sand, and fed upon grain, died in seven or eight months, and the bones became so brittle, that they broke with the slightest touch. Total abstinence from salt, the coustituents of which enter largely into the composition of the blood, is productive of various evils, among which, according to Woodward, scurvy is one of the most remarkable, and Dyer ascribes the frequency of tapeworm in the negroes, who eat but little or no salt, to the same cause. The necessity of salt is greater if the food be of vegetables, as less is contained in them than in animal diet. In strange contradiction to these well-authenticated facts, accepted by so cautious and critical a writer as Moleschott,[2] stands the extravagant nonsense of one of the apostles of abstinence, Mr. Sylvester Graham,[3] that "salt is a mineral substance, and is wholly innutritious and indigestible." Does this gentleman, and do those who crusade with him against the traditional symbol of hospitality, ignore the fact that bones are necessary, and that without mineral and innutritious articles of food we cannot have them?

Besides the salt we take with our food, we obtain a

[1] Schleiden, op. cit.

[2] 'Die Physiologie der Nahrungsmittel, ein Handbuch der Dietetik,' von Dr. J. Moleschott.

[3] 'Lectures on the Science of Human Life,' by Sylvester Graham, People's Edition, p. 270.

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large quantity of mineral substances from the water we use so copiously in all articles of consumption, even if we do not indulge in libations of the limpid element. And it has been observed that waters artificially deprived of their saline ingredients are unfit for domestic purposes. The phosphates, however, we must get either from animal food, or from grains, in which they exist in considerable quantity, and Mulder ascribes the frequent fractures he observed in a poor-house, to the exclusive potatoe diet. This disposition was rectified by giving the inmates rye-bread and butcher's meat, and the hint may be useful in dealing with various conditions met with among our own poor.

There is no great difficulty in comprehending how the inorganic materials are used in building up the body, for they undergo no other changes in it than those ordinary chemical de- and recompositions we are familiar with in the laboratory. The component mineral matters of the bones and other parts are all found in the blood, into which they are conveyed by the chyle the product of the food, and out of the blood they are selected by the capillaries of the bones according to laws of vital affinities of which we are totally ignorant. But this we do know, that it is now an ascertained fact, that there are no transmutations of elements in the body, that if phosphorus and iron are found in the blood, they must be previously discoverable in the food, and that there is not a tittle of evidence for the assumption that out of silica iron can be made, or out of oxygen phosphorus.

So much for the inorganic components of our frame. A much more difficult task is the description of the organic remainder. This has been divided into those

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substances which contain nitrogen and those which contain none. The nitrogenous elements (using the word element in a larger sense) of food are chiefly albumen, as it presents itself in the white of egg, or, in a slightly modified form, as it exists in the blood; fibrin, whether as found in blood, or in flesh, and caseine, or cheesy matter. Vegetable albumen, fibrine, and caseine, are identical with the animal substances so named. Gelatine, though nitrogenous, is generally considered not to be truly nutritive, as it cannot form blood; but it may form membrane, and so assist in nutrition. The non-nitrogenous elements are chiefly starch, sugar, and fat, in their various forms. Let us then proceed to consider how we obtain these substances, and what changes they undergo during their strange metamorphosis—we might almost say, without irreverence, their miraculous transubstantiation —from dead dry bread into living juicy flesh.

Before doing so, however, it may be well to point out the enormous importance of the facts just mentioned, that the body imperatively demands for its health a supply of certain mineral substances, among which soda and phosphorus hold an important place, and that various forms of disease, such as scurvy and softening of the bones, are the consequence of starving it in this direction. The obvious deduction for us is that to cure such unsound states of body, it is as essential to feed it with what it wants, as it is to give food to a famished man. Here we touch upon one of the limits of the application of the homœopathic or any possible therapeutic formula. Diseases arising from insufficient supply of any of the constituents of the frame, cannot be cured by any medicine whatever, and can only be cured by giving in some

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form or other the required aliment. This holds true, as was first demonstrated by Boussingault,[1] for the vegetable kingdom, and upon this fundamental law of nutrition the whole modern system of agriculture is based; and in consequence of the violation of this law, and growing potatoes upon a soil richly manured, arose the potato-disease, so eventful in its social and political consequences.[2]

Let us resume the previous enquiry as to the means by which the body gets its supply of nitrogenous ingredients. The answer will be sufficient if we can trace them into the blood, for the blood is the body in a fluid state; nothing is found in any part of the animal frame which does not exist, in some form or other, in the blood. We must begin with an analysis of this all-important fluid.

The nitrogenous constituents of the blood are:

1st. *Albumen*, of which, according to Becquerel and Rhodier, there are from 71 to 87 parts in a thousand. Salts of phosphate of lime, of sulphate of potash and soda, and chloride of sodium, are intimately mixed with this albumen, which, according to the formula of Mulder, consists of $N^{89}\,C^{680}\,O^{220}\,S^{8}\,P^{00}$.

2d. *Fibrine*, which differs chemically from albumen, chiefly in containing more oxygen and less sulphur. Mulder's formula for it is $N^{88}\,C^{671}\,H^{523}\,O^{231}\,S^{7}\,P$. The quantity of fibrine varies from 2·1 to 2·8 parts in a thousand.

[1] 'Rural Economy in its relations with Chemistry,' &c., by J. Boussingault.

[2] See Johnston, op. cit.

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3d. *Caseine.* A substance of a very complicated chemical nature, and found in a very small quantity in the blood.

4th. *Globuline.* A very important constituent, forming the white membrane of the blood-globules. It differs in many of its chemical aspects from the albumen, fibrine, and caseine, and according to Le Cann, amounts to 125·6 parts in a thousand. Mulder's formula for it is $N^{94} C^{694} H^{522} S^{6}$.

5th. *Hæmatin*, on which the colour of the blood-globules depends. It amounts to about 2·3 parts in a thousand, and its most interesting feature is its peculiar chemical nature, for it contains neither sulphur nor phosphorus, but consists of $N^{3} C^{44} H^{22} Fe$. Hence the necessity of iron for the proper nourishment of the body, and especially for the ruddy glow of health produced by the rich red blood.

Such, then, are the nitrogenous constituents of the body, which we must supply it with to preserve life and health, and we shall now consider whence the supply is derived, and in what form, and what changes it has to undergo before it is converted into this liquid flesh and bone.

The supply must come from either the vegetable or animal kingdom, for although we read of tribes who consume as their chief food large quantities of clay, yet it is now a recognised fact, that unless the clay contain animal or vegetable matter in some form, it may be merely as a multitude of those infusoria Ehrenberg describes, it is in itself as clay, or mountain meal, or any other merely mineral substance, incapable of sustaining

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life, although for a time it may appease the cravings of hunger.

Let us turn then to the vegetable and animal kingdom, and set out with this important observation of Mulder, "that those who feed on flesh, and those who feed on vegetables, partake of the same nutriment; they have both their albumen, the one derived from plants the other from animals, but for both the albumen is the same."[1]

It is obvious that the albumen, and its modification gelatine, which we derive from our animal food, requires less transformation to prepare it for returning to a condition almost precisely similar from that which it had previously held, than the vegetable albumen does to enter for the first time the portals of animal life. Indeed, all that it needs is to be dissolved, and this is done, first by the processes it undergoes in the kitchen, which holds to man somewhat the same relation that the first stomach or paunch does to ruminating animals; next by the mastication and insalivation it is subjected to in the mouth; and lastly by the solvent action of the gastric juice, which according to the observations of Beaumont on the Canadian, Alexis St. Martin, the operations of whose stomach were open to inspection by an accidental perforation, takes from two to three hours to dissolve an egg.

Albumen is first coagulated, and then dissolved by the gastric juice; gelatine is simply dissolved, and thus passed onwards, as chyme, to be absorbed into the blood. The nutritiousness of animal food is generally

[1] G. J. Mulder en W. Wenckenbach, 'Natur-en Scheikundig Archief.,' s. 128, quoted by Moleschott.

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calculated by the quantity of albumen it contains, and hitherto there has been a disposition to consider that gelatine only afforded material for cartilage and bone, and not for muscles. This opinion rested upon some experiments of the French commissioners appointed to investigate the qualities of different aliments, who succeeded in starving dogs by giving them nothing but pure gelatine to eat. However, these experiments, at once unphilosophical in their conception and cruel in their execution, are entirely refuted by the fact, that dogs not only lived, but throve and fattened upon a diet consisting of nothing but bones; a fact corroborated by the history of every convalescent, who is nourished by arrowroot and pure soup, as Mulder remarks.[1] Why dogs thrive upon bones, and starve upon the pure aliment the bones contain, is a problem of which there are many examples, some of the most curious are several instances where horses on a voyage pined away under the exclusive use of grain as food, and, impelled by the instinctive requirements of their organism, tore and eat all the dry wood within their reach; on this hint they got chips of wood, and very soon recovered their flesh and spirits.[2] From this we gather, that the vital processes, as well as the chemical products, are necessary for the maintenance of health, and it should make us hesitate before we subscribe to any diet-table submitted to us by the chemist, unless it be in accord-

[1] It is right to observe that this view of the utility of gelatine is not generally accepted by chemists, who say that both bones and soup contain other matters besides gelatine, a substance they consider incapable of forming blood, and consequently of sustaining life.

[2] Graham's 'Lectures,' &c.

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ance with the findings of popular experience. It may be that hereafter the chemist shall discover the value of some of those substances, such as kreatine, which exists in very small quantity in animal food, but which, as its name implies, is supposed to be a sort of essence of flesh—if the phrase be allowable; and it may be, that as the slight chemical difference in the amount of sulphur and oxygen makes the distinction in the laboratory between fibrine and albumen, so equally minute portions of chemical ingredients may induce totally different forms of the aggregation of the atoms from which the various tissues are evolved; and it is impossible not to recognise here how very small quantities of medicinal agents, properly administered, may exercise an enormous effect upon the development of the organism. How great may be the effect of Sulphur, of Phosphorus, of Silica, for example?

While the nutritiousness of food depends chiefly upon the quantity of albumen it contains, its digestibility seems to depend upon the form in which it is presented to the system. Various attempts have been made to classify the ordinary articles of animal diet, and some writers, such as Robertson,[1] give a regular schedule, beginning with mutton and ending with pork. It is enough to observe upon such schemes, that they rest upon purely arbitrary assumptions, or upon the fallacious experiments of Beaumont; "who, when he saw mutton-suet dissolve in his Canadian's stomach in five hours and a half, must evidently have mistaken," as Moleschott observes, "mechanical division for chemical solution." What vitiates his conclusions most, however,

[1] Robertson on 'Diet and Regimen.'

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is, that he employed mixed substances in his experiments, and also that with him solution, or rather, reduction into a homogeneous mass, was equivalent to digestion. A more recent case of a similar conveniently perforated stomach, taken advantage of by Grunewald and Schroeder, gave results diametrically opposed to Beaumont.[1] The Germans found raw meat and veal more rapidly digested than boiled beef! In the absence of all satisfactory experiments, we must rely much upon popular experience. It is certainly better to tell those who ask our advice upon the point, that although it may not be true, that after attaining maturity every man should be his own doctor, yet certainly every man may have discovered what agrees and what disagrees in the ordinary articles of animal food; and to stigmatise as unlawful the eating of the flesh of hogs, in a country which was never either Jewish or Mahommedan, is what may be called an infraction of Christian liberty. Pork is the cheapest animal food, and therefore the only attainable form for many, and because it does not agree with some, there is no reason to ban it with such Levitical austerity. Much depends, in this as in most things, on the form in which it is prepared, and we must take a peep into the kitchen before we dilate further upon what is digestible. On this matter we cannot do better than quote from the work of Professor Johnstone.[2]

"In cooking animal food, plain boiling, roasting, and baking, are in most general favour in our islands.

[1] 'Brit. and For. Med.-Chirurg. Rev.,' Jan., 1855.

[2] This passage, although quoted from Johnstone, is actually derived by him from Liebig's work on the 'Juice of Flesh.' It is a pity he should have left this fact to be stated by others.

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During these operations, fresh beef and mutton, when moderately fat, lose on an average, about—

	In boiling.	*In baking.*	*In roasting.*
4 lbs. beef lose	1 lb.	1 lb. 3 oz.	1 lb. 5 oz.
4 lbs. mutton lose	14 oz.	1 lb. 4 oz.	1 lb. 6 oz.

"The greater loss in baking and roasting arises chiefly from the greater quantity of water which is evaporated, and of fat which is melted out during these two methods of cooking. Two circumstances, however, to which it has not hitherto been necessary to advert, have much influence upon the successful result of these and some other modes of cooking.

"If we put moist flesh into a press and squeeze it, a red liquid will flow out; this is water coloured by blood, and holding various saline and other substances in solution. Or, if after being cut very thin, or chopped very fine, the flesh be put into a limited quantity of clean water, the juices of the meat will be gradually extracted, and by subsequent pressure will be more completely removed from it than when pressure is applied to it in the natural state, and without any such mincing and steeping. The removal of these juices renders the beef or mutton nearly tasteless.

"When the juice of the meat, extracted in either way, is heated nearly to boiling, it thickens, or becomes muddy, and flakes of whitish matter separate, which resemble boiled white of egg. They are, in fact, white of egg, or albumen, and they show that the juice of flesh contains a certain quantity of this substance, in the same liquid and soluble state as it exists in the unboiled egg. Now, the presence of this albumen in the juice of

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butchers' meat, is of much importance, in connection with the skilful preparation of it for the table. The first effect of the application of quick heat to a piece of fresh meat, is to cause the fibres to contract, to squeeze out a little of the juice, and, to a certain extent, to close up the pores, so as to prevent the escape of the remainder. The second is to coagulate the albumen contained in the juice, and thus effectually and completely to plug up the pores, and to retain within the meat the whole of the internal juice. Thereafter the cooking goes on through the agency of the natural moisture of the flesh. Converted into vapour by the heat, a kind of steaming takes place within the piece of meat; so that, whether in the oven, or on the spit, or in the midst of boiling water, it is in reality, when skilfully done, cooked by its own steam.

"A well-cooked piece of meat should be full of its own juice or natural gravy. In roasting, therefore, it should be exposed to a quick fire, that the external surface may be made to contract at once, and the albumen to coagulate before the juice has had time to escape from within. And so in boiling: when a piece of beef or mutton is plunged into boiling water, the outer part contracts, the albumen which is near the surface coagulates, and the internal juice is prevented either from escaping into the water by which it is surrounded, or from being diluted and weakened by the admission of water among it. When cut up, therefore, the meat yields much gravy, and is rich in flavour. Hence a beef-steak or a mutton-chop is done quickly, and over a quick fire, that the natural juices may be retained.

"On the other hand, if the meat be exposed to a slow

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fire, its pores remain open, the juice continues to flow from within as it is dried from the surface, and the flesh pines, becomes dry, hard and unsavoury. Or if it be put into cold or tepid water, which is afterwards gradually brought to a boil, much of the albumen is extracted before it coagulates, the natural juices for the most part flow out, and the meat is served in a nearly tasteless state. Hence to prepare a good boiled meat, it should be put at once into water already brought to a boil. But to make beef-tea, mutton-broth, or other meat soups, the flesh should be put into cold water, and this afterwards very slowly warmed, and finally boiled. The advantage derived from simmering, a term not unfrequent in cookery books, depends very much upon the effects of slow boiling, as above explained."

This passage contains the whole theory of the art of cooking meat. If we want to give our patients the full richness of the flesh, we must order a chop or steak, or something of that sort; if we want the flavour chiefly, and not the strength, we give beef-tea; if we want bulk rather than either flavour or strength, we can order the meat to be boiled to rags. This knowledge gives us all the command of the kitchen which we can expect to have.

So much for animal food; let us now consider what the vegetable world affords in the shape of albumen, and how it is to be prepared for the table.

1st. *Soluble Vegetable Albumen* exists in a greater or less quantity in the juices of all plants, and in greatest abundance in the so-called grains. In its proportion of nitrogen, carbon, oxygen, and hydrogen, it is exactly the

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same as animal albumen, but differs somewhat in the amount of Sulphur and Phosphorus. It is soluble in water and various acids.

2d. In the seeds of leguminous and corn plants, besides the vegetable albumen, is formed a substance called *kleber*, by Beccaria, by Leibig, vegetable fibrine. It is insoluble in water, but soluble in alkalies and acids.

3d. A modification of this is vegetable gelatine, which like kleber is insoluble in water, and soluble in alkalies and water.

4th. The last and most recently discovered albuminous ingredients in vegetables, has so close a resemblance to caseine, as to be called, by Liebig, vegetable caseine. Vegetable caseine is formed chiefly in leguminous plants. The Chinese make a cheese from peas and beans, closely resembling skimmed milk cheese.[1] From these chemical facts, we are able now to infer the digestibility of vegetables, as well as their nutritive property, for, as a general rule, while their power to nourish the body is in direct ratio to the total amount of their albuminous contents, the facility with which they are reduced to a state fit for assimilation depends upon the relative quantity of the soluble and insoluble form of this albumen. Besides this important explanation, chemistry also suggests the reason of dressing some vegetables with vinegar; for all forms of cabbage contain a large quantity of insoluble albumen, which is rendered

[1] One of the most distinguished chemists in this country informed me that he had prepared cheeses, *à la Chinois*, *i. e.*, from peas and beans, and that they were hardly to be distinguished from skimmed-milk ones.

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soluble by the addition of this acid. It is probable that in the preparation of vegetables for the table, we have yet much to learn, and no doubt the indefatigable efforts of the vegetarians will be of use.

The comparative amount of nutriment contained in the most nutritious form of animal and vegetable food, is, according to Professor Johnstone, about 3 to 1.[1] "Or a pound of beef-steak is as nutritive as three pounds of wheaten bread, in so far as the nutritive value depends upon this one ingredient," *i. e.* albumen. Thus we find that as Coleridge defined a rogue to be a fool with a circumbendibus, so we may consider a man who lives on vegetables alone, to be a roundabout flesh eater; for by a circuitous route he gets the same albumen as the beef-eater gets directly. Whether it is well to live solely on vegetables, or solely on animal food, or on a mixture of both, is a question to be decided by the whole circumstances of the eater. The appeal to nature made by the vegetarian propagandists, is manifestly futile; for man's nature is progress, and as one generation succeeds another, man alone of all creatures that dwell upon earth, inherits the accumulated acquisitions of those who lived before, and every child born stands in a new relation to the external world to what its parents did. Man is "the heir of all the ages," as such he accepts on his birth new duties in a new sphere of action from those who went before him. Is he to be denied the right of innovation, perpetual innovation and general progress in the food which is so essential to his life?

[1] Op. cit., p. 128.

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His nature is himself. He alone can resolve the problem of his duties for himself. If he finds that for their fulfilment he requires to abstain from flesh, let him abstain; but let him not insist upon putting a strait jacket upon his neighbour who is under no such necessity, and who on the other hand finds he can do his work better if he live on meat. Above all, it is lamentable that the apostles of this vegetable creed should be so carried away by their fanatical exclusiveness, as to convert the board of hospitality, where the bread, the meat, and the salt, are spread to satisfy the various instincts of our bodily requirements, and to express the oneness of our origin, our progress, and our destiny, into a conspirator's meal, at which each guest pledges himself against the common food of his fellow-men. In former times, when men fasted, they went some days' journey into the wilderness, not to annoy their neighbours with their lugubrious faces. Might not all exclusionists in diet do well to follow this example? But we have said enough, perhaps too much, upon this head, and we must conclude with the saying of Goëthe — "That is good which does us good." Newton wrote his treatise on optics living upon wine and water, biscuits and tobacco. That was good for him. We might have injured his labours if we had insisted upon his eating a mutton-chop. But it does not follow, that if we confine ourselves to biscuits and sherry, in process of time we shall be transformed into Newtons. In short, the reason of every man is, or ought to be, the absolute lawgiver upon this matter to himself, only the reason should be enlightened by the fullest knowledge of how he can best secure the ends it

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seeks. But as these ends vary infinitely, so should the means also vary, and the whole dispute between the vegetarians and non-vegetarians is terminated by the old proverb—"One man's meat is another man's poison."

