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With kind regards
from J. B. Sanderson

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ON THE

PROCESS OF INFLAMMATION.

BY

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THE PROCESS OF INFLAMMATION;

BEING THE COMPLETION OF THE ESSAY ON INFLAMMATION IN VOL. I.

THE author who engages to give information to others on any subject with which he is supposed to be conversant, takes upon himself a serious responsibility. His first duty is to place his readers in complete possession of all the facts relating to the subject, which have been accepted by scientific men up to the time at which he writes, including in his statement such collateral information as is necessary for correctly judging of the grounds of their acceptance. But in addition to this primary obligation the reader has a right to expect that he will not be presented with a mere narrative of unconnected observations which he must himself arrange and apply to the solution of the questions at issue, but that the work of comparison and analysis shall be done for him, and those conclusions stated in clear language which have the best claim to be incorporated in the ever-changing body of scientific doctrine.

In the preparation of the following essay on the process of inflammation, I have made it my object to fulfil both these obligations without going beyond them, deeming that by doing so I should be most likely to make my performance of practical use. I have striven, above all, to be cautious in the selection and statement of facts, remembering how often misstatements, which find their way into the writings of those who assume to teach, are apt to retain their place long after the sources whence they were derived have been forgotten.

I have myself repeated most of the observations and experiments to which I have referred. I have done so, however, not so much in the hope of adding to them or correcting them, as for the purpose of making myself conversant with the methods and results.

It is hard to have to acknowledge that during the last ten

years no research of any importance relating to the questions which will occupy us in the following pages has appeared in this country. The fact that we have to submit to receive instruction at the hands of German pathologists, instead of meeting them on equal terms, unwelcome as it is, is very easy to account for. In the present position of pathology, the methods which in times past have been employed with such signal success in this country are exhausted. Although it would be a great mistake to say that all that can be learnt by the rough investigations which can be made in the *post-mortem* theatre is already known, yet it cannot be doubted that for some years past every important advance in the science of disease has been accomplished, not by the collection of isolated observations, but by the same methods of systematic experimental research which are employed in physics and chemistry. The Pathological Institutes of Vienna and Berlin have no counterparts in Great Britain. The want of them is not only disadvantageous but fatal to progress—partly because they are necessary for the effectual carrying out of experimental inquiries, partly because, without them, that education in the methods of exact research by which alone a real pathologist can be produced is impossible.

INTRODUCTION.

By the 'process of inflammation,' I understand the succession of changes which occurs in a living tissue when it is injured, provided that the injury is not of such degree as at once to destroy its structure and vitality. With reference to their origin, all inflammations may be comprised in two classes—extrinsic and intrinsic. Of these two terms, the former is applicable to all those cases in which an injury, either sustained by the affected part or inflicted elsewhere, is the obvious cause of the morbid process; the latter to those inflammations which, from the concealment of their cause, are commonly called idiopathic. If, however, we desire to speak accurately, we must discard this word altogether; for there is no case in which it can be reasonably doubted that an injury must have preceded the earliest sign of local disorder, however little we may know either of the nature of the agent or of the mode of its action. We might advantageously substitute for idiopathic either of the words intrinsic or secondary; but inasmuch as there is no channel by which an agent from within, i.e. from some other part of the body, could penetrate into a tissue, excepting by the blood-vessels or lymphatics, we are entitled to use the only word which fully expresses this view of the mode of introduction of the material cause, and to designate all so-called idiopathic inflammations *infective*.

From what has been said it may be readily understood that the primary inflammations naturally affect those parts principally which are exposed to external influences, while those of the other class occur by preference in parts and organs to which there is no access excepting through the circulation. These distinctions, however, are not constant, for there are many instances in which secondary inflammations affect external parts, and many others in which internal organs are the seat of primary inflammations, as for example when nephritis arises from exposure to cold. Much more important distinctions, however, may be based on a comparison of the structural changes which the two processes determine in the tissues affected; or, in other words, on their pathological anatomy. In making this comparison, there is one important principle to be borne in mind. *In all inflammations, the form of the lesion is dependent on that of the area of influence of the injury.* Thus, in those cases of primary inflammation in which it may be supposed that an impression received by afferent nerves distributed to mucous or cutaneous surfaces, is reflected to internal organs (as in the case of nephritis from cold, already referred to), the area of influence of the injury is wide enough to comprise whole organs, and the resulting lesions are of corresponding extent. In the strictly local inflammations, the correspondence in form between cause and effect is, of course, closer and more obvious, the area of a traumatic inflammation being larger than that of the injury which produces it, but of exactly similar form. As regards infective inflammations, the correspondence is not so plain, but the consideration of their pathological anatomy is sufficient to satisfy us that it is equally complete. It is the anatomical character of all infective inflammations that the lesions to which they give rise are disseminated rather than diffused. Particles of matter, of the nature of which we can assert nothing, excepting that they are of extreme minuteness, are conveyed from a primarily inflamed part to other parts previously healthy, and become foci of infective induration or suppuration (miliary tubercles, pyæmic

abscesses) each of which is the product—if one may be allowed the expression—of a single seed.