Having thus briefly described the inorganic and nitrogenous ingredients of food out of which the bony skeleton with its ligaments, and the muscular fibres of the body are procured, let us proceed to investigate the character of the materials which furnish the non-nitrogenous remainder of the animal frame, and the complex series of transmutations which they undergo, before their final adjustment to the parts they play in the living organism.

The starch group is the first we here encounter, and the most important. It consists of—

1st. *Starch or fecula.* A substance found generally in those parts of a plant to which light does not penetrate. It exists in the greatest abundance in the potato, and forms above sixty per cent. of most grains. Its formula is C^{12} H^{10} O^{10}. It is soluble in warm water, and readily transformed into a substance called dextrine, of similar atomic constitution, but of different physical and chemical properties, by the contact of saliva and various organic substances.

2d. *Cellulose.* Of the same atomic constitution as starch, and like it, by long exposure to the action of acids, being converted into dextrine, but insoluble in water. It abounds in green vegetables, especially cabbage.

3d. *Pectin.* Of the same atomic constitution as the

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former two. It is imperfectly soluble in water, but if exposed to the action of acids at a higher temperature, it passes into a substance called *metapectic acid*, which is soluble. Pectin is met with in large quantities in various roots, such as carrot, turnip, &c., and still more in the fleshy fruits, such as raspberries and apples. The gelatinous-looking substance so abundant in Carrhageen moss, is nothing more than a modification of Pectin.

4th. *Dextrine*, or gum. These are essentially alike, but the term gum has a more limited application. *Dextrine* may be called the father of all the gums. It exists in large measure in all ripe fruits, and is the transition form of the metamorphosis of all feculent bodies into sugar. It derives its name from its action on light; the polarized ray is thrown by it to the right; probably the ray thrown to the left, acting upon similar elements, forms true gum.

5th. *Grape sugar* and *glucose*, are closely allied, and only distinguishable by the difference of their action on polarized light, and the crystallizable character of grape sugar, which glocuse cannot attain. Besides being readily produced by the action of various substances upon other forms of fecula, it exists naturally in figs, apricots, and many other sweet fruits, as well as the grape. It is readily soluble in water, and if any nitrogenous body be present it undergoes fermentation, as it is called, and is converted into alcohol. Sugar of milk has the same composition as grape sugar, but cannot pass spontaneously into alcohol; it requires first to be changed into grape sugar, a transformation readily effected by the action of an acid.

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Cane sugar differs atomically from all the other sugars, its formula being C^{12} H^{11} O^{11}; or, as there is probably one atom of water in this, the truer formula will be C^{12} H^{10} O^{10}. Like sugar of milk, it is incapable of direct fermentation, and to acquire the useful property it has to become grape sugar, through the action of an acid.

Thus we see that all the varieties of starch may be promoted into grape sugar, that highest point of advancement of the class, at which it undergoes the astonishing transformation from a sweet and harmless material for the nourishment of the body, into a fiery stimulant called alcohol, the most fertile source of every human crime, the great parent of madness and suicide.

The second non-nitrogenous group consists of substances from which fat is derived; for although grape sugar may be converted into fat, yet this transformation is made more easily if ready made fat be present, and it exists in large quantities in various forms of food. The most widely distributed form of fat is *elain*, which constitutes nearly eighty per cent. of olive oil. Its formula is calculated at C^{35} H^{35} O^{4}. It is generally met with as an elaic acid, and as such, or as a soap, is received into the blood. Next in abundance to elain is *margarin*, which constitutes sixty-eight per cent. of butter, and enters largely into the composition of all solid fats. Its formula is C^{35} H^{35} O^{4}.

Stearine is much more rarely met with; it is found in mutton suet and cocoa. Its formula is C^{37} H^{37} O^{4}.

The last member of this group which is of any importance in a dietetic point of view, is *Butyrine*, which is

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a constant constituent of milk, although it is in the small proportion of two per cent. It is readily decomposed. Its formula is C^{11} H^{11} O^{4}.

The above group, as will be perceived, abounds in carbon, and yet the demand for this element is so large, both for the purposes of respiration, and to form the many carbonates that exist in the body, that besides the contributions derived from the various forms of fat, a considerable amount is afforded by various acids, which constitute the third and last non-nitrogenous group.

The most important of these are the oxalic, malic, citric, tartaric, acetic, and lactic. All these acids are very similar in atomic constitution, consisting generally of four atoms of carbon, two of hydrogen, and from three to five of oxygen. The formula of lactic acid deserves more specific attention, for it is readily formed from grape sugar, it stands thus : C^{6} H^{5} O^{5}. As this acid is found, in combination with alkalies, in the muscles, it is probable that it passes unchanged into the blood. As also the acetic, for it is met with in the perspiration, although in small quantities, and, no doubt, much the largest quantity of the acetic acid we use, is changed into carbonic acid and water. All the acids we have enumerated are readily decomposed, and form new combinations with the various complex bodies they encounter in the blood. It would be out of place here, to attempt to follow their chemical progress, and we shall now proceed to the more interesting task, of describing the process by which the most important members of the non-nitrogenous groups are adapted for the various offices they have to fulfil in the state corporeal.

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Let us first pursue the history of starch, which enters so largely into most vegetable diet, from its entrance into the mouth, to the transformed existence it presents in the blood.

When food is taken into the mouth, there is, or ought to be, an immediate flow of saliva from the various glands connected with that cavity. The action of this saliva upon starch is very remarkable, as it gradually converts it first into dextrine, and then into sugar. Digestion then begins in the mouth, and from this important fact, too much lost sight of, we may deduce several dietetic rules. The first that occurs is an old adage, that food "well chatted is half digested:" a curious example of the popular instinct anticipating the scientific discovery; for it is literally true; the chatting during a meal prolongs the process of mastication, and has also the effect of conveying the requisite amount of nervous influence to the salivary glands. No part of the system is more under the influence of the emotions than these organs. "The mouth waters for dainties," is a literal fact. The sight and smell of food make the mouth weep in pleasurable anticipation of the "sweet morsel" it will soon "roll under its tongue." How exact is this description! While gaiety thus improves what we may call the oral digestion, fear and anxiety exercise as powerful an effect in the opposite direction. The dryness of the mouth is a symptom of terror, suggesting the expression, "vox faucibus hæret," or, "the tongue cleaves to the roof of the mouth." In India a thief is detected by desiring him to chew rice in his master's presence; the saliva will not flow from fear, for "conscience doth make cowards of us all," and the thief is unable to make any impression upon

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the dry hard grain. So much for the importance of recommending social meals, and the obvious risk of throwing an undue amount of labour upon the stomach by bolting food. Is it not possible, that this habit, which is said to be so common in America, is one of the causes of the remarkable leanness of the people? For, as we shall see afterwards, the ultimate destination of this starch and sugar is to supply fat to the frame.

Another question of great practical importance here suggests itself, viz.—whether we do wisely in restraining our patients from all spices. The action of mustard and pepper, and of seasoning generally, is very powerful upon the salivary glands; and it is a remarkable fact, that while the taste for sweet things is the characteristic of childhood and boyhood, the love of tarts and sugar-plums, as a rule, entirely gives place to a relish for spices in maturer years. If we condemn our patients to eat tasteless farinaceous food, shall we not incur the risk of subjecting them to the punishment of the thieving Hindoo, and will they not be too glad to get rid of the insipid stuff as expeditiously as possible, that is, little digested in the mouth, and gulp it down, "unhouseled, unannointed, unannealed," to the sepulchre of their stomach, where it can hardly expect a proper welcome, coming in so unmannerly a fashion. If this is in any measure true of mature manhood, it will be much more so of advancing years, when the apparatus for disintegrating food, so that it may be well kneaded with saliva, is all broken and useless, and at the same time the sense of enjoyment is diminished, and there is less lively participation of the cerebral functions in those of nutrition. To insist upon an old man eating his beef and bread

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without mustard, is almost as cruel as to give him mustard without bread or beef. The mustard, to him at least, is an essential; without it he could not instigate the glands of his mouth to pour out their contribution to the first act of digestion.

The starch, then, on its arrival at the stomach, has already undergone a partial transformation into sugar, the process is there completed by the action of the pancreatic fluid, which bears a close resemblance to saliva, and by the secretions of the stomach itself, the most important of which is the complex substance called *pepsin*, whose characteristic peculiarity is, its power of inducing active molecular changes in all organic matters susceptible of its influence. It is, in short, the digestive principle in its purest form. Besides converting the unreduced remnant into sugar, it acts upon the sugar in such a way as to transform a portion of it into lactic acid. This is also done by the bile, but the full action of the bile upon the chyme is yet unknown, and how sugar is converted into fat is still an unsolved problem—possibly an insoluble one, for it is the step out of chemistry into vitality. This first act of vital generation, by which the hard and angular chemical sugar is transformed into the soft and globular vital oil, wherewith the lamp of life is fed at the lungs, and the whole bodily machine kept pliant and warm, seems to be in some way or other under the direct control of the brain. The nervous system, the differential between what is vital and what is chemical, here asserts its claim for the first time over the contribution to the reconstruction of the living body presented by external nature. This important fact has come to light during the investigations into the cause of the curious disease

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known by the name of diabetes mellitus, which seems to result from the incapacity of the brain to discharge its requisite office, and transform the sugar, presented to the organs of assimilation, into fat, so that, as sugar it enters the blood, and as sugar leaves the body by various emunctories, and thus the body is starved of its fat, and extreme emaciation, going on generally to death, is the consequence.[1]

No doubt a portion of sugar, and a large portion, may continue unchanged in the blood, till it passes through the lungs, where it may be oxydized and converted into carbonic acid and water; but there is as little doubt, that what we consume as sugar is largely converted into fat; and hence the fattening effect of a purely sugar diet upon the negroes who work at the sugar-cane plantations. The facts which seem to me of such immense importance to us practically, are these two, that we are now on the road to discover what substances yield the elements of fat to the body, and what the condition of the body is which enables it to turn its opportunities to account; and what the bodily states are, on the other hand, which debar it from the privilege of converting the bounties of nature into that ductile element, which rounds the harsh masculine features of bone and muscle into feminine beauty, and tends to mollify the temper and tranquillise the mind.

Besides the fat we obtain from the starch and sugar we consume, there is a considerable quantity of it in the principal grains employed in making the various kinds of bread, or the cakes and other forms in which

[1] This subject forms the matter of the next Chapter, "On the Structure and Uses of the Liver."

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meal is prepared for use. And it is of consequence to know the proportion in which it exists in the different varieties of these bread-stuffs, for upon the amount of fat seems to depend the laxative or binding action of these articles of diet upon the alimentary canal. Professor Johnstone, in the book we have before so often alluded to, gives the following proportions:

In fine English wheaten flour . .	2 per Cent.
In bran of English wheat . .	6 ,,
In Scotch oatmeal . . .	6 ,,
In Indian corn	8 ,,

From this table we learn that fine wheaten bread contains only one third the quantity of oil contained in oatcake or bread made from bran, and that we may derive a useful hint from this in the management of cases attended with constipation. It was generally supposed, that the bran acted as a foreign body upon the mucous membrane, and that from the irritation it produced, the peristaltic action was increased. This may be partly true, but the presence of the oil is undoubtedly highly conducive to the greater activity of the bowels. Rice contains very little fat, and hence its binding tendency upon the bowels. Indian corn, on the other hand, is very rich in fat, and might probably be more extensively used in this country, with great advantage. All travellers in America dwell with much gusto on the delicious cakes of Indian corn that are there served up at breakfast.

Although we derive the elements of the fat, which enters into the composition of every tissue of the body except the enamel of the teeth, and constitutes the bulk

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of the brain and nerves from various forms of fat that present themselves both in the animal and the vegetable kingdom, yet the modifications these endure before they take their new place in our frame, is much greater than any which the other primary constituents undergo. In the brain it is found in the form of two acids, the one called cerebric, the other oleophosphoric; the latter possesses the interesting property of being resolved by heat and water into Elaine and phosphoric acid. This fact may throw a light upon the important observations recently made by Dr. Bence Jones, upon the amount of phosphates in the urine being the measure of the waste of the nervous system.

It does seem strange that with such facts as this one and many others staring us in the face, to show the immense value of fat as a source of nourishment to all parts of the body, and chiefly to the most noble parts of all—the brain, it should be the fashion of chemical physiologists to speak of fat as if it served no other purpose than to be burned at the lungs to warm the rest of the body. Surely, the curious fact of hibernation alone might teach us that fat went far to sustain life, for from the store accumulated in themselves during summer and autumn, must the beasts and other animals which pass the winter in sleep, without food, draw the whole nourishment during the months of their retirement from active life. To deny the name of nourishment to that which sustains the life of an animal, involves a paradox; and in this instance the paradox results from an exclusive application of the term nourishment to substances which supply the materials for constructing the fleshy parts of our tabernacle,

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and ignoring the existence of the earth and fat, which are essential to the formation of the organs of sensation and locomotion.

The sophism implied in this paradox lies at the root of the entire misrepresentation of the effects of alcohol, by those who deny to it any nutritive qualities; and as the subject is one of unsurpassed importance and interest to us as physicians, it will be right to inquire fully into the properties of alcoholic drinks, regarded, not as medicinal stimulants, but as articles of strictly dietetic or nutritious use.

The ground we are now entering on, demands from us a most dispassionate investigation, for it is beset with difficulties on all sides, and requires that we should at the outset clearly define what are our special duties in this matter, as contradistinguished from those who are most prominent in leading public opinion upon it.

To those who devote themselves to the arrest or mitigation of the frightful evils produced by intoxication, whether by popular lectures, or by addresses from the pulpit, or by the organization of societies, we wish to express the deep sense of our obligations, for their noble and patriotic efforts, and in so far as it is in our power to advance the great cause of temperance, in our capacity of citizens, it is surely our bounden duty to do so. But the restraint of excess is a wholly different question from the proper use of any article of food; and if out of dread of offering an obstruction to the promoters of a great philanthropic movement, we shirk a candid investigation of the alleged benefits to be derived from a temperate indulgence in all the drinks they find it expedient to condemn, we should be abdicating our position as ad-

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visers of those who consult us as to what is best for them, and be betraying their interest and our duty for the promotion of a remote and hypothetical good. We have no right to sacrifice our patients upon the altar of temperance. There is no more inconsistence in promoting the cause of temperance, and recommending the use of wine, than in doing all we can to advance a Fire-Insurance office, with all its machinery for extinguishing a conflagration, and each of us at the same time making these very efforts by the comfortable warmth of his "ain fire side." Temperance is the law of life; the torrid and the frigid zones are equally inimical to man, nor less so probably would be found the universal abstinence from wine and all its kindred. Nay, we do not take up a merely negative position in reference to temperance, while thus claiming a fair hearing for what its *soi disant* advocates condemn, we go further, and maintain, that if it be shown that the use of wine, spirits, and beer, be conducive to health in certain circumstances, by recommending their use, we are really doing more for the ultimate triumph of temperance, than by passing no end of Maine-liquor laws; for it is a fact beyond all denial, that one of the most fertile causes of intemperate indulgence is traceable to some bodily infirmity or depravity, and that intoxication is hardly possible for those whose bodily and mental conditions are in the highest state of vigour and refinement.

I have been thus free in the expression of my sentiments, because I find myself opposed to the views of many, whose opinions deserve the highest attention and consideration at our hands, and with none more than Dr. H. Madden, who, in an article published in 1846, in the

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'British Journal of Homœopathy,' makes the following observation. "Alcohol when acting not as a stimulus, but substantially, is not and cannot be a pabulum to any organ, and for the following reasons:—*first*, its chemical composition disqualifies it from taking any share in the formation of muscular fibre, or other azotized compound, since it possesses no azote; and it has now been proved that the higher orders of animals have no power to compound their proximate principles, but must obtain them ready formed: *second*, although its chemical constitution has some approximative relationship to nervous matter, we have no proof that it is or can be transformed into that substance, while numerous arguments may be drawn from transcendental chemistry, to show its extreme improbability."

In opposition to this statement, we think we shall be able to show that alcohol may supply a pabulum to every organ by affording it the materials whence to derive its fat, which is essential to its healthy action, and also that there is every probability of its nourishing as well as cherishing the nervous system. Human fat, according to Chevreul, contains 79 per cent. of carbon, while alcohol contains 53 per cent. of the same element, and the conversion of alcohol into fat is farther rendered probable by the examination of the blood of persons who have imbibed alcohol in large quantities: their blood has been observed to be of a whitish, milky appearance, from the quantity of fat globules that float there.[1] There is a case on record in which all the organs of the chest and abdomen, and even the muscles in the body of a drunkard,

[1] Wilson on the 'Pathology of Drunkenness.'

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were found converted in a great measure into a fatty substance.[1]

So much for the first proposition, that alcohol goes to make fat in the body: a statement which, although we cannot assume as a fact, we may assert as a very probable conjecture. As to the second, that alcohol by loading the blood with fat, probably affords special nourishment to the nervous system, which consists in a great measure of this substance, it seems also probable, not only from the fact of the chemical composition of the two, but from its physiological action as a narcotic. We know, indeed, as a positive fact, that the brain is nourished during sleep, and we also know that, as a general rule, the tendency to sleep is in direct proportion to the quantity of fat in the body. Fatness and somnolence are mutual cause and effect, as we see in the example before referred to, of hibernating animals who do not go to sleep till they have laid in a great store of fat, and who then pass their whole time in this condition, and the converse we see in the effects of sleeplessness producing all the evils of insufficient nutrition on the brain. "All the causes," says Dr. Bucknell, in a recent article in the 'British and Foreign Medico-Chirurgical Review,' "even of the acute forms of insanity, point to an interference with the due nutrition of the brain. Starvation causes raving delirium, mortification produces muttering delirium. * * * If anything is positively known of the brain and its functions, it is that it expends its powers during the waking state; and that it is nourished and its powers are recruited by cell-growth, or otherwise, during sleep, * * * and during the prodromic period of

[1] Rosch 'Der Misbrauch geistiger Getränke,' p. 93.

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threatened insanity, opiates often act like a charm."[1]—That alcohol does act narcotically, especially if combined with another narcotic, such as that of hop, requires no proof: it is an admitted fact. And now we are in a position to perceive how it is that the English and the Germans are a fat race, while the French and the Scotch are lean. The former drink beer, which contains about the same amount of alcohol as the light French wines, but in combination with a narcotic and nutritive extract to the extent of from 4 to 8 per cent; while in milk, the model food, the nutritive matter is about 12 per cent. So that a pint and a half of good beer is equal, in respect of solid nourishment alone, to a pint of milk. But it has this immense advantage over milk, that it soothes the over-active nervous system at the very instant that it presents to it its means of nourishment. It cherishes and nourishes at once. Shall we then, misled by the crude speculations of modern chemists, reject the evidence of all history in favour of the mighty boon conferred by Bacchus on our race? When we reflect that in the present age the work is done more with the brain and the nerves, than with the muscles and the bones, that we have now bones of iron and muscles of steam, which relieve to a great extent those of the human frame, and that this substitution is daily progressing; while, on the other hand, the strain upon the mental and cerebral system is proportionally increased, for we cannot multiply brains by any process of machinery yet invented, and that this very liberation from the toil of the hands begets greater and greater competition in the race

[1] 'Brit. and Foreign Med.-Chirurg. Rev.,' Jan., 1855.

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of mental achievement; when we reflect on all this, is there not something very presumptuous in venturing against the most advanced investigations of physiology, to enforce more than monastic ascetism upon those who instead of the dawdling life of the monastery, with the trifling expenditure of its hibernating existence, have daily to undergo a waste of cerebral and nervous matter in the furnace of a city, to an amount hitherto unknown in the annals of our race? If we forbid alcohol in all its forms, is there not a danger of our starving the brains and the nerves? May not a portion of this substance be absolutely necessary for those who like pleaders have to go through a work of intense cerebration, so to speak, for hours together? Certainly in my own practice I have met with many instances where the health was kept in a state of deterioration by abstaining from alcoholic drink, and where the addition of a glass of ale or a glass of wine permanently improved the health and comfort of the persons; and I believe, notwithstanding all the averments of the teetotallers, will contribute to the prolongation of a life it has already made happier. There is wisdom in the adage that wine is the old man's milk.

Before considering the comparative value of different forms of alcoholic drinks, let me observe that the cases in which marked benefit was derived from their administration as articles of regular diet, not occasional stimulants, were all characterised by the feebleness of the pulse, and a difficulty of obtaining a sufficient quantity of sleep. This entirely corresponds with what our physiological investigations might have led us to expect. In such instances, alcohol in its proper form at once propitiated the appetite of the nervous system, and presented

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to it its most appropriate aliment. At the same time it is right to state, that the greatest caution is required in deciding on the class of cases which are suitable for this method of treatment, for it certainly is extremely injurious in many cases characterised by a feeble pulse, if there is preternatural general irritability present. In such cases it acts not as a sedative, but as a stimulant, and increases instead of mitigating the evil. I have known immediate improvement to follow the withdrawal of all stimulants, in cases where the heart is at once feeble and irritable. Indeed I do not believe there is any matter connected with the management of those who consult us that requires so much care, and about which, with all our caution, we shall so often commit errors in giving a reply to the question: what must I drink? Perhaps it may be well to give Dr. Bence Jones's resumé of the various qualities of alcoholic drinks, as it may afford us some guide in the solution of this intricate and perplexing problem.[1] "Generally then, it may be said, that beer contains much saline matter, wines contain very little, and spirits contain none at all. Each year our knowledge of the chemical composition of these fluids will increase, and with advancing knowledge we shall obtain more clearness regarding the dietetic and medicinal properties of all fermented liquids. At present, the answer to the question which is the best wine, cannot be given, because of the imperfect knowledge which we possess of the chemical composition of different wines. But even when full knowledge is obtained, no universal answer can be given; for that wine which is best in one state, or for one person, may be the worst in a different state,

[1] 'Medical Times and Gazette,' for Sept., 1854.

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or for another person. Generally, however, it may be said, that that wine is best which contains least alcohol, least acid, least sugar, and highest flavour. This is the best for a person in health, while for a person in a state of low fever, that wine which contains least salts, most sugar, most astringent acid, and most alcohol, will cause least irritation of the bowels, will furnish most nutriment, be most febrifuge, and most supporting. Where there is much irritation and much depression, brandy will be better than wine. In some states in which diuretic action is desirable, Moselle wine, by the excess of salts it contains, may prove almost as energetic and far less heating than the essential oil in gin or whiskey. The difference in action being that between cream of tartar and oil of turpentine. In diabetes, ale and porter, which contain much sugar, cannot on rational grounds be advised, while claret, which is free from sugar, and contains perhaps much tannic acid, is highly beneficial. While cider, though free from sugar, contains much malic acid, and is diuretic. In dyspepsia and gout, the wine which is freest from ultimate acidity, and which is least stimulating, is best. The least acid claret wines, first, and then some sherry, and even perfectly dry champagne, when very free from excess of acid, are not unsuitable. When a taste is acquired for good Mansanilla, it answers all the requirements, and is obtained at a far cheaper rate than first rate Amontillado sherry, which may be procured free from all sugar, and with little acid, and scarcely stronger than Mansanilla, and so far surpasses it in flavour, that the difference in expense is almost forgotten. But by adding water to brandy, or other spirit, a fluid is obtained which is far less acid than any wine,

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and which may be made of any strength, and be free from all sugar. Theory, then, as well as practice, leads to the belief that if any stimulant is taken in dyspepsia, the best is brandy and water. With regard to porter and stout, these containing little acid, have much sugar, and hence give rise to much more acidity than some pale ale. [But at the same time are *pro tanto* more fattening if they agree.] Some pale ale may be found more sweet and acid than ordinary sherry, but generally pale ale is more acid than sherry, and less stimulating. Yet for this, compensation is made in the greater quantity of ale drank. The larger quantity of salts also in malt liquors must always be borne in mind. Such is the meagre information which at present is the best I can bring before you to enable you to answer the question: what may I drink?"

The question of how much, must also receive answers as various as are the habits and capacities of those who ask it. One man's extremest idea of temperance conveying to another the horror of a debauch. I recollect a celebrated writer, who is now far advanced in years, remarking to me in reply to my question of whether he took much wine, "I drink very little now," he said in the quietest tone, "I seldom take more than eight or perhaps ten glasses of sherry at dinner." A limit of indulgence which would have entitled him to be enrolled on the books of a temperance society instituted towards the close of the 14th century by the Landgrave of Hesse, the rules of which restricted its members to seven bumpers of wine along with each meal, which at three meals a day, and allowing six bumpers to a quart, would give each man more than three daily bottles.

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We should have here considered the effects of the other sources of fat, and especially of cod-liver oil, but that the subject has been ably handled by so many writers, as to make it a work of supererogation. There is one observation, however, it may be right to make, in order to prevent disappointment from the results of this fashionable remedy, and it is this: In cases of phthisis it may fatten the patient to such a degree as to give a delusive appearance of restored health, while all the time the mortal malady is making rapid progress to its inevitable end. As to the value of inunction, a still more fashionable mode of introducing fat into the body, I confess myself to be very dubious, from the few trials I have made of it. I have found, in several instances, unspiced turtle soup one of the most digestible forms of presenting fat to patients in a state of extreme emaciation. Of the dietetic value of cocoa, in this point of view, I shall speak presently, when considering its claims as a beverage, along with those of tea and coffee.

Before doing so, it may be right to state, that however important fat is to the economy, yet both oil and sugar are so readily decomposed and converted into a variety of acids, some of which are of a highly indigestible character, that, practically, they are usually found unsafe articles of diet, unless presented in circumstances well adapted to secure their admission without decomposition.

Sugar is safest in the form of grape sugar, and it is owing to this that the *grape-cure* owes its efficiency probably, in cases to which it is suited. It is said to be specific in certain forms of diarrhœa or dysentery, but I am not aware of any trustworthy details of the way in

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which it is used. Milk thickened with mutton suet, has an excellent effect upon children who are weak and lean from long-continued diarrhœa. And broiled bacon is found by many to be a useful substitute for butter at breakfast. No doubt in this, as in other forms of aliment, individual peculiarities of taste and disposition, impossible to specify by anticipation, must be taken into account.

The importance of coffee, tea, and cocoa, as articles of diet, requires no enforcement. Of coffee alone, the consumption in the United Kingdom, amounted, in 1853, to thirty-seven millions of pounds; and yet coffee is of comparatively recent introduction into Europe.

According to Abd Alkader Ebn-Mohammed, who wrote a treatise in 1566, entitled, 'The Prop of Innocence, in Reference to the Lawfulness of Coffee,' a very learned and pious sheikh became acquainted with this beverage in Abyssinia, and introduced it into Aden about the beginning of the fifteenth century, under the name of kahwa. In the year 1554, an alarming tumult arose in Constantinople, no less than an emeute of the priests against the sultan, because the opening of coffee-houses had been attended with such irresistible attractions to the true believers, that they had all deserted the mosques. With the sagacity of one born to rule, the sultan settled the matter by heavily taxing the coffee-houses, thus relegating to the mosques the poorer portion of those who frequented them, satisfying the priesthood, and enriching himself. In 1652, a Greek, of the name of Pasqua, opened the first London coffee-house, in George Yard, Lombard Street; and in 1671, the first

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was opened in France, at Marseilles; it was not till 1721 the first coffee-house was opened in Berlin.

Thus began the second Byzantine empire, which is likely to be more enduring and fruitful of great political results than the first. For it is hardly too much to say, that if there had been no cafés in Paris, there would have been no French revolution, or, at all events, its history would have been very different.

The most important chemical ingredients of roasted coffee are: 1st, a volatile oil produced during roasting; 2d, a variety of tannic acid, modified by this process; and 3d, a substance, called caffeine, similar to the theine and theobromine of tea and cocoa.

The physiological effects of the infusion or decoction of coffee are very peculiar. It seems to act especially upon the spinal and respiratory system of nerves, rather than on the cerebral, as alcohol does. It is well worthy of consideration, that while wine induces sleep, coffee and tea prevent it. And it may be, that the increasing number of spinal affections, which are undoubtedly met with now, are, in some measure, due to the much larger quantities of tea and coffee consumed. It has, besides, a most remarkable effect in retarding the waste of the tissues, and thus, is indirectly nutritious; it husbands the strength of the person who drinks it, and so increases his wealth: as a man's riches depend upon the excess of his revenue over his expenditure, and what diminishes the latter, augments the former. Some interesting experiments have been made upon this subject, and the result was to prove, in the instances recorded, that by the use of one and a half ounce of coffee daily, the absolute quantity of urea and phosphoric acid voided

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from the system in a day was diminished one third, while the absolute quantity of urine was increased by a fifth. If this experiment be confirmed, it shows that coffee, to some extent, gives us the control of the waste of our bodies, and when we recollect that life is an incessant stream, the rapidity of which, in a great measure, determines the amount of health and strength; that the rapid exhaustion of fever is the acceleration of the river of life in the body, and, that the arrest of decay implies increased energy and prolonged vigour, there is no wonder that those who are exposed to great wear and tear of the body, especially of the nervous ingredients of it, should cling with desperate tenacity to those beverages, which experience, now ratified by scientific observation, tells them, tends to reduce to a minimum the expenditure of substance during the efforts they are obliged to make. And, while to deprive a man of his wine may be to starve his brain and his nerves, by withholding from them their requisite sources of food, to deprive a man of his tea and coffee, may operate with equally disastrous consequences, by involving him in an expenditure to which his means are inadequate.

The quantity of tea consumed in the United Kingdom in 1853, amounted to fifty-eight millions of pounds, while in Germany, with a population of four millions more than the British, it was only one million and a half, and in France only half a million. This accounts for the ghastly parody of the refreshing beverage with which the traveller in these countries is generally mocked, and displays the absurdity of homœopathists transferring Hahnemann's inhibition against coffee to this country, while tea, which takes the same

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place here held by coffee there, is allowed to be used with certain faint admonitions against its excess. For the physiological effects of the two are very similar, depending, in a great measure, upon the presence of the peculiar highly nitrogenous principle, called theine or caffeine. The difference between black and green tea depends entirely, according to Mr. Fortune, our great authority in this matter, upon the mode of preparation. Green tea is dried rapidly, while the leaves are fresh; while black tea is exposed for some time to heat and air, which induce various unknown chemical changes in their substance, and mitigate its sleep-destroying properties. It is usual in China to prepare the green tea for the foreign market with a small quantity of copper to intensify its colour, and to the deleterious action of this substance, some of the evils of green tea are ascribed. However, recent investigations have proved, that the amount of copper employed is so minute, as hardly to be of any consequence — at least, so it is maintained.

All that has been said of the action of coffee upon the nervous system, is equally applicable to tea, and does not require to be re-stated. I believe its moderate use to be perfectly harmless, and that it in no way interferes with the action of homœopathic medicines.

The quantity of cocoa consumed in this country, although still greatly inferior to that of tea or coffee, is rapidly on the increase, being no less, in 1853, than 4,126,000 pounds, while in 1832, it was only 1,150,000. Like tea and coffee, it contains a peculiar highly azotized principle, called theobromine; the composition of this is, $N^4 C^{14} H^8 O^4$; while that of theine is $N^2 C^8 H^5 O^2$. And

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if cocoa were prepared and used in the same way as coffee, it is probable the effects would be much the same. This, however, is not the case; it is not roasted and infused, and the infusion drank, but boiled, and eaten in substance; and besides various ingredients in common with tea and coffee, it has a much larger proportion of oil, amounting to between fifty and sixty per cent. while in coffee there is only ten per cent. and in tea none at all. It is to this difference the fattening properties of cocoa are due, and this too is the reason why it is found to be indigestible by many. In Spain, and other countries where it is much used, it is not the practice to use bread and butter with it, but generally a species of rusk or roll of biscuit. In a chemical point of view, there is not much difference between a breakfast consisting of tea and bread and butter and one of cocoa, and the attribute homœopathic, applied to the latter, seems to be somewhat arbitrary, if not clap-trap. The so-called homœopathic cocoas are no way different from the other cocoas, except in being mixed with various proportions of sugar and arrowroot, or potato-flour, which lessens the proportions of oil, and improves their flavour to most palates, and their digestibility to most stomachs. On this admixture Dr. Hassall has made some very severe strictures, denouncing it as a fraudulent adulteration, on the ground that the only design the chemists who prepare the cocoa can have in view, is to increase their gains, by selling a cheaply prepared article at a high price. With his condemnation of those — and I am sorry to say they are a considerable number — who adulterate their cocoas with earths, it is impossible not to concur in the fullest extent, but I do not see that those

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who prepare a wholesome and nutritious article of food, the basis of which is cocoa, are to be blamed for selling it under the name of homœopathic, or dietetic, or any other title they fancy; as to the price, that is their affair and the public's. The whole idea of trade is gain. To buy in the cheapest market, and sell in the dearest, is the fundamental principle of commerce. Shall we blame tradesmen for obtaining the highest price for their wares? Shall we not rather say, if the price be too great, let it be brought down by competition. There is no monopoly except that obtained by superior skill, capital, and reputation.

MILK.

We have still a few words to say about *milk*.

This is called the model food, because it contains in nice proportions, albumen, gelatine, and fat. In a dietetic point of view, the chief point of interest is, to determine the kind of milk best suited to various purposes. The modification of milk called colostrum, on which the first days of the infant's life are sustained, differs from the after-milk, in containing a considerably larger quantity of all the solid parts. Thus, at birth, the caseine amounts to 40 parts in 1000, while at the end of the fourth day, it is only 35 or 36 parts, the sugar is as high as 70 instead of 41, and the butter 50 instead of 35. This is an important fact to bear in mind, when we are required to supply an artificial substitute for the natural aliment of a new-born infant. In the course of lactation, there is a gradual tendency to an increase of caseine, and a diminution of sugar and butter; at the fourth month, the

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relative quantities of these ingredients, as compared to the first fortnight, are, caseine 40 instead of 22; sugar 45, instead of 56; and butter 23, instead of 27. There is also a marked difference in the quality of the milk of fair and dark women. This observation, which is a popular notion, has been scientifically corroborated by 'Heretier, who subjected the milk of fair and dark nurses, of the same age, to chemical analysis, and found, that while in the fair the amount of solids varied from 108 to 118 parts in 1000, in the dark nurse's milk they amounted to 146 and 147 parts in 1000. The milk of the cow is nearer in chemical constitution than that of any other animal, to the human milk. By the addition of one third part of water and some milk-sugar, the resemblance is made nearly as perfect as we can expect. The milk of the ass contains much less butter than that of the cow, and for this reason it is preferred for many delicate persons.

The effects of disease upon the secretion of milk have not been much investigated, but Labillardiere has made the very important observation, that the phosphate of lime is increased no less than seven-fold, in the milk of cows suffering from a tuberculous affection of the lungs. This fact should put us on our guard, when we give an opinion as to the propriety of a mother suckling her child, and we must not allow ourselves to be induced, by the laudable efforts which are now fashionable, in favour of the mother being nurse to her own offspring, to endanger the future constitution of the infant, by supplying it with improper food at an age when growth is so much more rapid than at any future period of its life, and when, consequently, any imperfection in the

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elements on which it lives, is most likely to be attended with important results.

The publication of this paper in the 'British Journal of Homœopathy,' in the year 1855, excited much interest among the adherents of the homœopathic system in Germany. The greater part of it appeared in the 'Allgemeine Hom. Zeitung.,' and the accomplished editor of the 'Hom. Vierteljahrschrift,' Dr. M. Müller, wrote an able and liberal article upon it in his valuable quarterly journal. His article in its turn became the exciting cause of various others all more or less learned and ingenious. It would be out of the question here to do more than refer to these numerous articles, as a proof that the question was ripe for discussion, and I take this opportunity of acknowledging my obligations to the critics who have written about my imperfect little treatise, for the kindness and candour with which they have expressed their opinions when they differed, and the cordiality with which they agreed.

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CHAPTER III.

ON THE STRUCTURE AND USES OF THE LIVER.

In the annals of the liver we might read the history of science and the progress of human thought. It was probably first attentively scrutinised by the Augurs, the most primitive anatomists, and from its colour and shape political predictions were drawn, and inspiration given to the *Moniteurs* and *Times* of those days:—and

> "When science from creation's face
> Enchantment's veil withdrew,"

so far from the "glorious visions" yielding "their place to cold material laws," it was found that the more the matter was examined the greater grew the interest, and that one discovery only made way for another; so that although during the last two hundred years about one hundred and fifty treatises have been written, many of them by the greatest men of their age—such as Malpighi, Hoffman, Haller—upon the structure and functions of this organ, yet it was reserved for a man of our own time to announce a series of discoveries about the uses of the liver, of so new and startling a character as to attract the attention of the world at large, and which, if confirmed, will place the name of Bernard beside that of Haller. Interesting, in a general way, as these discoveries are to all, they must have a special interest for us whose high prerogative it is to convert all that is true

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in the science of medicine into something fruitful in the great art of healing.

The liver is the largest gland in the body, even in adult life, although then very much smaller, proportionally, than in the fœtal condition; in the human species it is one third of the weight of the whole fœtus at the end of the first month, and one eighteenth at the end of the ninth month.[1] Its average weight in a healthy man is from three to four pounds; it measures about twelve inches in length, and six to seven inches in depth; its bulk corresponds to nearly one hundred cubic inches. This great mass is made up of a multitude of lobules, and the variation in its dimensions in different classes of animals depends much more upon the number of lobules than upon their size. Between the lobule of the liver of a mouse and of an elephant there is but a comparatively trifling difference, the enormous magnitude of the latter depends upon the enormous multitude of lobules. In some animals, as the pig, these lobules are distinct, but generally they are only discoverable by dissection. Each lobule may be looked upon as a little liver, and the anatomy of one is the anatomy of the whole. The fundamental requirements in a lobule are three-fold: 1st, Vessels to bring blood to it; 2d, Cells to operate upon that blood; and 3d, Vessels to carry away what has been analysed by the cells. The analysis consists in the separation of bile, which flows off by the biliary ducts, and the formation of sugar, which passes into the venous blood and is carried on to the heart and lungs. The first set of vessels, or those which supply the liver, are the hepatic artery and the portal vein. The

[1] Burdach, vol. iii, p. 483.