Although in a treatise on inflammation all the forms of the process ought to be discussed, I have thought myself justified in omitting the whole subject of secondary indurations and suppurations on the present occasion; not that I underrate its importance, but that the material for its satisfactory discussion is still wanting. The intimate pathology of the process of infective inflammation (e.g. tuberculous, scrofulous, or pyæmic) has been only very recently subjected to experimental investigation, so that, although pathologists are beginning to see the bearing of the facts already observed on clinical experience, the subject is not yet ripe for dogmatic exposition.

For a similar reason the consideration of those cases in which inflammatory processes originate at a distance from the locality directly affected by the injurious agent, must also be omitted; for the ideas we at present entertain with respect to them are not founded on experiment, but merely inferred by analogy, i.e. by comparison of what occurs with other known processes. The actual limits of pathological knowledge seem therefore to confine the scope of the present article to the discussion of those purely local inflammations which arise in a tissue in consequence of the direct application of injurious stimulation.

The purpose of this article is therefore to describe the effects of injurious irritation of tissues. Enumerated in the order of their apparent occurrence, they are (1) disorder of the circulation, (2) transudation of the constituents of the blood, and (3) altered mode of growth of the elements of the inflamed texture. As, however, it is more convenient to divide the consideration of these several derangements of function according to their seat than according to their nature, I propose to describe them under two headings—the first comprising all those changes which have their seat in the blood-vessels; the second, the alterations of the tissues.

SECTION I.—CHANGES WHICH HAVE THEIR SEAT IN THE BLOOD-VESSELS.

PART I.—DISORDER OF THE CIRCULATION.

When a grain of dust is accidentally introduced underneath the upper eyelid, much pain is felt, and the conjunctiva becomes vascular. This effect occurs so rapidly that it is difficult to suppose that the obvious dilatation of the vessels has been preceded by a preliminary state of contraction. On the other hand, we know from direct observation and measurement that if we irritate a minute artery, it contracts at the point of irritation. How are these two apparently opposed facts to be reconciled? Are we to suppose that, notwithstanding the shortness of the time that intervenes between the application of the stimulus and its effect, the apparent paralysis has been preceded by a transitory condition, or are we to believe that the condition of the arteries which leads to the increased activity of the capillary circulation is intermediate between that of complete relaxation and that of spasm?

Before entering on the consideration of this question, it will be well to give a short account of the vascular changes as they are seen in actual progress in the transparent parts of certain of the lower animals. For years the web of the frog's foot was the only field of observation. Now that, by the use of curare, we are enabled to obviate the difficulties arising from muscular movements, preference is often given to the tongue or the still more transparent mesentery.

When the mesentery is spread out (in the way to be hereafter more particularly described) for microscopical examination, the first change which is observed in the circulation, as a result of exposure to air, consists in dilatation of the arteries; the increase of width being accompanied by a corresponding increase of length, which manifests itself in more or less contortion. The dilatation begins immediately, and is ushered in by no antecedent stage of contraction. It is, however, progressive; the diameter of the artery gradually increases for ten or twelve hours, at the end of which period it is often twice as great as it was before; having thus attained its maximum, its size remains unaltered for many hours. This dilatation of the

arteries is followed by a similar change in the veins, but inasmuch as there is a considerable interval between the two events, a time occurs at which the arteries, instead of being sensibly smaller than the veins which correspond to them, far exceed them in diameter.

Along with these changes the rate of movement of the blood is also altered. At the beginning of the process the circulation is quicker than natural. Yet although the two changes go on at the same time the acceleration cannot be regarded as a result of the increase of calibre; for the inevitable consequence of dilatation would be diminution, not increase, of the rate of movement, supposing the activity of the heart and the resistance opposed by the capillaries of distribution to remain the same. The absence of any causal relation between the two is still more clearly shown by what is observed at a later period; for whereas on the one hand, as has already been stated, the dilatation lasts for many hours, the acceleration is confined to the first stage of the process. The rate of movement soon returns to the normal, and this is shortly followed by a change in the opposite direction; so that by the time the arteries are fully dilated the circulation is much slower than it was originally.

Such are the main facts as they occur in the frog's mesentery. In so far as every inflammation begins with increased activity of the capillary circulation of the affected part, which is followed by diminished circulation, they may be considered as representative. Nothing, however, can be learnt from them as to the relation between these changes and the variations which occur along with them in the degree of contraction of the vessels themselves. For the study of this relation we must have recourse to other tissues in which the conditions of vascular contraction are better understood than they are in the mesentery. But before doing so it appears necessary to give a short account of what is at present known as to the influence of the nervous system on the blood-vessels.