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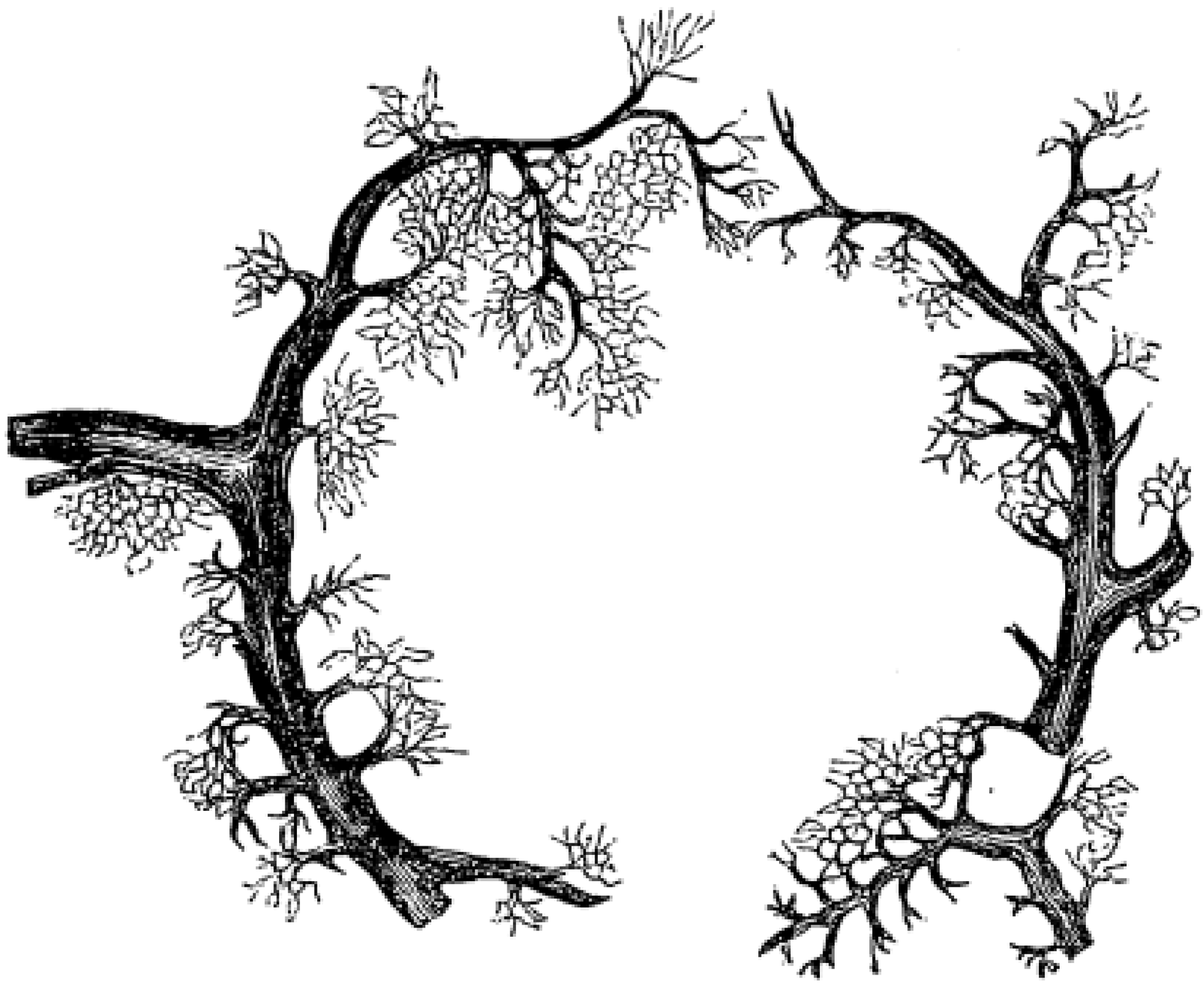
hepatic artery, a branch of the abdominal aorta, is the feeder of the liver; as such, it is distributed upon the coats of the ducts and capsule: after discharging its functions there its contents are collected by venous radicles and discharged into branches of the portal vein. The portal vein is formed by a union of all the veins of the intestines, except those of the kidneys and bladder. It comes direct from the seat of digestion, and contains

Fig. *a*.



LOBULE OF THE LIVER, OF A PIG.

This represents the branches of the portal vein in the liver of a pig forming an imperfect circle round the centre of the lobule where the hepatic vein lies, but which is not represented in this plate.

the blood in its rawest condition. Its distribution in the liver is very peculiar, and unlike that of any other vein in the body. The ordinary office of veins is to collect the blood, not to distribute it, and in their course the branches naturally diminish in number and increase

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in size; but the branches of the portal vein ramify like an artery, and encircle the lobules—as represented in this woodcut.

The cells of the liver are the seat of the vital force which operates incessantly upon the blood, inducing in it, in a wholly mysterious manner, those molecular changes which it is the appointed office of the organ to effect in the general economy of the body. These cells are very minute, varying from 1-800th to 1-2000th of an inch in diameter.[1] Each cell contains a nucleolus of a circular or oval form, and about 1-3000th of an inch in average diameter. Besides these nucleoli the cells contain oil-globules and granules of yellow coloring matter, and are remarkable, as compared with other gland-cells, for the variety in their form and size, and their high refractive power. It has been, and still is, a disputed point, whether these little cells lie simply imbedded among the capillary vessels, or enjoy a private dwelling-place in what is called a basement membrane. Dr. Beale is decidedly in favour of the latter opinion, and represents the cells as occupying a tubular network radiating from the centre towards the circumference of the lobule like a spider's web. In the fœtus the distinction between the capillary and the tubular network is manifest, but the partition walls being of the most delicate material, what was originally an interlacement of tubes, tends to become a common cavity, and it is very difficult to demonstrate this arrangement in the liver of the man. Whether such a net of fine tubes containing these secreting cells exists or not is not apparently a question of any great import-

[1] Dr. Handfield Jones, 'Philos. Trans.,' 1846–49–53, and 'Med. Chirurg. Trans.,' 1852.

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ance, except to the anatomist. It is enough for the physiologist that a mechanism exists by which the blood of the portal vein, loaded with the drainage of the stomach and bowels, should be exposed to the action of these resolving cells by which its division into blood and bile is effected. How it happens that the bile should percolate the walls of the capillaries and the blood flow onward, belongs to the elective affinities of the organism, which must be recognised as an ultimate fact, at which we may wonder, but which we can never comprehend.

We have now arrived at the centre of the liver, to which the blood has been conveyed by the ramifications of the portal vein, to be exposed to the influence of the cells and decomposed. Let us trace the course of the vessels which carry out of the liver the two fluids separated from the portal blood. These vessels are the hepatic duct and the hepatic vein. The hepatic duct is a tube of a firm fibrous structure, dividing into branches which lie alongside the branches of the portal vein. In the fibrous walls of these tubes are a row of small apertures, which, on being traced, are found to lead into a corresponding number of little pouches. It has hitherto been generally believed, that these pouches were for secreting the mucus of the bile, but, as Dr. Beale acutely remarks, a pouch with a very narrow neck would be a bad contrivance for a viscid secretion, besides, in those animals in whose livers these pouches are most numerous there is no corresponding increased viscidity of bile. It is more likely that they are diverticula or little gall-bladders. This view seems highly probable when we consider the sudden and very copious secretion of bile which is observed to follow the taking of food. For ex-

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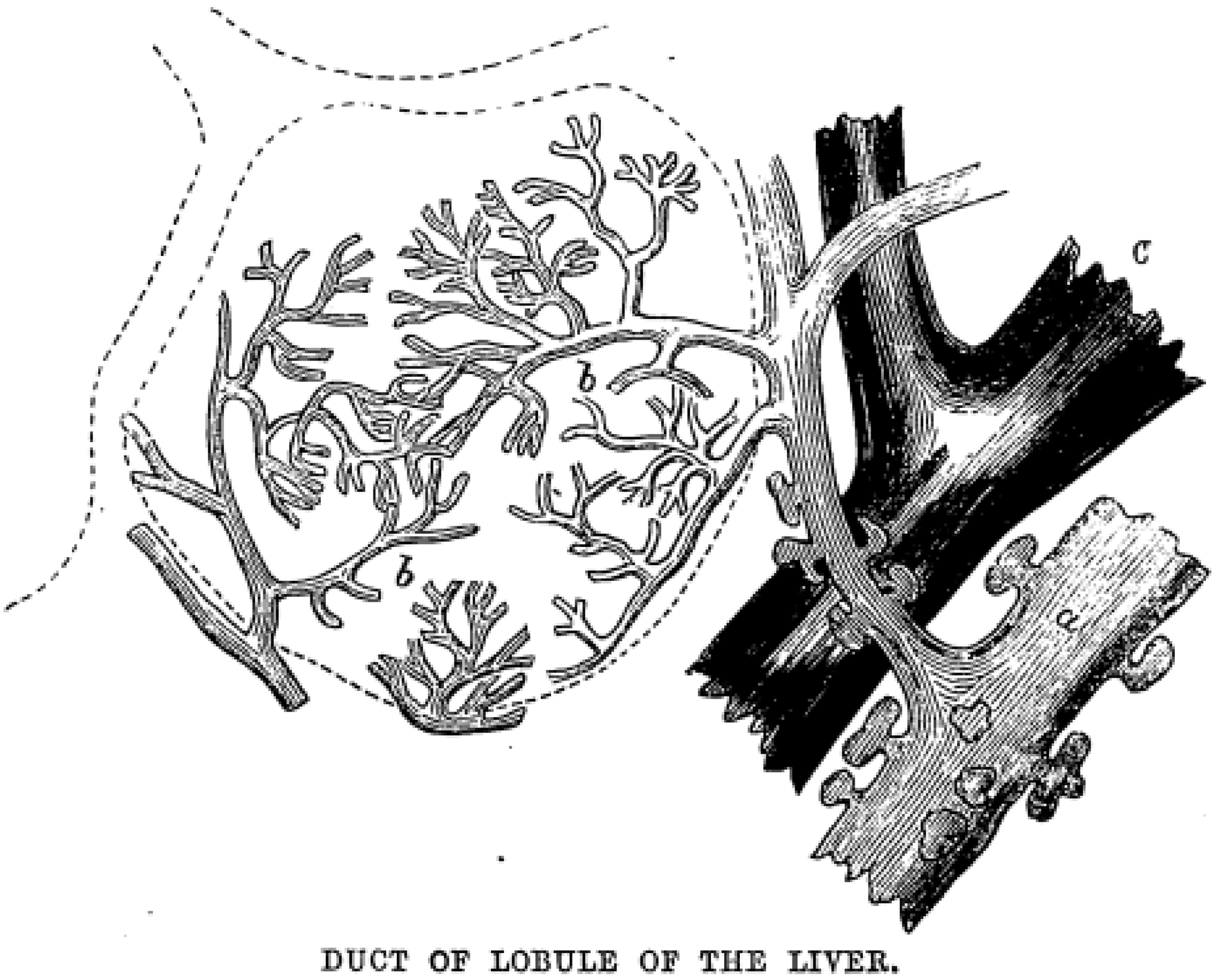
ample, Bidder and Schmidt (p. 166) observed, that while a dog fasting yielded 1558 grammes of bile in an hour, the same dog, after drinking 185 grammes of water, yielded 5165 grammes of bile in the same time. The manner in which the branches of the hepatic duct terminate has been much canvassed, and the opinion of many most distinguished investigators was in favour of the termination consisting in blind extremities; Dr. Beale, however, maintains that these ducts which carry off the bile are directly continuous with the tubes which contain the cells where the bile is secreted, and that he has succeeded in injecting the latter from the former. He has given some plates in which this is shown.

Fig. *6.*



DUCT OF LOBULE OF THE LIVER.

This represents the distribution of the duct upon the surface of a very small lobule of a pig's liver. *a*. Duct with little sacculi in its coats. *b*. Small branches of the duct which are distributed upon the surface of the capsule: these are represented at least twice as wide as they ought to be. *c*. Inter-lobular branch of portal vein.

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That the bile is formed in these cells and does flow along these biliary tubes, whether entering them by a free opening or by percolation through their walls, is a certain fact; but it is not known by what force the bile is propelled. Some have assumed it to be from the pressure of the full stomach upon the liver, but this notion is refuted by Bidder and Schmidt, who observed the contents of the gall-bladder and the biliary tubes to begin to discharge rapidly in a cat some hours after a meal; their opinion is that it is by a contractile power of the tubes themselves, although they have not been able either to detect muscular fibres in them, or to succeed in making them contract by galvanism. And they attribute much importance to the descent of the diaphragm during respiration. The question, however, is by no means satisfactorily settled as yet, and demands farther investigation. The office of the gall-bladder is obvious enough, being simply a reservoir of bile which is emptied into the intestines at a short interval after a meal. The bile collects in it after digestion ready for a recurring demand. In connection with the hepatic duct should be noticed a system of vessels accurately described by Weber, of Leipsig, and named by him *vasa aberrantia*. They are most numerous at the transverse fissure of the liver, where they form an intricate net-work communicating with the larger branches of the biliary duct. By means of this vascular reticulation the ducts are connected with one another—they are, in fact, the anastomosing apparatus, and have been injected by Dr. Beale not only in the transverse fissure, but high up in the portal canals. They are thickly set with cœcal pouches, which are probably not mucous glands, but, like those of the duct

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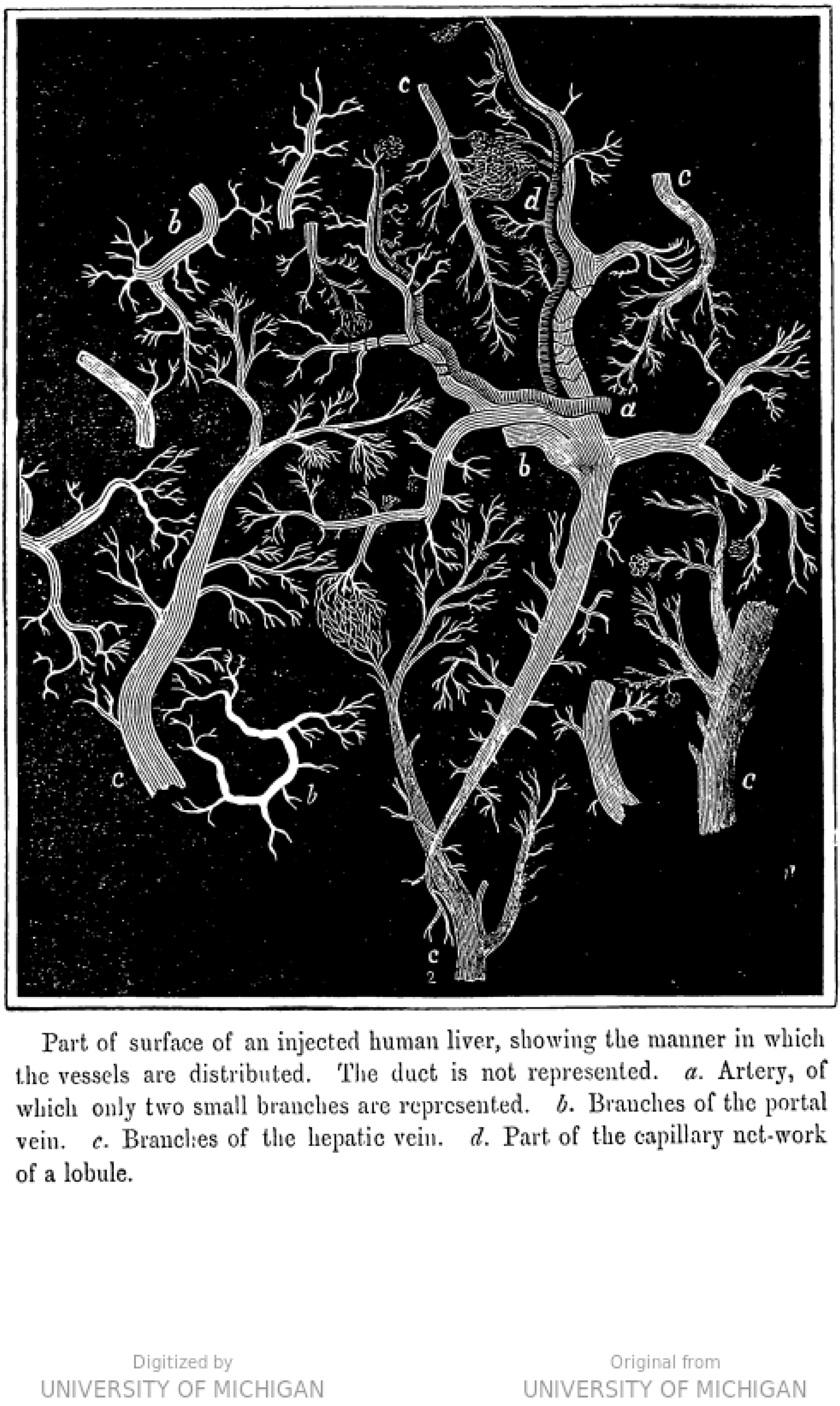
itself, little reservoirs of bile or gall-bladders in minature.

Fig. *c*.



Part of surface of an injected human liver, showing the manner in which the vessels are distributed. The duct is not represented. *a*. Artery, of which only two small branches are represented. *b*. Branches of the portal vein. *c*. Branches of the hepatic vein. *d*. Part of the capillary net-work of a lobule.

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The distribution of the hepatic vein is the converse of that of the portal. The portal divides in its course into diminishing branches and ends in capillary vessels. In these capillaries the hepatic vein takes its origin, and the smallest of its ramifications is larger than the extremities of the portal vein: thus from the first its special office of a collector, not a distributor, is indicated. Its position is about the centre of the lobule; into it the blood from the portal vein is poured through the capillary vessels lying in immediate contiguity with the liver cells, so that in its passage from the portal to the hepatic vein it has a full opportunity of being subjected to their influence. The branches of the hepatic vein unite and enlarge as they proceed outwards to the circumference, and in some animals, as the seal, which require to abstain long from breathing, they form sinuses capable of containing a large quantity of blood, so as to prevent the gorging of the liver on one hand, and of the heart on the other. After uniting into large branches they pour their contents into the *vena cava inferior*, the largest vein in the body, which enters the right auricle of the heart, whence it is immediately passed on to the right ventricle, and from thence to the lungs to undergo further changes. What we may call hepatic blood then is to be found only in the hepatic veins, and in a diluted condition in the vena cava inferior.

Lymphatic vessels are met with in abundance in the portal canals and transverse fissure, and materially contribute to the capacity of the liver for receiving a large addition to its fluid contents without inconvenience. Thus, for example, a pig's liver may be easily made to contain a quantity of water equal to its own weight, which may be injected from the portal vein.—(*Beale.*)

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We have now described the mechanism of the liver, which may be rudely likened to a sponge placed between the stomach and the lungs, receiving the blood from the one organ and passing it on, after certain changes, to the other. To enable it to effect these changes, however, a supply of nervous force is required, and this it derives from two sources: one the cerebro-spinal, and the other the sympathetic. In the human subject this double origin is not easily demonstrated, for the cerebro-spinal nerve, the pneumogastric, is lost in the solar plexus on the right side, where it becomes blended with the great sympathetic, and on the left it is distributed on the stomach. In the dog and rabbit, however, the two pneumogastric nerves, after penetrating the diaphragm, give off distinct twigs, which accompany the hepatic artery, and become lost in the intimate structure of the lobules.—(*Bernard*, p. 323.) We may fairly presume that they proceed as far as the capillary network and the secreting cells. It also receives branches from diaphragmatic, which are distributed chiefly upon the walls of the hepatic vein. The explanation of the functions of the liver, both in health and disease, hinges so much upon the fact of the double supply of nerves, that we give a woodcut from Bernard's work in which it is shown (from p. 322).[1]

[1] For the whole of the facts and the woodcuts contained in this paper we are indebted to the following interesting and important Works:

1st. 'On Some Points of the Anatomy of the Liver of Man and Vertebrate Animals,' by Lionel S. Beale, M.B., Lond., 1856.

2d. 'Leçons de Physiologie experimentale appliquée à la Médecine, faites au Collège de France,' par M. Claude Bernard, &c. &c., Paris, 1855.

3d. 'Die Verdauungssaefte und der Stoffwechsel. Eine physiologisch-chemische Untersuchung von Dr. F. Bidder und Dr. C. Schmidt, Professoren in Dorpat. Mitau und Leipzig, 1852.