During the last ten years important additions have been made to our knowledge of the innervation of the arteries. Many new facts have been discovered, and others previously known are better understood. To attempt fully to discuss them would exceed the scope of this article. I shall, therefore, confine myself strictly to those physiological considerations which have an immediate bearing on the disorder of the circulation which manifests itself in inflammation. Until Bernard proved by experiment that the nerves which preside over the arteries of the integument of the head are contained in the cervical portion of the sympathetic, the very existence of vaso-motor nerves was merely matter of inference. For a long time after that discovery, physiologists had no precise knowledge of the vascular nerves of the rest of the body. More recently, the mode of innervation of many other parts and organs has been demonstrated experimentally, particularly the right of the splanchnics to be regarded as the vascular nerves of the abdominal viscera, and the derivation of the vascular fibres of the upper and lower extremity from the sympathetic system, by means of communicating branches passing between that system and the anterior roots of the spinal nerves. By these researches the doctrine which has long been considered probable, viz. that all vascular nerves pass through the ganglionic nervous system, has been established. At the same time, it has been shown that although the vascular nerves are immediately derived from the sympathetic, their ultimate origin is to be found in the cerebro-spinal nervous system, as evidenced by the fact that when any part of the ganglionic cord is isolated by the division of its spinal attachments its vaso-motor functions are paralysed, the same vascular effects being produced as if the sympathetic were itself destroyed. We further learn that the vaso-motor nerves are not only subject, like other efferent nerves, to the direct action of stimuli, but that they may be excited in the reflex way by stimulation of certain afferent spinal nerves. And hence we are compelled to admit that the whole vaso-motor system is under the control of an excito-motory centre. The precise position of this centre is as yet uncertain. We know, however, that it is in the intra-cranial part of the cord: in the first place, because some of the afferent nerves in relation with it are cranial, and secondly because section of the cord immediately below the *medulla oblongata* produces paralysis of the whole vascular system. Of the afferent nerves above referred to, the most important is that which is now known as the depressor,* a branch of the vagus, the excitation of which by a feeble interrupted current leads to a general reflex acceleration of the flow of blood through the capillaries.

* For a full account of this subject, see E. Cyon and C. Ludwig, *Die Reflexe eines der sensiblen Nerven des Herzens auf die motorischen der Blutgefässe.* Ludwig's *Arbeiten*, 1867, p. 77.

As in other parts of the nervous system, the special physiology of the vascular nerves is known almost exclusively by experiments, in which the effects produced by the stimulation or division of particular nerves are observed. The most important results of this kind of investigation are, that section of a vascular nerve produces congestion of all the tissues to which it is distributed; that excitation by the interrupted current, or by mechanical means, produces constriction of the minute arteries presided over by the irritated nerve, and consequent anæmia; *that excitation of a sensory nerve produces increased activity of the capillary circulation in the part in which the nerve originates*; and, finally, that all arteries manifest alternating states of contraction and dilatation, their rhythmical movements being entirely independent of those of the heart and of breathing, and ceasing when the vessel is paralysed by division of its nerves.*

Of these results, the one which has the most direct relation to our present inquiry is the third. It is founded, as regards mammalia, on the well-known researches of Ludwig and Lovén, of which I content myself with giving a very cursory account, referring the reader to the original paper for more complete information.

All of Lovén's experiments were made on curarised rabbits † in which respiration was maintained artificially, so as to avoid the disturbing influence of muscular movements. The nerves selected for excitation were the large nerves distributed to the external ear of the rabbit, and the *dorsalis pedis*. When the central end of a divided auricular nerve is excited by feeble induced currents, congestion of the corresponding ear follows in a period which varies from three to six seconds. This congestion is more intense than that produced by section of the sympathetic, and is accompanied with obvious dilatation of the arteries,

* Of these results, the first and second may be easily demonstrated in the rabbit by section of the cervical sympathetic followed by excitation of the peripheral end of the divided nerve: in the frog by section and excitation of the spinal cord. The effects of the excitation of the depressor nerve can only be shown in the rabbit. The modes of experiment required are described in my physiological lectures recently published in the *Medical Times and Gazette*.

† I have found, however, that all the facts observed can be demonstrated in animals under the influence of chloral (six grains or more in solution injected into a vein). In this way the experiment is rendered much easier and can be done without inflicting any pain on the animal.

varying in duration according to the degree of the excitation, and the time for which it is continued.* In the experiments in which the *n. dorsalis pedis* was excited, the *arteria saphena* was made the subject of observation. As this vessel in the rabbit is easily exposed in its long course down the inner surface of the thigh, derives its vaso-motor branches exclusively from the *n. saphenus*, and is distributed in great measure to the same region as the *dorsalis pedis*, it is particularly suited for the purpose. The results are as striking as in the other case. The vessel begins to enlarge and pulsate visibly a few seconds after the commencement of the stimulation. The dilatation soon attains its maximum and begins to subside, lasting only a very short time after the removal of the electrodes from the nerve.†

In the frog the vascular nerves which supply the web find their way by various channels to the arteries to which they are distributed, so that there is no single trunk by the division of which these vessels are completely paralysed. It is probable, indeed, that the distribution of the vascular filaments differs in different individuals, for while in some frogs division of the sciatic nerve in the thigh widens the arteries very distinctly, in others it produces no appreciable effect, either on the state of the vessels or on the activity of the circulation. There is a similar uncertainty in the results produced by exciting the peripheral end of the divided sciatic, which obviously, if that nerve always contained vaso-motor filaments, ought always to induce arterial contractions. In some frogs it is so, i.e. when the peripheral end is excited, the arteries contract markedly, and the circulation is suddenly arrested, but in others the effect is so inconsiderable as scarcely to admit of demonstration. If, however, the central ends of the divided sciatic be excited, the opposite effect—namely, increased activity of the circulation—shows itself with much greater constancy, proving that however variable may be the proportion in which vaso-motor filaments are contained in the sciatic nerve, the arteries of the web are always supplied more or less completely through other channels.

The accelerating influence of excitation of the central end of

* In my experiments, the dilatation had often disappeared ten or twelve seconds after the commencement of excitation.

† Chr. Lovén, *Ueber die Erweiterung von Arterien in Folge einer Nervenerregung.* Ludwig's *Arbeiten*, 1867, p. 1.

the divided sciatic on the circulation has been lately so carefully studied by Professor Stricker and Dr. Riegel that there can be no doubt of its nature. The method they employ consists in comparing the movement of the blood-corpuscles in a selected arteriole with that of a current of water containing particles of solid matter in suspension, which is so arranged as to pass through a horizontal tube fixed on the eye-piece of the microscope at such a distance from the eye-glass as to be distinctly seen by the observer. The apparatus by which this current is produced is so constructed that its rate can be varied at will, and its actual velocity at any given moment can be determined. The comparison is made by first fixing the attention on the arterial current, and then accelerating or retarding the test-current until the two velocities are equal. By this means it is obvious that any diminution or increase in the rate of movement can be appreciated with the greatest exactitude. With a view to the observation, the frog must be slightly curarised ($\frac{1}{2000}$th of a grain of curare in solution injected under the skin). The sciatic nerve having been divided on one side, the web is placed under the microscope, so that a small artery passes through the field in a direction which coincides with that of the test-current. As soon as the two movements have been brought to agreement, the central end of the sciatic is excited by a moderate current, immediately after which the acceleration begins, and goes on increasing so long as the irritation is continued, even when the observation lasts for half an hour, or longer.* In all Dr. Riegel's experiments the acceleration of the blood-stream was associated with some narrowing of the vessels. This observation was so carefully and so frequently made by him, that I should not doubt of its reality, even if I had not satisfied myself of its truth by repeating it. Its importance is obvious, for it affords the strongest ground for believing that in certain states of the arteries accelerated flow of blood may be associated with persistent reflex arterial contraction.

There are several instances known to physiologists in which contractions of arteries are produced which are not attended with increased activity of capillary circulation, but, on the contrary, with anæmia—as, for example, by excitation of the peripheral end of the sympathetic in the neck after division, in the

* Riegel, *Ueber die reflectorische Innervation der Blutgefässe. Med. Jahrbücher* 1871, p. 101.

rabbit, or by excitation of the spinal cord in the frog. If this were only the case when vaso-motor nerves are irritated directly, we could more readily understand it; but it also happens under conditions which so closely resemble those which we have been considering, that at first sight the results are difficult to reconcile. Saviotti has lately found that by exciting the cutaneous surface in various ways, e.g. by tapping on the integument of the belly, by pricking the skin of the same part or of the back, or by pinching the toes, very marked contraction of the arteries of the web of the frog's foot can be produced, which is attended not with increased but diminished progressive movement of the blood, amounting for the moment to complete arrest of the circulation. As I have said, this looks at first contradictory, but before we judge of its bearing on our present question we must call to mind that the conditions of Saviotti's and Riegel's experiments are not so comparable as they seem. It is well known as regards some at least of the modes of irritation employed by Saviotti, that they act not merely on the vaso-motor nerves but on the vagus heart-nerves. I need not remind the reader that tapping on the belly of the frog arrests the movements of the heart in diastole just in the same way as direct excitation of the vagus itself does. Consequently in Saviotti's experiment we are not merely obliged to admit the possibility that the arrest of movement is partly cardiac, but are tolerably certain that it must be so. This consideration is of great importance; for although there is no doubt that anæmic contraction of arteries is an ordinary consequence of direct stimulation of vaso-motor nerves, there is no case (except that of Saviotti's experiments) in which anæmia is produced by reflex irritation.*