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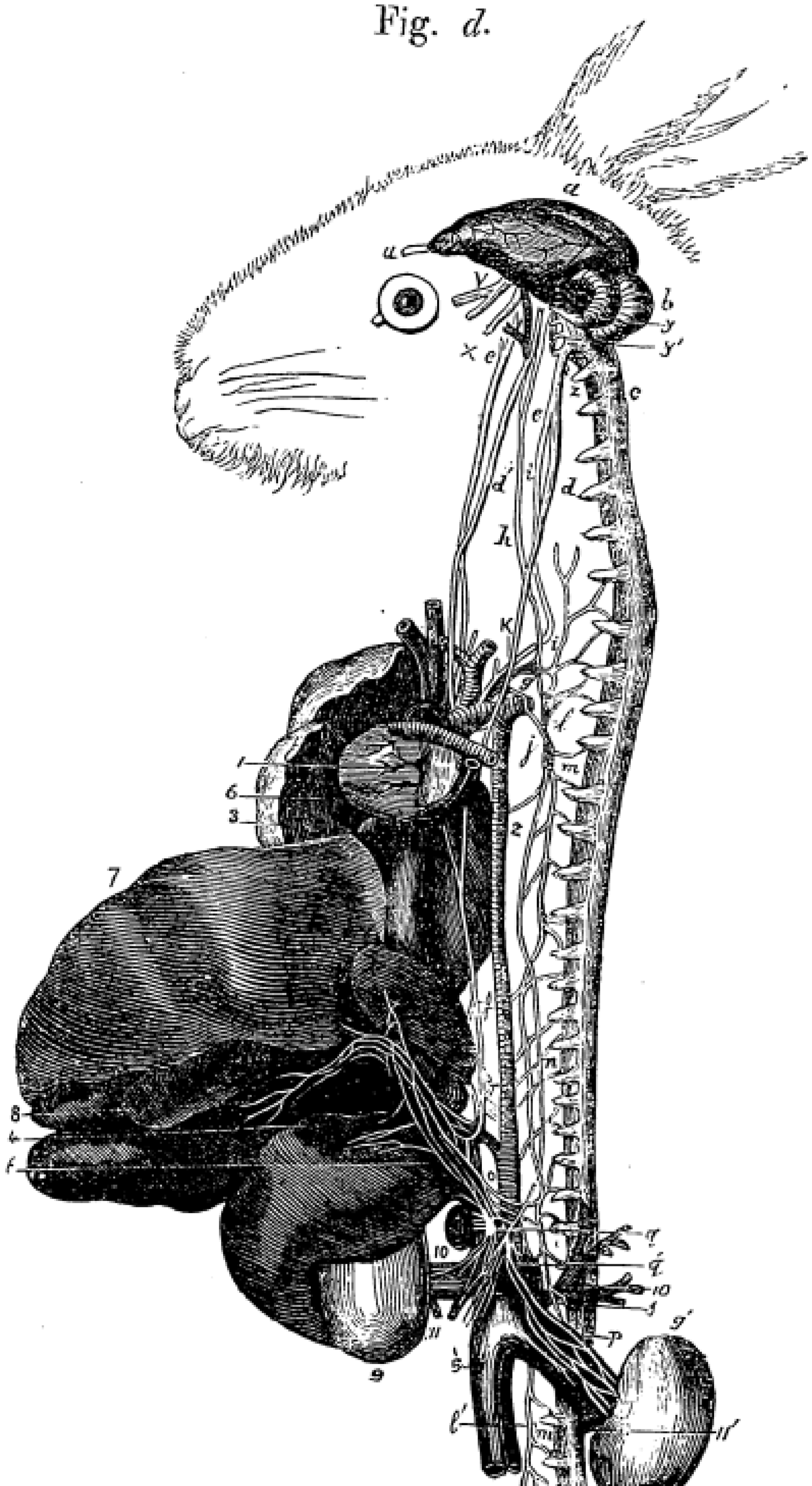
Fig. *d*.



Representing the cerebro-spinal axis and splanchnic nerves of a rabbit and showing the relation of the nerves of the lungs, liver, &c. *a*. Brain.

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b. Cerebellum. *c c*. Spinal cord. *d d*. Trunk of the pneumogastric nerves. *e*. Upper cervical ganglion. *f*. Common trunk of the two pneumo-gastric nerves, reunited below the lungs. *g*. Inferior cervical ganglion. *h*. Filament of connection between the superior and inferior cervical ganglia. *i*. Cardiac filament of the pneumo-gastric. *j*. Cardiac filament going from the first thoracic ganglion of the great sympathetic. *m m*. Filaments of connection of the different ganglia of the great sympathetic with one another. *l*. Thoracic ganglion. *n*. Filament of connection between the spinal cord and the great sympathetic. *o*. Hepatic nerves going from the great sympathetic. *p*. Renal nerves going from the great sympathetic. *q q*. Ganglia of the solar plexus. *r*. Hepatic nerves going from the pneumogastric. *s*. Renal nerves. *t*. Hepatic nerves united. *u*. Olfactory nerves. *v*. Optic nerves. *x*. Nerves of the third pair. *y*. Nerves of the fifth pair. 1. Heart. 2. Aorta. 3. Vena cava. 4. Portal vein. 5. Inferior vena cava below the renal veins. 6. Lungs. 7. Liver. 8. Gall bladder. 9 9. Supra-renal capsules. 10. Kidney. 11. Ureters.

Let us now inquire into the functions of the liver, or what changes the blood undergoes in its passage through its intricate net-work of vessels on its way from the stomach to the lungs. An obvious plan of commencing this inquiry, is to subject the two kinds of blood which we call the gastric and hepatic to a careful chemical analysis, so that the difference between them may be ascertained. This has been done by Professor Lehmann, of Leipzig, whose paramount authority on questions of organic chemistry is universally acknowledged in the scientific world, and the result was communicated on the 12th of March, 1855, to the French Academy. As the matters this memoir embraces belong to science militant as distinguished from science dogmatic, it may be well to give a literal translation of the whole of Lehmann's short and satisfactory paper.

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"*Comparative analysis of the blood of the vena portæ and of the blood of the hepatic veins, &c., to serve as a history of the production of the sugar in the liver, by* M. C. G. LEHMANN, *Professor of Physiological Chemistry in the University of Leipzig.*

"The results of the following analyses have been obtained from dogs and horses taken in different states of alimentation, (care has always been taken to place ligatures round the vessels to prevent a mixture of the kinds of blood under examination). I will not delay by describing the analytical processes, which will be found in my treatise on organic chemistry; and I shall likewise abstain from certain details of the composition of the blood of the portal and hepatic vein contained in my published memoir ('Einige vergleichende Analysen des Blutes der Pfortader und der Lebervenen'). I shall only here insist upon the points which serve to exhibit the formation of the sugar in the liver.

"1st. *Sugar.*—The blood of the vena portæ never contains the slightest trace of sugar in dogs in a fasting condition, or those fed on animal food. In the same animals fed on vegetables (such as boiled potatoes), sugar is discernible in the portal blood, but in quantities so minute as to be incapable of measurement. In horses fed upon rye-bran, chopped straw and oats, the portal blood contains slight traces of sugar. In one case I found gr. 0·055 of sugar to 100 grains of blood. In another case the serum of the portal blood of a horse contained gr. 0·052 of sugar in 100 grammes of blood.

"The blood of the hepatic veins always contains sugar

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in considerable quantity. In three dogs fed upon animal food, I found the following proportions of sugar to dry blood: gr. 0·814 to 100, gr. 0·799 to 100, and gr. 0·946 to 100. In other three dogs which had for two days been in a state of complete abstinence, I found in the blood of the hepatic veins the following quantities of sugar: gr. 0·764 to 100, gr. 0·638 to 100, and gr. 0·814 to 100. In two other dogs fed upon cooked potatoes, the blood of the hepatic veins contained gr. 0·981 of sugar to 100 of blood in one, and gr. 0·854 to 100 in another. In two horses fed on vegetable food (hay, straw, and oats), the hepatic veins contained in one case gr. 0·635 of sugar to 100, and in the other gr. 0·893 to 100. The following table exhibits the results of the preceding analyses.

Animals.	Conditions as regards Food.	QUANTITY OF SUGAR.	
		In the blood of the vena portæ on its entrance into the liver,	In the blood of the hepatic veins as they leave the liver.
		Grammes.	Grammes
Dog	Fasting for two days.	0·000	0·764 to 100
,,	,, ,,	,,	0·638 ,,
,,	,, ,,	,,	0·804 ,,
,,	Fed on animal food.	,,	0·814 ,,
,,	,, ,,	,,	0·799 ,,
,,	,, ,,	,,	0·946 ,,
,,	Fed on potatoes.	A trace, too small to estimate.	0·981 ,,
,,	,, ,,	,,	0·854 ,,
Horse	Fed on hay, straw, and oats.	0·055 to 100	0·893 ,,
,,	,, ,,	0·052 to 100	0·635 ,,

"It is enough to cast the eye upon the comparative quantity of sugar contained in the blood of the vena portæ and the hepatic vein, to see that the opinion of

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the formation of sugar in the liver which M. Bernard was the first to announce, is put beyond a doubt.

"2d. *Fibrine. Albumen.* — The blood of the vena portæ in horses and dogs contains fibrine which does not sensibly differ either in kind or quantity from the fibrine of other veins. The blood of the hepatic vein if carefully collected without admixture of other blood, contains no fibrine. The few flocculi which are sometimes obtained by beating up the hepatic blood of the horse, are almost entirely composed of white globules, which are very abundant in the blood of the hepatic veins as compared to that of the portal vein. The blood of the hepatic veins of the dog exhibits the same character as regards fibrine, that is to say, this substance almost entirely disappears in the liver.

"Very careful comparative analyses of the blood of the portal and hepatic veins, have convinced me that a remarkable quantity of albumen also disappears in the liver, and that this quantity is relatively greater in dogs than in horses: upon the incontestable fact of the disappearance of the fibrine in the liver, I have established the opinion already expressed in my first memoir, that the sugar formed in the liver is derived from the fibrine.

"3d. *Fat and Blood globules.* — The blood of the vena portæ always contains much more fat than that of the hepatic veins. The serum of the portal blood in dogs fed upon animal food, is generally much richer in fat than that of horses; notwithstanding this, no more fat is found in the serum of the hepatic blood of dogs than of horses. In horses the blood-globules of the

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portal vein are much richer in water, and especially in iron, and on the other hand much poorer in globuline, extractive matters, and salts, than those of the hepatic veins. In dogs as well as horses the blood of the hepatic veins is much richer in blood-globules and extractive matter, than that of the portal vein. I have observed in dogs as well as horses, that a considerable quantity of iron always disappeared from the blood in its passage through the liver. But the difference in the quantity of iron found in the blood at its entrance into, compared with the quantity at its exit from the liver, is greater in dogs than in horses. It results from this that a portion of the hematine of the blood disappears in the liver, and probably goes to the formation of the colouring matter of the bile, which would prove again the complete analogy between bilifulvine and hematoïdine, as one of my pupils is about to demonstrate.

"*Comparative analysis of the blood of different veins with arterial blood* (*all the comparisons have been made from blood taken from the same horse.*) The blood which leaves the liver by the hepatic veins, always contains incomparably the most sugar of any in the body. This blood mixes with that of the *vena cava inferior* to mount to the heart. I can here but confirm the statement long ago made by M. Cl. Bernard, that the blood of the *vena cava inferior* is that which always contains the greatest amount of sugar after the hepatic veins. I found in the solid residue of the blood of the vena cava of a horse, sugar to the amount gr. 0·346 to 100, gr. 0·211 to 100, and gr. 0·492 to 100 of blood. After the blood has passed through the lungs and become arterial, no sugar is to be detected as a general rule. I

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have never found any in the arterial blood of horses, although they had been fed upon starch and oatmeal. In that of dogs and rabbits, sugar could be detected in arterial blood, if the venous blood contained more than 0·3 per cent. of it. This is the case in all the conditions in which sugar is passed with the urine; for example, in those instances after Dr. Bernard's operation, after the injection of large quantities of sugar into the veins or into the stomach, or finally in the instances of rabbits which had eaten largely of carrots and beet root. But in all these instances it is always the hepatic vein which contains most, then the *vena cava*, &c.

"The blood of the small veins, such as the cephalic, digital, temporal, and external abdominal of the horse, contains always a smaller quantity of blood-globules and more serum, and therefore more water than arterial blood. But the blood of the larger veins, especially the vena cava inferior, contains blood of the same or even greater concentration than the arterial. All my experiments go to prove that a remarkable quantity of blood-globules disappears in the general capillaries of the body. The equal or superior density of the blood of the *vena cava inferior* as compared with the arterial blood, is not due altogether to the expulsion of water by the urinary secretion, but is chiefly owing to the influx of the blood from the hepatic vein. This was shown in a striking way by the analysis of the blood of a horse which had not drunk for twenty-four hours before it was killed. The comparison of all these analyses goes to prove that two functions proceed simultaneously in the liver,—the formation of sugar, of blood-globules, and of bile, as M. Bernard has long ago established. The blood of the smaller veins

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contains more fibrine than the arterial blood, and than that of the vena cava and the jugular vein. In the vena cava I found only one half the quantity of fibrine that I found in arterial blood. The arterial blood always contains more mineral salts than the venous blood."

This Leipzig manifesto embraces and exhausts all the chemical elements of the question. It demonstrates the fact that the blood comes to the liver rich in fibrine, albumen, and fat, wholly devoid of sugar, and comparatively poor in blood-globules; that the blood which leaves the liver always contains a quantity of sugar, and is rich in blood-globules but not in fat, while the bile is full of fat; hence the inevitable inference that in the liver-cells the blood coming from the seat of digestion is decomposed, the fibrine certainly, and probably also some of the albumen transmuted into sugar, and passed onwards by the hepatic vein towards the lungs, and the fat and some other ingredients separated and expelled in the form of bile. This transmutation of fibrine into sugar is utterly opposed to the theories of Liebig, which have met with so general an acceptance hitherto, that no such power existed in the animal economy, but that all the chemical principles of the nature of sugar were only absorbed, and never generated by the purely vital forces. It would be out of place to follow up to its possible consequences this important revolutionary doctrine promulgated by Bernard, and established by Lehmann and other chemists. We have enough at present on hand in stating merely the bare facts, and before passing to the consideration of the conditions which regulate the manufacture of sugar, and the uses this sugar subserves in

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the economy, it will be well to say a few words about its chemical character.

There are many kinds of sugar met with in both the vegetable and animal kingdoms, but for practical purposes they may all be divided into two classes, according to the action of acids and alkalis upon them. The first-class sugar, or *cane sugar*, is found in the sugar cane, beetroot, the maple, and many other vegetables. Its chemical equivalent is $C_{12} H_9 O_9 + {}^2HO$. When brought into contact with the lining membranes of a calf's stomach, or with caseine, it is transformed into lactic acid. When boiled with diluted sulphuric acid this cane sugar is converted into sugar of the second class, known as *grape sugar* or *glucose*. This is the sugar found in the liver and hepatic veins. It differs from cane sugar in being readily acted on by alkalis, while cane sugar is not, and not being affected at all by weak acids, which decompose cane sugar rapidly. Alkalis convert it into a brown matter, and strong sulphuric acid dissolves without charring it, and forms sulphosaccharic acid. "Notwithstanding the fact," says Professor Gregory,[1] "that cane sugar is easily converted into grape sugar, and that the formulæ differ only by three equivalents of water, it is evident that these two kinds of sugar differ more than if they were merely different hydrates of the same compound. Strong mineral acids instantly decompose cane sugar, but have little action on grape sugar; while alkalis which combine with cane sugar to form crystalline compounds, rapidly convert grape sugar into dark compounds, glucic, and melassic acids. And though both sugars agree in

[1] 'Outlines of Chemistry,' p. 389.

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undergoing the same (vinous) fermentation, yet it is most probable that cane sugar before fermenting becomes grape sugar." The most certain test by which the presence of sugar can be established, is its fermentation; when mixed with yeast or diastase, it is transformed into alcohol, which may be obtained pure by the distillation of the residue, and carbonic acid which may be collected and tested as it escapes. In all instances where Cl. Bernard affirms the existence of sugar anywhere, he had ascertained it by this infallible experiment, which he performed by a simple apparatus consisting of a tube into which the substance to be tested was put in contact with yeast, while a smaller tube was passed down to the bottom of the first, and curved over it at the other extremity, where it passed through a firmly fitted cork. Thus when fermentation took place, the liquid was forced up through the small tube, and its alcoholic character determined, and the gas remaining behind was also examined. As some of the objections to M. Bernard's experiments take a chemical direction, and deny the sufficiency of his analyses, it is right at the outset to put this to rest by stating that the sugar he asserts to exist was not known only by its action on copper, &c., but by its property of fermenting.

M. Bernard examined the livers of mammalia, birds, reptiles, osseous and cartilaginous fishes, and molluscous animals, and in all he found sugar. The quantity is seldom greater in health than 4 parts of sugar to 100 of fresh liver, and the average is between 1 and 2 per cent. The opportunities of investigating this subject in man are very rare, for it is only found in the bodies of those who die in a state of health, and soon after death.

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These conditions are seldom to be met with in ordinary circumstances except in criminals who have been executed. All the experiments that have been made are here represented.

Date of observation.	Age.	Alimentation.	Weight of the liver.	Quantity of sugar per cent.	Total quantity in liver.
			Kilog.	Grammes.	Grammes.
1. 22d May, 1850	43	Fasting	1·300	1·79	23·27
2. 1st Feb. 1851	45	,,	1·330	ascertained	not calculated
3. ,, 1851	,,	,,	1·175	not weighed	not weighed
4. ,, 1851	22	Mixed.	1·200	2·162	25·704
5. ,, 1851	,,	,,	1·175	ascertained	

In a sixth instance, that of a man killed by a musket shot, although in consequence of the examination not being made till two days after death, the liver was in a state of incipient putrefaction, yet it contained gr. 1·10 to 100 parts, and the total calculated to exist in the whole liver, was 17·10 grammes. If these results stood alone it might give rise to the suspicion that the presence of sugar in the liver of a person dying by violence was an attendant upon the mode of death, and did not represent the natural contents of that organ. But when we take into consideration that in all the animal kingdom the liver is found to contain sugar, and that it disappears very readily under the influence of very many morbid conditions, the natural and fair inference is, that man is not an exception to the general rule, but that what is exceptional, is having an opportunity of examining the body of a healthy person recently killed; and as when this exception is met with, it corresponds with what our previous knowledge of the liver of animals led us to

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expect, the exception in this case certainly confirms the rule.

That the character of food has not much influence upon the quantity of sugar contained in the liver, is shown by the following table:

	Per centage of sugar in liver.
1st Dog fed on animal food . . .	1·90
2d „ „ . . .	1·40
1st Dog fed on animal food and bread .	1·70
2d „ „ . . .	1·30
3d „ „ . . .	1·30
1st Dog fed exclusively on fæcula and sugar	1·88
2d „ for 6 days exclusively on sugar	1·50

In all these examples the dogs were killed at the same period of their digestion, and the experiments show that the nature of the food made only a fractional if any difference in the quantity of sugar. That a sugar diet had hardly increased the sugar in the liver, and that a purely animal diet did not diminish it. In carnivorous animals there is a great difference in the quantity of sugar which passes into the blood at different times. When digestion is not going on, the quantity delivered by the hepatic veins is about 1 per cent., which when this blood is further diluted in the right auricle is much diminished in proportion, and entirely disappears after the blood passes through the lungs; but after a full meal the liver is filled like a sponge, and its activity greatly increased, so that there is a great augmentation in the quantity of sugar *it delivers*, not all secreted at the time, perhaps, and when a certain excess arrives at the lungs, it is not

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all destroyed there, but some passes into the arterial capillaries, and is found in the excretions.

If the early appearance of a function in the fœtus, and its universal distribution over the whole animal kingdom be a proof of its importance, then unquestionably the sugar function in the liver must rank very high among the vital actions, and such being the case, it is surprising to find that it is so easily arrested. It seems that all morbid states attended with fever, immediately put a stop to the formation of sugar. This is the explanation of the disappearance of sugar in diabetic patients when they are attacked by some acute disease, a change in their condition liable to be mistaken for an improvement, although it is generally the reverse. It is probably to this arrest of the sugar formation in the system by fever, that the rapid loss of strength is due in that process, for, as we shall afterwards see, the sugar seems to be one of the primary requirements for the renovation of the body; and we all know how wonderfully rapid is the decline of strength in fever, so much beyond what any mere loss by perspiration or otherwise could account for. It is not that the body wastes; properly speaking, it is that it is not made.

Besides acute disease, many external unnatural conditions have the same effect. If an animal be exposed to intense cold, either by immersing it in a freezing mixture, or by coating its surface with varnish, so as to destroy the non-conducting efficacy of its fur, in the course of a few hours there is a total disappearance of sugar from its liver. Heat at first augments the quantity of sugar produced; but if intense and long continued, it exerts the same influence upon it as cold. As a general

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rule, abstinence from food lessens the quantity of sugar *pari passu* with the diminution of the whole mass of blood; but the sugar entirely disappears a considerable time before the death of the famished animal. However, there is a remarkable exception to this rule in the case of hybernating animals. In a marmot the quantity of sugar was found to be in its usual abundance after thirty-nine days' total abstinence. In the stomach of this marmot a neutral brown fluid was formed, and from this Bernard supposes the blood derived the materials for having sugar made at the liver. In other animals long before death by starvation the stomach is perfectly empty.

It is rather singular that while the liver should be so delicately affected by general causes which affect the whole system, such as fever, as to have its glycogenic function rapidly arrested, yet that the formation of sugar should go on with unimpaired vigour when the whole substance of that organ presented the characters of the well-known *foie gras*.[1] Even the existence of cysts, hydatids, and cancer in the liver, does not prevent the formation of sugar in those parts which are not the immediate seat of the disease. Endowed apparently with remarkable general sympathies in respect to other organs, especially the lungs and spinal cord, it seems to have little internal relationship with itself. This may be accounted for by its anatomical structure; and it may, perhaps, be looked upon rather as a congeries of little formations depending on central influences, like a group of island dependencies upon an empire, than as one large

[1] Lereboullet sur le Foie Gras. 'Mém. de l'Acad. de Méd.,' Paris, t. xvii, p. 477.

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independent organ. Be the explanation what it may, the facts that a liver greatly diseased may yet continue to produce sugar, and that this important operation is instantly suspended whenever fever invades the frame, are of paramount practical interest.

We have already given a tabular view of the comparative effect of different kinds of diet upon the formation of sugar. This, however, was rather to establish the fact, that the sugar which was found was not contained in the aliment. The influence of various articles of food upon the glycogenic function of the liver has been more fully examined, and yielded interesting results. In the first place, Bernard found that fat is incapable of conversion into sugar. Dogs fed on fat soon lost all their sugar; so that, in a certain superficial way, the dietary of Rollo for diabetic patients is justified by these experiments. No doubt feeding a person on fat will prevent the formation of sugar at the liver, and consequently cause its disappearance in the urine; but as diabetic urine is merely a symptom of some profound lesion of the nervous system, probably the so-called cure is nearly as deceptive as is the disappearance of the sugar in a diabetic patient attacked with acute disease. In neither case does the absence of the symptom indicate the non-existence of the thing signified. The fog that prevents the danger-signal from being seen does not contribute to the safety of the line, nor does the suppression of indications of danger by allopathic expedients secure the safety of the patient.

In the second place, he found that pure gelatine and albumen, so far from reducing the quantity of sugar below its average amount, had rather the opposite effect. This, how-

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ever, although at first it excited his surprise, is what we should now expect to have been the result. But the experiments with the remaining one of the three primitive elements of diet, the sugar and starch group, gave most unexpected results. He found that, although at first dogs did not like an exclusively starch and sugar diet, yet after a few days they got used to it, and it seemed to nourish them; and on examining the liver, there was no excess of sugar found there, but almost exactly the same quantity as when the dogs had been fed exclusively on albumen. One of the dogs was fed for three days with potatoes and starch, and killed three hours after the last meal. In the liver the proportion of sugar was 1·88 to 100 parts; but the liquid obtained from the liver, instead of being clear, as is usually the case, was of an emulsive and opalescent appearance. The sugar had been absorbed from the stomach and intestines, and had been carried by the vena portæ to the liver, but it had not there been converted into the true diabetic or grape sugar, and continued to mix with the blood in a sort of transition state between cane and grape sugar; no more than the average quantity of hepatic sugar being formèd out of this exclusively sugar diet. This demonstrates the futility of the attempts made to cure diabetes by withholding feculent food. Whether animals live on starch or albumen makes little difference in the amount of sugar delivered by the hepatic veins into the vena cava, but other conditions have an immense influence, and chiefly the nervous system.

The action of the nervous influence upon the liver has been demonstrated by Dr. Bernard, by a series of experiments not less distinguished for ingenuity of design than

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for anatomical expertness in their execution. The liver, as we have seen, is chiefly supplied by the pneumogastric nerves. These nerves have their origin in the medulla oblongata, behind the olivary and very near the restiform bodies, by one or two rows of filaments from five to six lines in length, and disposed in regular order. After leaving the skull they accompany the internal jugular vein; on entering the thorax there is a difference in the distribution of the right and left pneumogastric nerve. However, they are both lost in a plexus of nerves situated upon the stomach. From this plexus the liver derives its nerves. Before descending to the organs of the abdomen, however, it gives off large branches, which supply the lungs. In the rabbit this nerve gives off a distinct branch after supplying the lungs, which goes to the liver; so that in that animal there is a double nervous distribution, the one direct from the main trunk of the vagus, the other from the plexus of the stomach.

When the pneumogastric nerves are cut in the neck, and the animal is killed on the day after the operation, no sugar is found in the liver. The first explanation of this effect that offers itself is naturally that the nerves so divided were the channels by which a vital force was conveyed, which the liver required to enable it to convert the fibrine of the blood into sugar. In that case substituting a current of galvanism might have the same effect. This was accordingly done, and Bernard found that when the galvanic stream was directed downwards to the liver it had no influence, but if sent upwards towards the origin of these nerves it restored in an exaggerated

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degree the glycogenic action of the liver. The following explanation of this was proposed. The pneumogastric nerve is a mixed or compound nerve; on the one hand it transmits an influence from the centre to the circumference, giving rise to muscular contraction in the œsophagus and to secretions in the stomach; while on the other hand it carries inwards impressions received in outlying organs, and transmits them first to the centre, and where they may be perceived and give rise to sensation, or from whence they may be at once forwarded, without being *read off* like a telegraph message, to some other outlying organ. The two organs in this instance which are thus united are the lungs and the liver. Before the liver will act it must receive a message from the lungs; but if the pneumogastric nerves are divided in the neck then the lungs are cut off from the centre, their call for aid is not taken up to the brain, and therefore cannot be sent down to the liver. In order to test the correctness of this explanation, Bernard performed a crucial experiment. He divided the branch of the pneumogastric in the rabbit, which comes off from the main trunk at a point below the origin of the pulmonary branches. The place will be seen at once in the diagram (p. 94); it is marked *r*. By doing this he left intact the circle from the lungs to the liver by way of the spinal cord, but he cut off the direct influence of the brain from the liver. The result was that the formation of sugar went on at the liver. Thus it is an ascertained fact that the stimulus given to the liver by which this special function is called into play begins at the lungs.

Before this experiment, Dr. Bernard had instituted a series of most interesting ones upon the effect of irri-

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tating by a sharp-pointed instrument the portion of the spinal cord whence the pneumogastric nerves arise. The exact spot where the wound was made is limited by a line drawn transversely above from one of the tubercles of Wenzel to the other, and below from the two sets of the pneumogastric nerves. After the operation "the rabbit looks a little astonished at first," (*semble être un peu étonné sur le moment*), but it soon recovers its senses, and it does not seem to be the worse. The effect of this irritation or wound of this portion of the spinal cord is, that in the course of an hour or so the urine is diabetic.

We are now prepared for this effect; but when it was first announced it had a most startling effect. The explanation of the effects of the operation is simply that an extraordinary excitement was produced at the centre, simulating the ordinary stimulus which goes from the lungs to the liver through this centre, and that in consequence the liver discharged a greater quantity of sugar into the hepatic veins and vena cavæ than the lungs could use up, and the excess was passed on by the arteries of the body; from one of these arteries (the renal) the kidney separated it, along with the water in which it was held in solution, and hence the diabetic urine. That such is the true explanation of the phenomenon of artificial diabetes is certain; and it is highly probable that when this disease is met with it depends upon some diseased state of that part of the brain from which the pneumogastric nerves take their origin, and it is therefore not likely to be cured by feeding patients on bread made of gluten, or by means which tend to affect the symptom, not the cause.

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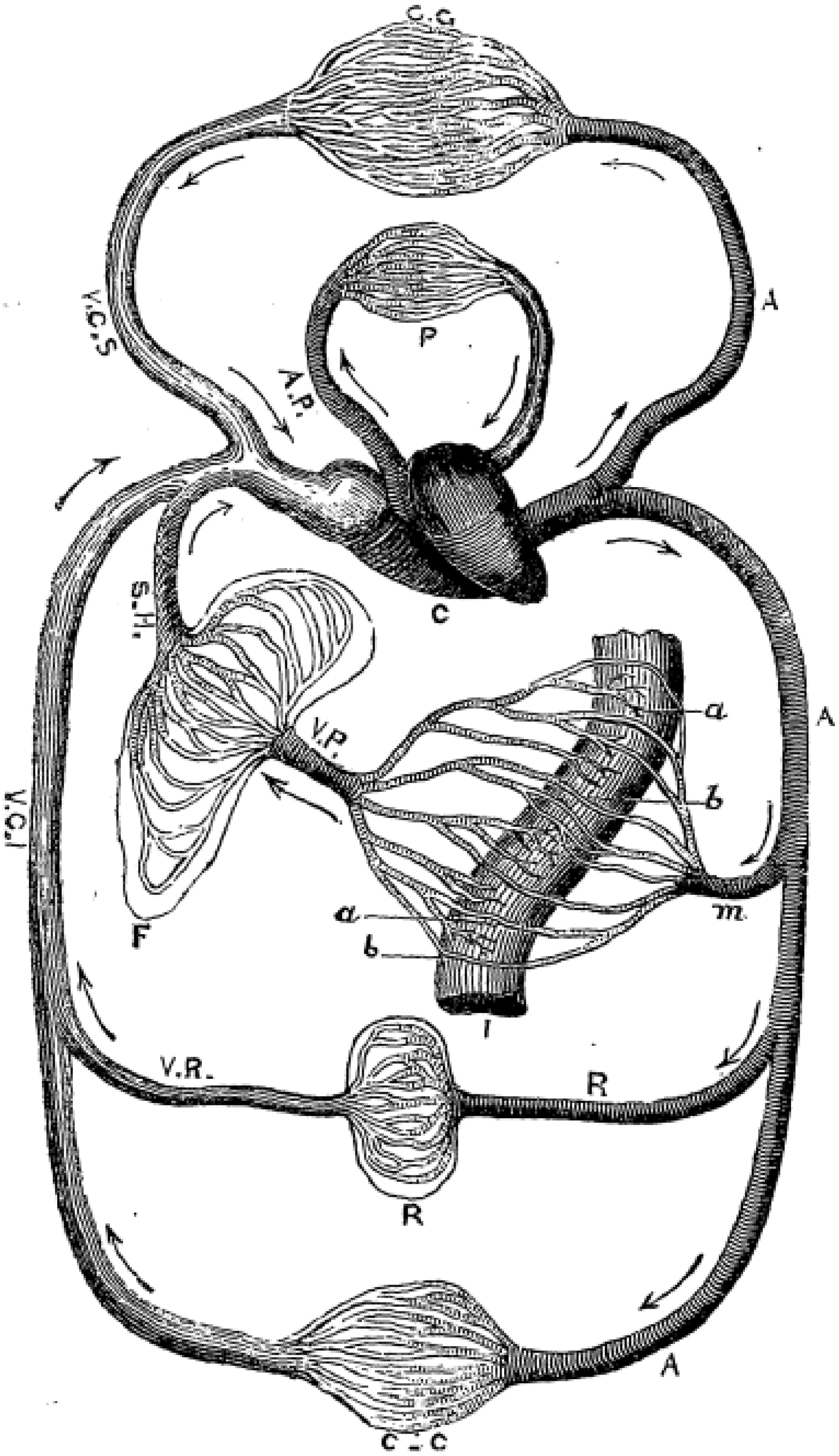
Fig. 5.



THIS DIAGRAM ILLUSTRATES THE WHOLE PROCESS OF DIABETES.

The source of the saccharine matter is the liver (F.) where it is produced, and in the substance of which it is always to be found. The liver receives the materials to make sugar from the blood which is brought by the

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vena portæ (V. P.), and which contains no sugar in carnivorous animals. This blood consists in part of the blood of the mesenteric arteries, which passes by the capillaries into the branches of the vena portæ at *b*, and some of this blood is made up of the materials absorbed directly at *a* from the intestine at I. The unsugared blood of the vena portæ (V. P.) enters into the liver, diffuses itself there, is subjected to metamorphoses in contact with the elements of the gland, becomes full of sugar, and then passes into the hepatic vein (H. S.). We have thus the blood entering the liver at V. P. (the portal vein) without containing a particle of sugar, and full of it at H. S., where it leaves the liver. The sugar then must have been generated at the liver. The sugared blood of the hepatic vein (H. S.) then arrives at the vena cava inferior (V. C. I.) near the heart, mingles with the non-sugared blood of the vena cava, and enters with it into the heart (C.), where it again mixes with the blood of the vena cava superior (V. C. S.); so that the quantity of sugar is diluted from being in the proportion of 0·980 to 100 at H. S. to 0·300 to 100 at the heart (C.). The sugared blood of the right ventricle (C.) is driven through the lungs by the pulmonary artery (A. P.), it arrives at the capillaries of the lungs (P.), where the sugar is almost entirely destroyed, the blood returns to the left side of the heart by the pulmonary vein. Thence the blood, having thus lost all its sugar, passes by the arterial or aortic system (A. A.) on to the general capillaries of the body (C. C.). There the blood undergoes further changes, and becomes converted into venous blood, to return by the vena cava superior and inferior (V. C. S., V. C. I.). The blood of the mesenteric artery (*m*) expands in capillaries upon the intestines, and absorbs from the intestine the matters there dissolved at I, and then goes on to the liver. The blood of the renal artery (R.) arrives at the kidney (R.), and yields the products which form urine, but has no sugar to give in its normal condition. This only happens when the quantity of sugar contained in the right side of the heart is more than 0·300 to 100, for this is all that the lungs can destroy. When more than this the excess passes into the arterial circulation, and the renal arteries carrying sugared blood deliver the same to the kidneys. The kidneys then become eliminators of sugar, and the individual diabetic.

It is now an established fact that there is a constant formation of sugar at the liver, and an equivalent destruction of that sugar at the lungs, in health, and that this subterranean operation is only manifest either when the liver yields too much or the lungs consume too little. The questions that remain to be considered are, what

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becomes of the sugar, and what purpose does it serve in the economy. The first supposition was, that, as sugar was a very combustible material, it was united with oxygen at the lungs, and was emitted thence in the form of carbonic acid, and some experiments seemed to corroborate this hypothesis. It was found that animals became diabetic on the administration of ether and chloroform, which was ascribed to the inability of the air loaded with these vapours to consume the sugar, which accordingly passed into the general circulation. However, Bernard demonstrated the fallacy of this theory, which at first he felt inclined to adopt, by exposing blood which contained sugar to the action of various gases. He divided a quantity of blood taken from the jugular vein of a dog in a state of digestion, and consequently sugared, into three equal portions; one he mixed with oxygen, one with nitrogen, and one with carbonic acid gas. The sugar disappeared in the portion mixed with nitrogen, but was unaffected by the oxygen and carbonic acid. We cannot detail his other experiments, but they were quite enough to satisfy him that the disappearance of the sugar was not owing to a conversion into carbonic acid at the lungs. Neither was it owing to the action of the alkalis of the blood. It is true that the blood always is alkaline, and that an acid, or even neutral, condition of the blood is incompatible with life; but it was chemically demonstrated by Lehmann that the amount of alkali in the blood was not enough to destroy the sugar. The explanation proposed and accepted by M. Bernard is, that the disappearance of the sugar at the lungs is owing to its transformation into lactic acid by a species of fer-

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mentation. The chemical constitution of the two is isomeric. Sugar being represented by C_{12},H_{12},O_{12}, and lactic acid by $C_{12},H_{10},O_{10},+{}^{2}HO$, or an atom of water; and this precise transformation may be imitated in the laboratory by mixing sugar and yeast, prepared in a particular way, when instead of converting the sugar into alcohol and carbonic acid, it changes it into tartaric acid and water.

Up to this point we have advanced step by step upon a series of facts, or what we must accept as facts until they have been disproved by counter-experiments and observations; but we now enter the region of conjecture, and although we may admit that M. Bernard's suggestion of the purpose the sugar he unquestionably discovered to be formed at the liver serves in the animal economy is extremely ingenious, perhaps perfectly true, yet it would be unjust to him as well as to science to place it on the same level as his great discoveries. The explanation he offers is this. If we take yeast and pass it through a filter, the filtered liquid will contain albuminous matter; now if we divide this fluid into two parts, and to one add sugar, and to the other nothing, in the course of a certain time the sweetened liquid gets turbid, and minute egg-shaped bodies appear in it, which gradually enlarge and give off similar bodies like themselves, until at length a sort of chaplet is formed, more or less simple, or branched, as may be. No such change takes place in the other portion. We have here the most primitive development of the vegetable kingdom in the form of this fungus, which appears when sugar and dissolved yeast granules are left in contact at a sufficient temperature. If instead of

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yeast we take a little serum of the blood, and divide it into two portions, exposing both to a sufficient temperature, but to one only adding sugar, we shall find that, in the course of four or five days, the unsugared portion has putrefied, but the sugared has had formed in it a quantity of cellules, not like the little cells that appeared in the yeast, but having a close resemblance to the white blood-globules. Here then is a sort of animal germination in the presence of sugar. If we follow up this hint, we find that throughout the vegetable kingdom the phenomenon of germination is invariably attended by sugar; and if we examine the animals in the fœtal state, we find that in all the muscular tissues and in the lungs there is a large quantity of lactic acid when soaked in water; but when treated with alcohol, instead of lactic acid we meet with glucose. The object that the sugar always present is supposed to serve is to present to the tissues during their formation the materials of their growth in a sort of nascent condition. Sugar, as we know, is a very unstable compound; it is very readily decomposed; it is in a state of constant fermentation, as the word is popularly used; and if a substance is brought in contact with it, de- and recomposition are quickened. Thus it promotes life by a sort of catalytic action, imparting to what it approaches the molecular restlessness of its own nature. In this way it is supposed to promote the formation of blood-globules in the liver, and serves as the nursery of the body.

While the genius of Bernard has thus disclosed the wonderful action of the liver in forming sugar, the labours of Professors Bidder and Schmidt have been directed to

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ascertain the history of the complementary function of that organ in the elaboration of bile. The sugar and the bile have this in common, that while the whole of the former is disposed of in the vital processes without appearing at the surface, except in morbid conditions of the system, the latter too is partially used up in the same way, while the remainder is cast out as refuse. In order to determine the exact quantity of bile secreted, and what became of it after it was poured into its proper channels, it was necessary first to institute a series of experiments to collect the bile as it was made in the liver, and this was done by introducing a tube into the bile-duct of various animals, measuring and analysing it, and comparing the conditions of the animals when deprived of their bile with their natural state. Bidder and Schmidt found that when the tube which gave them the command of the bile was successfully introduced, it did not materially affect the health of the subject of the experiment; and although the experiments in some respects have been considered fallacious, yet enough of undisputed conclusions remain to make the results highly valuable.