Our knowledge of the innervation of the blood-vessels is, notwithstanding the progress which has been made during the last few years, too imperfect to enable us to harmonise all the facts. But the impossibility of constructing a complete theory on the subject does not prevent us from drawing some inferences which will be of use in enabling us to understand what happens in inflammation, at all events better than we should do without them. From what has been stated, it is tolerably clear that whatever difference there may be in other respects, there is one

* Saviotti, *Untersuchungen über die Veränderungen der Blutgefässe bei der Entzündung.* Virchow's *Archiv*, vol. i. p. 592.

effect of exciting the sensory nerves distributed to any part which is pretty constant, viz. increased activity of the circulation; so that whether the actual quantity of blood existing in the part at any given moment be greater or less, the quantity of blood which passes through it in a given time is certainly greater.

In the commencement of the process of inflammation in the web of the frog's foot the successive changes are similar to those I have already described in the mesentery, but differ considerably according to the irritant employed. Most irritants, such as weak solution of caustic soda, dilute sulphuric acid, &c. produce dilatation first of the arteries and subsequently of the capillaries, with marked acceleration of the circulation—these conditions being followed by arterial contraction and capillary anæmia. But liquor ammoniæ and carbonate of ammonia in substance, appear always to occasion a certain degree of primary arterial contraction, which begins in one or two minutes after excitation, and is attended with retarded flow of blood through the capillaries, with distension of the branches given off by the artery nearer the heart, and increased activity of circulation in the neighbourhood of the irritated part. This state of things lasts for an hour or two, and is succeeded by dilatation and acceleration. In other words, ammonia and carbonate of ammonia produce results which are directly opposed to those of other stimuli. Croton oil appears to occupy an intermediate position between the first-named stimulants and ammonia, for while it always gives rise to acceleration of the flow of blood as a primary result, this change is sometimes associated with widening, sometimes with narrowing of the arteries. So that here, as in the case of reflex electrical stimulation of sensory nerves, the only fact which is constant is acceleration.

In order to judge whether the two kinds of acceleration we have been considering are of the same or opposite nature, the best way is to observe their action simultaneously in the same part. If, for example, in the web of the frog's foot the acceleration due to excitation of the central end of the sciatic is of the same nature as that of inflammation, we should expect it to be increased by local irritation; and, conversely, the effect of irritation, if already existing, to be heightened by exciting the nerve. The very careful experiments of Dr. Riegel show that it is so. Having found that after section of the sciatic the effects of irritation were slightly retarded but otherwise unmodified, he repeated the observation in another animal, excited the central end, and then applied croton oil to both webs. On the injured side the accelerating effect of the croton oil lasted much longer than on the other, so that at the time stasis had already set in on the sound side, the circulation was going on more briskly than natural on the injured side.

As regards the precise nature of the modification of vascular contractility which is associated with the primary quickening of the capillary circulation, we cannot venture to speak in any terms more precise than have been already employed. The effect of local irritation is certainly not to paralyse the arteries leading to the irritated part, but rather to modify their *tonus* in such a way as to facilitate the flow of blood through them. For the present we must be content to leave the question open, for no good would be gained by endeavouring to conceal the insufficiency of our knowledge under a comprehensive theory.*

In all forms of inflammation of sufficient intensity, the circulation after a variable period of excitation becomes retarded. This effect is so closely associated with the other phenomena of

* Many physiologists are of opinion that the arteries do not act merely as dead elastic tubes, but are endowed with powers of contraction analogous to those of the intestine. If this be admitted, it can be easily shown that the quantity of blood conveyed by an artery in a given time, and consequently the activity of the capillary circulation, might be increased by alterations in the contractility of the tube of a nature the very opposite of that of relaxation. Such a theory supposes that an artery, after it is distended by the injection of blood from the heart, does not content itself with returning to its state of elastic equilibrium, but that at a variable interval after receiving the systolic shock of the heart it contracts actively on its contents, just as a bit of intestine would do, and thus assists in propelling them. Admitting it to take place, the effect of such contraction would depend not merely on its intensity, but on the relative duration of the period of arterial distension as compared with that of the succeeding collapse. Thus if the contraction were so immediate as to happen while the artery was still acted upon by the heart, and consequently to coincide with what would otherwise be the period of greatest distension if the artery were a dead elastic tube (that is to say, about a tenth of a second or so after the shock), it is evident that it would tend to impede the circulation by increasing the resistance of the artery; but if, on the other hand, it were postponed until after the operation of the *vis a tergo* had ceased, it would not only keep up the flow of blood during what would otherwise be the period of greatest retardation, but would prepare the way for a more effectual filling of the artery at the next systole by previously emptying it of its contents.

the second stage of the process of inflammation, that it cannot be advantageously studied until they have been considered (see p. 745).