The bile is a clear fluid, varying in colour from golden yellow to grass-green. These variations depend upon the nature of the food and the action of the substances it is in contact with. The yellow bile is the result of animal and the green of vegetable food, but the clearest yellow becomes green very soon when exposed to the action of the atmosphere, and when retained for many hours in the gall-bladder, but the bile of the gall-bladder taken two and a half to three hours after a meal is as yellow as that taken directly from the liver. We may thus ascertain whether ejected bile has been long or recently secreted

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by the colour it presents, if we know what the person has been eating. When first secreted the bile is always neutral, and it derives any alkaline character it may afterwards acquire from the mixture of mucus from the gall-bladder.

As to the quantity of bile secreted in a given time, for example, in twenty-four hours, this is only approximately ascertained by comparing the amount of different periods after food is taken. The following table shows the fluctuation it underwent in a cat.

One kilogramme of cat afforded in one hour:

Hours after the last meal	$2\frac{1}{2}$–3	12–16	24	48	72	168	240
Of fresh bile (grammes)	0·600	0·807	0·410	0·291	0·179	0·153	0·094
Of dry residue . . .	0·033	0·045	0·025	0·020	0·018	0·011	0·007

From this table, which is only one of many that give the same result, it will be seen that immediately upon taking food there is an increase in the flow of bile, before digestion can have taken place to such a degree as to have at all enriched the blood with chyle, and, consequently, simply from the greater quantity of blood which the stimulus of food retains in the organs where it is first received; that this increase gradually augments and reaches its maximum from twelve to fifteen hours after a full meal, by which time, probably, all the products of the digestion have been received into the blood. At this point a diminution begins in the amount of bile secreted, so that in twenty-four hours after the meal the quantity is far less than two hours after. From these data we may strike an average of the daily secretion of bile in the cat with tolerable exactness; and this gives 0·608 grammes of fluid bile, and 0·034 grammes of dry to a

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kilogramme of cat. A dog yields one fiftieth of its weight in the day, and a rabbit no less than one eighth of its weight. It is most probable that a man yields about the same proportion as a dog: this would give about 60 ounces to a man of ten stone. We may observe in passing that there is a partial but not complete coincidence in the increase of sugar and bile delivered by the liver. The sugar at once attains its highest amount after a meal is taken, but although even then the quantity of bile secreted is much above its average, yet it does not attain its maximum till long after the sugar has begun to diminish. Let us now consider what becomes of all this bile that is poured into the intestines. Does it pass off by the alvine channel or by the kidneys? An analysis of the solid constituents of bile disproves the latter supposition: they are not found in the urine. But they are in the fæces, but not nearly the whole quantity. In order to ascertain this, the quantity of solid matter yielded daily by a dog fed on animal food was accurately determined, and then the fæces were subjected to a careful analysis. The result is shown in the following table:

Water	56·4 grammes.	
Solid residue	40·9	,,
Of these neutral fat soluble in ether . .	2·82	,,
,, Fat with traces of bile	4·97	,,
,, Bile products soluble in alcohol . .	3·80	,,
In this organic matter . .	3·42 with 0·70 sulph.	
,, inorganic ,, . .	0·38 grammes.	

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Extracted by muriatic acid and boiling alcohol:

Not derived from the bile .	9·60 grm.	with 0·084 sulph.
Fat, oils, acids, and ox. of iron . . .	6·35 „	
Hair, sand, &c. . .	13·36 „	
	40·90	

From this it is proved with absolute certainty that the larger part of the bile discharged into the intestines is there absorbed into the blood, for only a small portion of the 40 grammes of solid residue of the fæces was derived from the bile, while the quantity of solid matter delivered in the same period was nearly equal to the whole of the fæcal residue, being 39 grammes, while the other is only 40. The fact is brought out by attending to the Sulphur. In the 39 grammes of solid bile discharged by the liver in the five days into the intestines, there must have been 2·37 grammes of Sulphur: now the whole quantity of Sulphur contained in the fæces was only 0·384 grammes, and more than half of this was derived from the hair, &c., so that almost all the Sulphur which is contained in the bile is absorbed by the blood again, and about seven eighths of the whole of the solid contents of the bile. As the bile then does not obtain an exit by the alvine or urinary channel, there remain but the lungs to be examined. Bile consists chiefly of a feeble acid called bilic, or cholaic combined with soda. The composition of this acid is thus atomically represented: $C_{44}, H_{26}, NO_{13}, = C_{44}, H_{25}, NO_{12}, HO$. (Gregory). We

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see from this what a large proportion of carbon there is in bile, and we know that carbon is being constantly emitted from the lungs in the form of carbonic acid; hence the conclusion is irresistible, that the expired air must carry off a portion of the secreted bile. Some chemists have gone so far as to declare that all the carbon discharged from the system passes through the liver, but this undoubtedly is not the case; for a dog fed on animal food yields at the rate of 8·6 grammes of carbon for every kilogramme of its weight in the air it expires, while during the same period the liver does not discharge above 1 gramme of bile into the intestines, and out of this not above 0·5 grammes of carbon are absorbed by the blood. So that about six per cent. only of the carbon which is given off by the lungs as carbonic acid comes from the liver.

Let us now consider the uses of the bile in the animal economy; a subject only imperfectly known as yet, notwithstanding all the labour and skill which have been brought to bear upon it. From the following description of the effects of depriving a dog of its bile we may gather much information; it is given by Bidder and Schmidt, and is a fair specimen of what was generally met with by them in the whole series of their experiments. After an opening had been made in the side of the dog, and a tube inserted intothe hepatic duct, which received every drop of bile furnished by the liver, and carried it outside, where it was of course of no use to the dog; for the first two days the animal refused its food and seemed much indisposed; but from the third day it ate greedily of flesh to the amount of 160 and even 403 grammes a day. The weight of the dog at

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the beginning of the experiment was 6 kilogrammes, and at the end of a month it weighed only 3·10 kilogrammes. In thirty-four days it had lost two fifths of its weight, notwithstanding the large quantity of flesh it had consumed. The emaciation proceeded gradually from the first, and consisted chiefly in the disappearance of all the fat, so that the muscles of the body looked like long ribbons seen under the skin, which hung in loose folds over the skeleton. The muscles too wasted away, and there was great feebleness of movement from the first. The hair came out in handfuls and left bare patches. During the whole of this miserable spectacle the digestion seemed to go on with perfect regularity. The alvine evacuations were infrequent, at first slimy and white with gray and green portions, but latterly of the normal colour and consistence; but during the whole period they had a strong assafœtida smell, owing to the putrefaction of the food in the intestines. Although the breath was also most fetid and disagreeable during the whole of the experiment, there seemed to be no tendency to dissolution of the fluids, as there was neither ecchymosis nor hæmorrhage. The urine was small in quantity, but of high specific gravity. Notwithstanding the extreme emaciation, the dog retained its animation to the last, and although it could hardly stand, it rose when we (*i.e.* Bidder and Schmidt) entered the laboratory of a morning and followed us to the balance. There was no disturbance either of the respiration or of the heart's action, but a most marked decrease of animal heat: a thermometer inserted into the rectum indicated 34·6 C. degrees, and fell even so low as 29 C. The death of the animal was without a struggle. On dissection, the body

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disclosed no abnormal appearance of importance, except the almost absolute absence of fat and thinness of the muscles. The naked eye could not detect an atom of fat in the cellular tissue under the skin, and it had entirely disappeared from the kidneys. It would seem from this that this dog, although largely fed with its appropriate food, was simply starved to death, because, although it could digest, it could not assimilate its meat when deprived of its bile. Besides being starved, it was made cold by cutting off some of the charcoal which should have been burned at the lungs. Let us now endeavour to ascertain how it was starved by its bile being cut off, in other words, to the assimilation of what portion of the aliment bile is essential. Taking the usual division of food into the albuminous, the feculent, and the oily, Bidder and Schmidt made the following experiment to determine the action of the bile on each species. They gave a dog, which they had absolutely deprived of bile, 3·035 kilogrammes of liver and lungs, from the 29th of March to the 2d of April. This quantity of flesh contained 806·8 grammes of solids, of which 693·2 grammes were albuminous and salts, 113·6 grammes were fat. The alvine evacuations of this dog during that period yielded 124 grammes of solids, of which 72·2 grammes were fat, and only 51·8 grammes were albuminous matter and salts. The difference between 693·2 and 51·8 represents the quantity of albumen digested by the dog in these five days without the aid of bile. From this and similar experiments we may conclude that it is not in virtue of its action on the albumen of the food that bile is required for its assimilation.

Another dog was fed for eight days upon black bread.

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The alvine evacuation was very voluminous and had an acid reaction; on being tested with iodine to determine whether starch was present, only a few blue spots appeared here and there, and not more than was observed in similar circumstances in a dog in its normal condition. The dog increased in weight, and the same observation was made in an experiment with another dog fed upon white bread after its bile had been withdrawn. In this, for instance, in eight days the dog increased in weight from 5·325 to 5·590 kilogrammes. The inference drawn from this seems undeniable, that feculent food can be perfectly digested without the assistance of bile, and that it therefore takes no part in the assimilation of this portion of the aliment.

The influence of the bile upon the fat alone remains for consideration, and that fat too can be assimilated without it, is proved by the experiment first detailed. This dog, whose weight was 5300 grammes, received in the first series of observations 95 grammes of fat in eight days, and in the second, 41·4 grammes in five days, while there were found 85 grammes of fat in the alvine evacuations in the first experiment, and 72 grammes in the second. So that in one case the dog assimilated 11·88 grammes of fat daily, and in the latter 2·24 grammes. Similar results were obtained by Lenz, who made many experiments on this subject, which are published in his thesis.[1] Although there can be no doubt from these experiments that *some* fat may be absorbed into the system without the action of the bile, yet it would seem that the quantity is far less than when this secretion is present. For a dog not much larger than

[1] De adipis concoctione et absorptione diss. inaug. Dorpati, 1850.

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the one previously employed, absorbed no less than 446·9 grammes of fat in five days, which is about seven times as much as was possible for the dog to do when deprived of its bile. That bile enables fat to be received into the animal economy seems to be an established fact; but the method of its operation in this process is still a matter of uncertainty. It evidently influences the absorption of fat by the lacteals, for while the average of fat in the chyle is 32 per mille, it was only 2 per mille in animals deprived of their bile. If the fat were acid the soda of the bile might neutralize it, and it does when this is the case: but the fat for the most part is neutral, so it is not in this way it acts. Nor does it act by making a sort of emulsion with oil globules as has been supposed, for such an emulsion would not be more easily absorbed than fat in a state of minute division. Its true mode of action, according to Bidder and Schmidt, is by moistening the epithelial cells of the mucous membrane of the intestine, and so disposing them to permit the permeation of the oil-globules: without this the fat is not absorbed. Its action is thus purely mechanical, and affects the molecular attraction of the interstices through which the oil passes, and may be imitated by moistening a capillary tube with bile, and dipping one end in oil, while a dry tube of the same diameter is similarly placed; the oil rises higher in the tube moistened with bile than in the other. Thus it would seem that an animal actually dies of starvation, because it cannot absorb fat without the aid of the bile; and one of the chief offices of the bile is to lubricate the passages by which the fat passes into the blood!

The inordinate appetite observed in dogs deprived of

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the use of their bile, arises from an instinctive perception of the large quantity of food that is required to compensate the want of fat which should have been absorbed. Hunger is one of the constant effects of a deprivation of bile. The exact difference in the quantity of albuminous diet required to sustain a dog in its full weight without the aid of bile, is thus computed: a dog of 4 kilogrammes in weight consumed daily 250 grammes of flesh; and now, by direct experiment, it was ascertained how much carbonic acid this dog expired, and the quantity represented 34·36 grammes of carbon in the day. If the flesh he ate had been pure albumen, it would not have been enough to meet this consumption at the lungs, for it would only have given 27 grammes of carbon, but the deficiency was made up by 19 grammes of fat, which gave 15 grammes of carbon. This, with the carbon contained in the salts of the meat, was sufficient for the demand at the lungs. The dog got 39 grammes of carbon in all, and only required about 34.

But how was it with the dog that had its bile turned off? It was found to consume 47 grammes of carbon at the lungs in a day; but it lost 3·5 grammes of carbon by the escape of the bile, which had also to be made up, so that its demand was 50 grammes of carbon a day. Now all the fat he could get into his system, owing to the want of bile, was 10 grammes a day, yielding 7·8 grammes of carbon; so that there was a deficit of 42 grammes of carbon to be made up by a corresponding increase in the quantity of albumen, to no less an amount than 400 grammes of raw meat to cover this enormous loss of carbon. This dog actually consumed 525 grammes of flesh, and required it all to keep him-

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self in condition, while his fellow who had the use of his own bile, required only 250 grammes: hence the hunger.

It would, however, be an error to suppose that the only function of the bile was the assistance it gave the system in the absorption of fat; undoubtedly, after its absorption, it discharges most important offices, in aiding those wonderful transmutations by which the external materials, out of which the body is made, are converted into flesh and blood. But into this mysterious region chemistry cannot follow; it notes its entrance and awaits its exit; but what transpires in the secret chambers of life, where conception is constantly going forward, is beyond its utmost efforts to penetrate. Again we see the fallacy of hoping to find in chemistry, a key to the cure of the morbid alterations in the vital actions, from which even in their normal state the chemist is entirely excluded. The chemist tells us that in the bile absorbed, there is always a minute quantity of sulphur, and that this sulphur plays an important part in the transformation of the tissues; but this is all, and the physiological chemists most candidly confess that there are probably an infinite number of uses of the bile within the economy, of which they know absolutely nothing. We have seen how important was the action of the nervous system upon the glycogenic function of liver, and we cannot doubt it is equally potent in its influence upon the quantity, and probably also the quality of the biliary secretion, as shown by the sudden yellowness of the skin from anger or jealousy: but, of all this, beyond the naked facts, we know absolutely nothing.

Even of the much more accessible department of know-

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ledge—the influence of different kinds of aliment upon the quantity of the bile secreted—we know extremely little, and that little provokingly proves that this influence is very great. In the series of experiments made by Bidder and Schmidt to ascertain the effects of discharging the bile directly from the liver, and depriving the animal of it, the effects upon the quantity secreted within a given time by different aliments, are noted by the way, and afford some curious and interesting, although imperfect information. The most certain fact ascertained was, that abstinence rapidly reduced the quantity of bile secreted, while flesh and water, on the contrary, as certainly increased it. That eating flesh should do so is not surprising, but that pure water should augment not only the quantity of fluid bile, but of its solid constituents, was an unexpected result. The following table exhibits the fact.

Day of experiment.	While fasting the Dog yields.			Fed.		Now yields.		
	In ¼th of an hour.	Fresh bile.	Solid residue.	Beef.	Water.	In ¼th of an hour.	Fresh bile.	Solid residue.
		Grms.	Grms.	Grms.	Grms.		Grms.	Grms.
Nov. 7	4	1·558	0·040	—	185	4	5·165	0·143
„ 5	2	1·009	0·028	—	60	2	2·217	0·062
„ 3	4	2·689	0·085	25	158	4	4·030	0·177
„ 2	4	2·109	0·103	140	140	4	4·105	0·138
„ 15	2	0·397	0·046	160	—	2	2·073	0·153
						2	2·814	0·129
„ 17	4	2·001	0·217	185	—	4	2·883	0·135
„ 14	2	0·74	0.061	200	—	2	1·486	0·100
						2	1·442	0·058

It will be seen that in the first experiment, the drinking of 185 grammes of water increased the quantity of the bile delivered in an hour from 1·558, to 5·165,

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or more than four-fold. Here is an important fact for the water-cure! The effect of lard and butter was rather to diminish the quantity of bile as compared to the effects of a meal of flesh; but it would be rash to draw too many inferences from tables recording experiments made for a different purpose. The difficulty of the subject is very great, but as the zeal of experimental physiologists seems setting at present towards the liver, we may hope for much important addition to the present stock of our knowledge in regard to the action both of alimentary and medicinal agents upon this important organ; and we cannot but look forward with hopeful anticipation to a rapid increase in our practical ability to cope with the multitude of affections which depend more or less upon some disorder of the hepatic structures, when we are made more fully acquainted with the value of each of its functions, the methods of gauging their efficiency, and recognising their aberrations, so as to bring them under better control, by more enlightened management and medication.

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CHAPTER IV.

ON DIGESTION.

THESE are bad times for the young and earnest physician of the old school of medicine. Let him round the circle of the sciences; let him master the stethoscope and the microscope; let him perfect himself in all chemical knowledge; let him understand the most occult anatomy of the body of man and of all living beings; let him advance into physiology as far as it is possible for him to penetrate, and if he then attempt to apply all these vast acquirements to the simplest problem his patient gives him to solve, by asking him to explain how he digests a mutton chop and a potatoe,—or why he can't do it,—which is the more likely form of the interrogation, our young philosopher will be compelled to make the humiliating confession that from the moment these representative constituents of the material of the flesh and blood enter the mouth till the period of the accomplishment of their mission, their analysis, appropriation, and rejection, every stage of their progress presents subjects for controversy so keen that, but for the improvement in the manners of this age, we should have discoverers brandishing their knives at one another, and only too thankful that they escaped the fate of George Wirsung, who was assassinated for his

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daring innovations about the pancreas. "Wirsung mourut assassiné, dit on, le 22 Août, 1643. Nous constatons de nouveau ici que les découvertes anatomiques et physiologiques suscitent aujourdhui moins de passions." So says Bernard in Paris, and we fancy we hear Herrn Bidder and Schmidt mutter, "Gott sei dank" from Dorpat. For there is scarcely a single proposition made by the brilliant Frenchman that is not met either with a point blank contradiction by the patient painstaking Germans, or at least subjected to very rude handling. And the subjects of these controversies are not minute or trivial, but no less that the effect of almost every different agent on the fecula, the albumen, and the fat from the entrance of these substances into the mouth till their disappearance in the intestines. They differ about the action of the saliva upon the fecula, about the action of the gastric juice upon the albumen, about the action of the pancreatic fluid and bile upon the oil and upon the albumen. We shall not be guilty of the presumption of attempting to decide which is in the right, but shall satisfy ourselves with stating the most important conclusions that each party has arrived at, leaving to future investigators the ultimate decision. And we do not think that it is a waste of time to discuss these matters, even although we cannot arrive at positive and final opinions upon them, for among the various knowledges we do well to acquire there is none more important than the knowledge of our ignorance.

Without farther prologue we shall at once proceed to detail the observations of Bernard, and Bidder and Schmidt, upon the action of the different agents to whose

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operation every kind of food is exposed in the process of digestion.

The first of the long series of changes which food undergoes to prepare it for incorporation takes place in the mouth where it is subjected to the process of mastication and insalivation: the former being a purely mechanical division of its parts so as to form a mass which can be easily swallowed, the latter being partly mechanical in so far as the lubrication of the mass is concerned and its admixture with the air, and partly probably chemical from the action of the saliva upon the fecula it may contain. This saliva, which is generally spoken of as a simple secretion is, in fact, a combination of three secretions derived from three different glands, and each of the three possesses peculiar qualities. The first we shall consider is the secretion from the parotid glands. This consists of about 95 per cent. of water, and its action according to Bernard, is to facilitate mastication. He found that after its obstruction in the horse the animal took much longer to eat its hay, oats, and straw, and that the quantity of the secretion was in proportion to the dryness and hardness of the food. Thus while eating 250 grammes of bread in 3 minutes, 185 grammes of parotid fluid flowed from the ducts, which were exposed so as to be subjected to experiment, while no less than 932 grammes were secreted in 12 minutes when the horse was masticating hay. The absence of this secretion increases the thirst in a marked manner, so that an animal which before the obstruction was satisfied with about 20 kilogrammes of water in the day, required after the operation about 30 kilogrammes to appease its thirst. And this thirst does not

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arise from dryness of the fauces, for the mere swallowing of water has no effect in abating it, if that water does not reach the stomach and intestines, as happens when an artificial fistula is made in the œsophagus. Thirst is a general not a local appetite, and can only be satisfied by increasing the fluids generally.