Part II.—Exudation of Liquor Sanguinis and Leucocytes.

It is now many years since it was taught by Dr. C. J. B. Williams, as the result of his own observations on the phenomena of inflammation in the web of the frog's foot, that in the second stage of the process, when the capillary circulation is becoming retarded, there is an apparent increase in the number of white blood-corpuscles in the vessels, and that they manifest a remarkable 'disposition to adhere to their walls.'* Dr. Williams attributed these appearances to the production in the vessels of inflamed parts, of young colourless blood corpuscles differing from those ordinarily met with in their consisting, not of cells (in the sense in which the word was then, and for many years afterwards understood, as implying the existence of nucleus, membrane and contents), but of masses of gelatinous consistence (p. 328). He considered that their tendency to adhere to the internal surfaces of inflamed vessels, and to creep along them, was due to their not having membranes (p. 331). He further observed that 'in the frog's web, after inflammation has continued for some hours, there appear outside of the vessels, especially where the strongest current encounters the most complete obstruction, white globules or corpuscles with specks in them, exactly like the pale granular globules within the vessels' (p. 335). He did not, however, suppose that the objects so exactly resembling each other which he saw outside and inside of the membrane respectively, were in reality identical; for although Dr. Addison had already maintained that pus globules and the white globules of the blood were indistinguishable from each other, and had represented that in inflammation the white globules first passed into the substance of the wall of the blood vessel, and were then thrown out from it, it appeared to Dr. Williams so difficult to understand their passage through the walls of vessels in which

* *Principles of Medicine*, 3rd ed. p. 330.

no pores are visible under the highest magnifying powers, that such an explanation could not be accepted.

In 1846 the statements of Dr. Addison were confirmed by the late Dr. Augustus Waller. His paper on the subject displays such clearness in the description of the phenomena he observed as to leave no doubt in the mind of the reader that he actually saw what he represents. 'In some instances,' says Dr. Waller, 'the manner in which the corpuscle escaped from the interior of the tube could be distinctly followed; that part of the tube in contact with the external side of the corpuscle gradually disappeared, and at nearly the same time might be seen the formation of a distinct line of demarcation between the inner segment of the corpuscle and the fluid parts of the blood in contact with it. Any slight agitation then was capable of disengaging the corpuscle from the vessel to which it was now external,' &c.* This passage is taken from a description of one of Dr. Waller's experiments. I quote another which contains his explanation of the phenomena. 'In endeavouring to account for the fact of the passage of the corpuscles through the vessels, we find considerable difficulties. It cannot be referred to the influence of vitality, as it is observed likewise to take place after death. It may be surmised, either that the corpuscle, after remaining a certain time in contact with the vessel, gives off by exudation from within itself some substance possessing a solvent power over the vessel, or that the solution of the vessel takes place in virtue of some of those molecular actions which arise from the contact of two bodies; actions which are known as exerting such extensive influence in digestion, and are referred to what is termed the catalytic power' (p. 402). That the speculations and observations of Dr. Addison, even when so definitely confirmed by Dr. Waller's experiments, fell into oblivion, is to be attributed partly to the theories about cells which then prevailed, and partly to the extreme difficulty of the investigation. For it is to be remembered, to Dr. Waller's great credit, that neither curare nor chloroform, which have since so wonderfully facilitated research, were at his disposal, and that consequently anything like minute observation of the phenomena was rendered almost

* Microscopical Observations on the Perforation of the Capillaries by the Corpuscles of the Blood, and on the Origin of Mucus and Pus Globules. By Augustus Waller, M.D. *Philosophical Magazine*, vol. xxix. p. 397, 1846.

impossible; for it was only by patiently waiting for short moments of tranquillity that the observer could see anything.

Before proceeding to the consideration of the discoveries which have rendered Professor Cohnheim's name so well known, it is desirable to give some account of the successive steps of investigation by which the true relation between the colourless corpuscles of the blood and other similar forms occurring in the tissues, either in health or disease, has been recognised. The common physiological property by which all these bodies are associated is that of spontaneous movement, manifesting itself either in progression or merely in continuous change of form. The bodies possessing this property are called in German by the terms *bewegliche Körperchen*, *Wanderzelle*, *Lymphkörperchen*, for which I propose to employ the English equivalent leucocyte,* understanding it to mean a mass of contractile living protoplasm. The importance of this definition in relation to our present inquiry is very great, for so long as a blood-leucocyte was supposed to be a cell, in the sense in which the term cell was used twenty years ago, it was quite impossible to understand how it could find its way through a structureless membrane; but from the moment that it was understood to be a mass of contractile material similar in all respects (which can be judged of by observation) to that which forms the body of an amœba, and endowed with a similar faculty of movement, the process became much more intelligible. Although the comparison between the movements of amœbæ and those of leucocytes is so familiar, it cannot be considered either an undue digression from the subject, or a waste of the reader's time, to recall to his recollection some of the facts relating to the mode of life of these organisms which fit them to serve as illustrations of the contractile corpuscles of the higher animals.