The parotid fluid is a slightly alkaline clear watery secretion, containing a peculiar substance called ptyaline along with a trace of albumen; it is under the influence of the facial and the fifth pair of nerves, and when these are excited a corresponding increase of action in the gland is observable. It is essentially an appendix to the function of mastication, and this is probably its only use. There is indeed some discrepancy about its chemical action on starch. Bidder and Schmidt distinctly affirm that it has no action at all upon starch, even after being some hours in contact with it, while, on the other hand, Jacobowitsch maintains the contrary. However, we may safely conclude that at all events the chemical action of the parotid fluid is quite insignificant, and that Bernard is justified in representing it as subserving mastication and that alone.

The second source of saliva is the submaxillary glands. The secretion from these is limpid like the parotid fluid, but much more viscid, and becomes gelatinous on cooling; it is also decidedly alkaline, even more so than the secretion of the parotid. Its inorganic constituents are chiefly chlorides, carbonates, and phosphates of lime and soda. According to Bernard the use of this secretion is to assist the functions of taste. The quantity that is poured out by these glands is determined by the greater or lesser degree of sapidity of the food.

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Thus he found that when sugar and water was applied to the fauces of a dog, there was hardly any action excited in the submaxilliary glands, but when the gullet was moistened with diluted vinegar or extract of colocynth there was an immediate and copious secretion. In accordance with this view he also found that while the nerves which supply the muscles of mastication on being irritated increased the discharge from the gland of mastication, viz., the parotid, in like manner when the gustatory nerve was excited by galvanism there was an immediate action observable in the submaxillary gland. Their sympathies are with the stomach, for when the secretion of gastric juice takes place it induces an action in the submaxillary glands and not in the others. The submaxillary, (like the parotid fluid,) seems to have a very trifling if any chemical action on the food, for Bidder and Schmidt found that starch remained unchanged when left for some hours in a mixture of the saliva derived from the parotid and submaxillary glands.

The third and only remaining gland is the sublingual. The existence of this in the dog and some other animals has been denied by many anatomists, but Bernard contends that this has arisen from an oversight on their part, and that the sublingual glands are always present, and the secretion from them possesses properties wholly different from those of the other two sources of the saliva. The difference consists in the much greater viscidity of the secretion, and its richness in ptyaline, and the traces of sulpho-cyanide of Potassium, which are always to be met with in it. It is, in fact, a sort of mucus, and approaches the true mucus of the buccal

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cavity in all its essential characters, and as we shall afterwards see, this mucus performs a most important part in digestion.

The distinctive qualities of the three sets of glands are thus, according to Bernard, that the parotid secretion is almost entirely water, and assists deglutition; the submaxillary more viscid and gelatinous on cooling, and is associated with the property of taste; while the sublingual is also very viscid, and does not jelly on cooling, contains much ptyaline, and cooperates with the mucus in its chemical action upon fecula.

Bernard relates some curious experiments exhibiting the elective power of the salivary glands to pass or to refuse various substances carried to them by the blood. He injected into the jugular vein of a dog, 25 grammes of warm water containing 5 grammes of yellow prussiate of potash, 5 grammes of iodide of potassium, and 4 grammes of grape sugar. As soon as the operation was over, he provoked the salivary glands, and on collecting their discharge he found iodine, but not a trace of the prussiate of potash or sugar. After seven minutes, the observation was repeated with the same result. The urine was then examined, and, there, prussiate of potash was found, but neither sugar nor iodine. In twenty-five minutes more they were both examined, and now the urine contained prussiate of potash and a trace of sugar, but no iodine, while the saliva was full of iodine, but without sugar, and prussiate of potash. In three hours the iodine was recognisable in the urine. Thus, the prussiate of potash appeared in seven minutes, the grape sugar in twenty-five to forty, and the prussiate of potash not for three hours. While in the saliva iodine was

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found in four hours in its former abundance, but still not a trace of the other two substances which had been injected along with it, and which had both been passed by the kidneys. This experiment, often repeated, never failed to give the same result; and Bernard considers it an established fact, that iodine is at once and constantly discharged by the salivary glands when present in the blood; indeed, its power of passing is so great, that its presence affords an entrée to substances, of which iron is one, which without it would be refused admittance into the salivary vessels. This he proved by various ingenious experiments which we have not space to detail, but the result was that while, in general, salts of iron are not to be found in the saliva, although they have been injected into the blood or stomach, yet iodide of iron forms an exception. But a still more remarkable fact, and one of special interest to us, is, that when a solution of two grammes of iodide of potassium were injected into the stomach by an artificial fistula, long after all trace of it had disappeared from the urine, it was still found in the saliva and in the gastric secretions even after an interval of three weeks. This observation has an important bearing upon the duration of the action of medicinal agents, for its persistence is manifestly due to vital, not chemical laws, and we may now understand the reason of our inability to affect our patients beneficially with our medicines, if they have in their system a poisonous spring, the existence of which we are ignorant. And even if we did know of it, how could we either stop or neutralize this stream of mischief, deranging the healthy actions, and paralysing our remedies? The wonder is, that in the old poisoned patients who come to

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us, we can ever do any good at all, and not that we cannot produce, as they expect, immediate and constant improvement!

To return from this digression to the action of saliva; it is a well-established fact that the mixed saliva found in the mouth has properties peculiar to itself, and different from any of its constituents. The most probable theory is, that when the secretion from the salivary glands meets the mucus in the mouth, there is then a peculiar action of the nature of fermentation engendered, and that the result of this is to produce a combination which has a very rapid and intense action upon all feculent articles of food. MM. Bidder and Schmidt give a table of the time required to convert starch into sugar in the presence of different secretions, and the chief facts brought out by it are, that this change was *instantaneously* begun, when saliva from a man, a child of three months old, or a dog was brought in contact with it, while it required twenty minutes to make its appearance when the mucus of the mouth without the secretion of the parotid gland was employed, and one to three hours when the parotid and submaxillary glands were substituted. It seems, then, that neither the buccal mucus, nor the saliva by itself, can do what the two together at once effect. The mixture is required for the ferment.

Although it is an established fact that the saliva found in the mouth has a most rapid and energetic action upon the fecula of the food, which it converts, as far as its action goes, into sugar; yet we meet with the singular contradiction of there being no sugar discoverable in the stomach, even although the morsel swallowed be at once removed and examined. The explanations offered of this

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apparent anomaly are not satisfactory; on the one hand it is advanced that the exposure of the feculent materials to the action of the ferment is too short, and that the condition of the starch in the food is not favorable to its being acted on by the saliva; while on the other hand it is supposed that the sugar is either absorbed on its passage to the stomach, or if it reaches that cavity, is immediately converted into lactic acid by the gastric secretions. Such are the explanations; but it is admitted that as yet the difficulties are far from being satisfactorily removed. Thus, at the very outset of the investigation of the process of digestion we encounter an unsolved problem, and we cannot affirm with confidence what chemical changes are undergone by a morsel of bread in its passage from the mouth to the stomach.

The existence of sulpho-cyanide of potassium in the saliva is a curious fact that has given rise to much fanciful speculation. It is known that this salt is a powerful poison, and it is conjectured that the poisonous action of the saliva of rabid animals may arise from an increased quantity of this natural ingredient of that secretion. This notion rests on no evidence of any weight, and we may at once dismiss it as wholly insufficient to account for the phenomena of hydrophobia. At the same time it is certainly important to know that this very poisonous substance is liable to be generated in greater quantity by the salivary organs, when a morbid action is developed in them. Thus Lehmann[1] examined the saliva of a patient suffering from mercurial salivation, and found its properties very different from the normal secretion: it was thick, gelatinous, flocculent, and very

[1] 'Jahrbuch der Phy. Chem.,' B. II.

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poor in ptyaline; but, on the other hand, contained a large quantity of sulpho-cyanide of potassium. It is certainly not improbable, that some of the noxious effects of this dreadful drug—mercury—may be attributed to its exciting this poisonous secretion, some of which must be swallowed and thus complicate the evils the original poison has inaugurated.

Among the other morbid secretions of the mouth, may probably be reckoned the tartar of the teeth. This consists, fundamentally, of phosphates; but although these salts form the basis of the deposit, particles of carbonate of lime, epithelial cells, &c., are mechanically combined with it, and form a notable portion of the whole. The origin of this was supposed to be simply the evaporation of the saliva; but Bernard has advanced the supposition, that it is the result of a slight inflammatory action of the alveolar processes, a chronic periostitis, depending upon irritation of the organs of digestion, and partial denudation of the roots of the teeth. It is more abundant always in the lower than the upper jaw, because the gums are more pressed from the teeth of the former during mastication; and he found it take place rapidly in dogs, when their digestion was disturbed by a partial closure of the orifice of the gastric fistula, and when the exciting cause was removed, and the normal condition of the digestive functions restored, it as speedily disappeared. This observation has an important practical bearing, and if it is confirmed, the state of the teeth and gums may be found to have a constant relation with certain specific affections of the alimentary canal, and be thus both remediable in themselves, by appropriate medication, and a guide to the prac-

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titioner in the selection of the remedy proper for the cure of the original malady, on which this lesser one has supervened. Thus the lights of advancing science, which exhibit with increasing clearness, the baleful action of the mercury, the iodine, and the other poisoned arrows which fill the quiver of old physic, are so many safety lamps to us, improving our practice by giving us more exact indications of morbid actions, for the excitement of which, *we* are not accountable.

We have made but small progress as yet, all we have done is the division of the food into smaller parts, its mixture with a watery fluid containing much atmospheric air, and the conversion of a portion of its starch into sugar; in this condition it is projected into the stomach. Here it is exposed to the action of two substances, the one an acid, the other a peculiar principle called pepsine. These two, combined, constitute the gastric juice, and the presence of both is necessary to effect digestion Till of late, our knowledge of the properties of the gastric juice in its physiological relations, were chiefly derived from the experiments of Dr. Beaumont on his patient, who had a fistulous opening in the stomach; but, recently, various experimenters have found that such an opening being easily made in animals, without causing them much inconvenience, they had in their power to repeat and vary these original observations, and have arrived at many more exact conclusions than before were possible.

It seems ascertained by a great number of experiments, that this gastric juice varies much both in quantity and quality at different times, these variations depend upon the nature of the food received into the

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stomach; and it is probably owing to this, that there is so much discrepancy about its chemical composition among the highest authorities. As regards the quantity, Bidder and Schmidt found that a dog of 12 kilogrammes yielded

In ½ an hour	63 grammes,	3 hours after having been fed on meal and water.	
„ 1 hour	21·3	„	with a perfectly empty stomach.
„ ½ an hour	33·5	„	when bones were in the stomach.
„ 1 hour	62·2	„	in the same circumstances.
„ ½ an hour	12·0	„	with a perfectly empty stomach.
„ ½ an hour	38·0	„	3 hours after a meal of flesh.

So that in four hours this dog, of 12 kilogrammes in weight, yielded 23 grammes of gastric juice. If the human stomach is equally productive, an ordinary sized man would secrete 6·4 kilogrammes of gastric juice in the twenty-four hours. This estimate is considered very high, but it corresponds with the experiments of Lehmann as to the quantity of gastric juice required to dissolve albumen. However, as it appears that all the albumen received into the system is not dissolved in the stomach, the correspondence is of less consequence, and when we observe such an enormous difference in the yield of different hours, we may well suppose that there is an equal difference in different stomachs, and that before it is possible to strike an average, it will require an almost infinite multiplication of observations. It is of more interest to know that there is this difference, and that it is affected by various causes. For example, it was found that the mere sight of a piece of meat called forth a copious secretion of gastric fluid, after the salivary duct had been obstructed. So that the stomach

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as well as the mouth *waters* at the sight of proper food.

Again it was noticed that while this necessary secretion was thus excited by the prospect of eating, an opposite change took place if the animal had been exposed to a painful operation, then the gastric juice lost its digestive power entirely; this, however, was from a change in its quality not its amount. It lost its acidity without which it has no power to perform its office. This was probably owing to the large quantity of saliva which pain often produces in animals, a sort of weeping from the mouth, and this saliva being alkaline neutralized the acid in the stomach. Thus the operation of sorrow if it gives rise to copious tears may produce a chemical incapacity to digest food, as well as physiological depression of the digestive organs.

While it is an undisputed fact that free acid is present in the stomach, and that its presence is an essential condition of the action of the gastric juice upon the aliment, what that free acid is has occasioned much keen controversy. Some, as Prout, Tiedeman, and Gmelin, maintain that it is hydrochloric, while Lehmann and Heintz are of opinion that it is lactic. Sometimes there is free phosphoric acid present, or at least acidity is given by the presence of super-phosphates, but only after the animal has been fed on bones. The result of eighteen analyses made apparently with the greatest possible care has led Bidder and Schmidt to a conclusion which reconciles the conflicting views. They found that in carnivorous animals, after fasting for eighteen or twenty hours, pure hydrochloric acid *alone*, without a trace of lactic, was present in their stomachs, whereas the stomach

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of herbivorous animals contained a certain small and variable amount of lactic acid, which was probably derived from the food. We may assume, then, that the acidity of the gastric fluid in man, if normal, is due to the presence of free hydrochloric acid.

Of pepsine little is known chemically; it so far resembles albumen as to be coagulable by heat, and that it is indispensable for the action of the gastric fluid is a certain fact. Neither the acid nor the pepsine suffices alone, for if it be neutralized by carbonate of soda it loses its power, and regains it when the alkali is removed, and equally destructive is the application of heat, which coagulates and destroys the pepsine. The fact of an alkali destroying the effect of the gastric fluid, and instantly arresting its digestive power, explains the mischief bile does when it gets into the stomach. It was found that a very small quantity of bile when mixed with the gastric juice at once and entirely suspended its chemical action upon the substances which it was vigorously handling before the addition. So that after all the phrase in every patient's mouth about being bilious may sometimes have a sounder foundation than we are apt to imagine.

Such being the constitution of the secretions from the stomach embraced under the designation of the gastric juice, let us proceed to inquire what is the character of its action upon the three constituents of all digestible food, viz., fat, fecula, and animal or vegetable fibrine.

Its only action on the fat is to dissolve the walls of the cells which enclose it, and to liberate their contents; by this, fat is rendered fluid, but undergoes no other change either physical or chemical.

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The fecula contained in vegetable cellular tissue, as in a potato for example, is likewise enlarged from its confinement by the solution of the walls of the cells, and is brought into immediate contact with the gastric juice. The only effect of this upon the starch is to produce a hydrate either by its acidity or temperature. This is merely a physical change and not a chemical one, and its reaction with iodine is unaffected by it. Nor are the sugars affected materially, except that cane sugar if long exposed to its action is converted into grape sugar, and this in its turn into lactic acid. However, from the length of time required to effect these transmutations, it is probable that they seldom, if ever, occur in ordinary digestion.

Its main work is with the azotized matters. A piece of flesh, for instance, when exposed to the action of the gastric fluid, either in or out of the stomach, is softened and transformed into a grey paste to which the name of chyme has been given. On examining this with the microscope, it is found that the change consists in the separation only of the fibres by the solution of the cellular tissue which bound them together, and that the original structure of the muscle is quite distinguishable, so much so that it is found to be much the best way of procuring the primitive fibres to expose for a length of time a muscular tissue to the action of gastric juice. This separation of the muscular fibre is much more rapid if it have been previously cooked, as was proved by experiments upon dogs. So that, in fact, a great portion of the work of the stomach as regards flesh may be anticipated in the kitchen. Milk and albumen it first coagulates then dissolves; bones it dissolves and removes

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from them their azotized portion which is very considerable, leaving behind the insoluble earths to be expelled. In this the action of the gastric juice is the converse of acid fluids which dissolve the earths and leave the animal parts of the bone. In all these instances the action of the secretion is strictly physiological, but it has also the power of acting chemically in virtue of its acid nature upon various salts.

Considering the wonderful energy with which it attacks and destroys animal tissues, the question naturally arises, Why it should not act upon the stomach itself? and the common reply is, that it is owing to the fact of the stomach being endowed with life which in some mysterious way preserves it from this otherwise inevitable suicide. That this is not the true solution of the difficulty is proved by some experiments of Bernard, who introduced portions of live frogs into the stomach of a dog, and found that during the life of the unfortunate victim of science it was partially digested. And the explanation he gives is that the immunity to its own secretions enjoyed by the stomach is due to the epithelial coating which is quite impermeable by the gastric juice, and entirely resists its corroding power, and that it is the absence of such a protecting film in the frog that makes it liable to be digested alive. This theory is probably the true one, and it may serve to explain the curious fact that the poison of the most venomous snakes may be received with impunity in its raw and native state into the stomach, while it produces rapidly fatal effects when injected into the blood. But it would be contrary to all analogy to suppose that because when in its concentrated state it did not act in the stomach being

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probably digested and decomposed, that therefore it would not do so when presented to the system in a diluted form, so as to be readily absorbed into the blood, or without this absorption to be brought into direct contact with an extensive surface of the mucous membranes on which nerves are largely distributed. This may reassure us of the probable activity of snake poisons when given as we give them in a highly expanded or diluted state, even in the face of their inactivity, when swallowed in their natural bag, as is done in India.

Although it is established beyond a doubt that the gastric juice has the power of dissolving albumen, yet from the experiments of Bidder and Schmidt it appears no less certain that it requires a large quantity of the fluid to dissolve a small portion of the solid, a much larger quantity, in short, than is met with in the stomach. Not only in experiments made with gastric juice taken out of the body, but in all experiments made by dropping portions of coagulated white of egg into the stomach, through an artificial opening, they found without a single exception, that even after six hours, and when there was an excess of gastric fluid present, yet that all the albumen was never dissolved, seldom more than half of it. This startling fact leads to the inevitable conclusion that albumen is only partially digested in the stomach, and we must follow the course of the food down the alimentary canal, before we can account for the digestion even of the albumen, the only one of the three parts of the aliment which undergoes a certain gastric digestion. The pultaceous mass, called chyme, when it leaves the stomach and enters the duodenum, consists of disintegrated food, fecula, free oil, and albumen, in a state of solution,

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along with fibres of flesh, &c., before it has proceeded far on its downward course, it encounters the secretions of the pancreas and liver, which entirely change its chemical character, and arrest instantly the gastric digestion by throwing down the pepsine, and neutralizing any free acid that may have passed over. The action of the pancreatic fluid has hitherto been regarded as similar to that of saliva, and the pancreas has always been considered as an internal salivary gland, of comparatively little consequence in the process of digestion. Bernard entirely objects to this received opinion, and maintains that the action of the pancreatic fluid is of immense importance. This belief rests upon the following series of facts:— 1st. The consequences of a destruction of the pancreas in the human body by disease. 2d. Its destruction in animals artificially. 3d. That the changes in the aliment, which he ascribes to the presence of this secretion, do not take place in the duodenum of those animals in which the pancreas discharges itself lower in the intestine until the point where this discharge takes place; and, lastly, upon the experiments he has made upon the action of the pancreatic fluid extracted from animals and brought in contact with the elements of food. In order fully to understand his views it will be necessary to state his experiments, as well as those of Bidder and Schmidt upon the same subject. Unfortunately, the results are in some respects conflicting.

The pancreas is an oblong gland, with a duct running from one end to the other in a line with its longest diameter; as this central canal approaches the duodenum, where it debouches, it separates into two branches, the larger of which in man unites with the ductus choledo-

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