With this view the best examples which can be selected are the gigantic amœbæ which are known to biologists as the

* Dr. Williams, in a recent note (*Med. Times*, Jan. 21, 1871), objects to the term leucocyte, that the body to which it is applied is not a cell, and suggests the adoption of a new word, viz. Sarcophyte, which corresponds more exactly with its anatomical characters and its physiological properties. I have nothing to advance in answer to Dr. Williams' arguments. Sarcophyte is clearly the more expressive and accurate word, but it is unknown; whereas leucocyte is well understood on both sides of the Channel. Moreover, the word cell, and its Greek equivalent, have so entirely lost their original meaning, that surely no misunderstanding can arise from their use.

Plasmodia of the Myxomycetes, and the more familiar forms which are closely related to the Monads. The myxomycetes, although possessing some of the characteristics of animals, have been always, on account of their development and mode of growth, associated with the fungi.* Like other fungi, they originate from spores. If the spores of *Physarum* (a genus of myxomycetes) are sown in water on an object-glass, and examined under the microscope twenty-four to thirty-six hours afterwards, the water is seen to be peopled with contractile corpuscles, each of which is at first provided with a single cilium, and contains, in addition to a contractile vesicle, a delicate vesicular nucleus, usually placed in the neighbourhood of the cilium. In its original state the corpuscle moves about so actively that the contractions of its substance cannot be studied; but after a while the cilium falls off or is retracted, and it then assumes in every respect the aspect and character of an amœba. Let us for a moment study its motions.

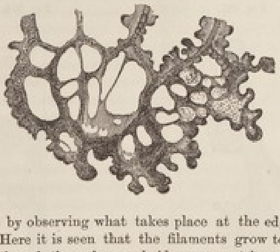
The mass is constantly changing its form. But as these changes go on in all parts of the hyaline substance of which it consists, simultaneously, the only way in which they can be understood is by confining the attention to one point at a time. If this is done it is seen that each act of movement begins by the budding out of a ray or process of contractile substance in a centrifugal direction. What next happens varies in different cases. Sometimes the projection subsides just in the same way as it was formed; at others the finely granular fluid, or rather labile matter, which occupies the more central parts of the corpuscle, streams into the offshoot, gradually widening it out, until it grows into a mass greater than the remainder, which it finally draws into itself. It is evident that the process last described must always be attended with locomotion, for each time it is repeated the whole mass rallies round a new centre, the position of which corresponds to the extremity of the offshoot. If the amœba always sprouted in the same direction, the progress would be continuous and rectilinear; as, however, there is no appreciable order in its efforts, its locomotion is correspondingly irregular.

The form of the adult plasmodium of Didymium, another genus, is shown in Fig. 396. It consists of a reticular film of grey

* L. Cienkowski, *Das Plasmodium. Zur Entwickelungsgesch. der Myxomyceten. Jahrbücher für wissenschaftliche Botanik*, vol. iii. pp. 325, 400.

fibres which spreads over the rotten leaves on which the plant vegetates, and would be regarded by the casual observer (if he recognised its claim to be considered a living structure at all) as undoubtedly a fungus. Under the microscope it is found to be neither more or less than an enormous mass of contractile protoplasm; for every part of it is constantly undergoing changes of form similar to those already described in the minute amœba, with which (as Cienkowsky's researches have clearly shown) it is organically continuous. As illustrative of the manner in which hyaline contractile material may shape itself into specific form without the intervention of cells, it is well worthy of our attention. Its mode of growth can be best

FIG. 396.



understood by observing what takes place at the edge of the network. Here it is seen that the filaments grow terminally, and that although there is amœboid movement in every part, this movement is much more active at the growing points than elsewhere. It is further seen that the process by which the growing end lengthens, is exactly similar to that by which the original amœba throws out rays. In each filament the outer part appears to be hyaline and contractile, the central part labile and granular; and when the process of elongation is carefully observed, it is seen to consist first of a budding out of the external substance, and secondly of an afflux (preceded by more or less marked alternations of ebb and flow) of the axial semi-fluid matter towards the growing point. The reticular arrangement of the filaments results from the fact that every marginal growing end meets with another, with which it unites so as to form a loop. The union, however, is not instantaneous, but gradual. For a time the two ends are merely in contact, the labile axial matter being separated by a double septum of hyaline substance. Gradually the septum melts away, and a channel of communication is established in which the ebb and flow of currents can be distinguished.

The purpose I have had in view in giving this short sketch of the mode of life of the myxomycetes is to show that in contractile protoplasm the two functions of motion and growth are, so to speak, confused together in such a manner that the more closely we scrutinize the mode in which they are exercised the more difficult does it appear to distinguish them. No reference has as yet been made to another power which all amœbæ possess—that of absorbing the nutritive substances with which

Fig. 397.

A plasmodium beginning to surround a vegetable cell containing chlorophyll.

Fig. 398.

At the lower part of the figure a Vampyrella is beginning to adhere to a cell containing protoplasm and chlorophyll granules, which it is about to plunder. The other two cells have been perforated and their contents drawn into the bodies of the Vampyrellæ, which have retracted their processes.

they come into contact. This property is manifested in perfection in the colossal amœbæ we have been studying, which not only appropriate material derived from the soil with which they are in contact by their external surfaces, but surround their food with their own substance for the purpose of digestion (Fig. 397). I prefer, however, to seek for an illustration in the

history of those more minute forms which, from their close relation to the Monads, have always been regarded as animal.

I select as an example an amœba called by Cienkowski *Vampyrella Spyrogyræ*,* the specific name of which is derived from the confervoid alga on which it lives parasitically. Its appearance is shown in Fig. 398. It is granular towards the centre, hyaline at the surface, and the superficial part shoots out into pointed rays, or less frequently buds into obtuse promontories, in which an oscillation of granular matter is seen similar to that already described. It moves about apparently without purpose until it meets with a filament of Spyrogyra. It then adheres to the external cellulose membrane of the alga, and soon penetrates it. It thus comes into contact with the protoplasmic lining (primordial utricle) of one of the cells of which the filament is composed, which it at once proceeds to exhaust of its chlorophyll, drawing it into itself through the narrow opening which it has made in the cell wall. After having plundered several cells in this way, the amœba, much increased in size, entirely ceases its movements, and becomes enclosed in a distinct envelope, in the interior of which new amœbæ are formed by an endogenous process which it is unnecessary to describe.

Although the morphological relations of amœbæ are so various and in many instances so undetermined that no single example can be taken as typical, yet those above related may serve to render more intelligible the following general statement. It appears to be well ascertained that it is the destiny of every amœba, after it has enjoyed its active life for a certain time, to assume the immobile condition, in which it becomes invested in a membrane; and that as this transformation is always preparatory to its entering on the reproductive function, no amœba can itself be a parent. It is also certain that all amœbæ, so long as they continue in the active state, are endowed with a remarkable power of dissolving and absorbing the nutritive substances on which they live. In connection with this faculty, the exercise of which may be regarded as the final cause of all their movements, they are able, at one time, to surround the material to be digested with the substance of their own bodies, at another to penetrate membranes in which there are no visible

* Cienkowski, *Beiträge zur Kenntniss der Monaden*. Schultze's *Archiv*, vol. i. p. 203.

pores, when such membranes are interposed between them and their food.

Amœboid movements of leucocytes.—The first precise observations on the amœboid movements of leucocytes were those of Professor v. Recklinghausen, contained in a paper on suppuration which appeared in Virchow's *Archiv* in 1863.

The publication of this research may be well considered as the commencement of a new era in histology,* not so much on account of the importance of the facts announced in it, as because the author enforced a principle which, at all events in its application to pathology, was at that time new, though it is now recognised by every one—namely that the elements of tissues, especially those in which life is most active, are so altered in the very act of dying, that the appearances they exhibit when dead and still more when disfigured by immersion in such liquids as acetic acid or water, are mere caricatures of their true aspects; for although the dead remains may be full of instruction, yet if we wish to know organic forms as they are, they must be studied either in the living state or at all events under physical and chemical conditions resembling those of life as closely as possible.

Guided by this principle of research, Recklinghausen was able to show, for the first time, that the changes of form of leucocytes are of the same nature as those of amœbæ, that they are capable of surrounding particles of any kind, if sufficiently small, with their own substance, and that they possess the power of moving from place to place. The first of these facts is established in his paper by observations on the pus corpuscles which are to be found in the liquid obtained by puncturing the anterior chamber of the eye of the frog a few days after keratitis has been produced by the application of nitrate of silver to the cornea. The description given of the movements as they are seen in the turbid humor aqueus, provided that it has

* It is somewhat difficult to state to whom the merit of having discovered the amœboid movements of leucocytes is to be assigned. There is no doubt that they had been noticed by several anatomists before they were made the subject of special investigation. Among the more important early observations on the subject may be noticed those of Virchow on the corpuscles of hydrocele fluid (Virchow's *Archiv*, vol. xxviii. p. 238), of Lieberkühn on the fluid of ascites (Müller's *Archiv*, 1854, p. 15), and those of Messrs. Busk and Huxley on mucous corpuscles, in a note to their translation of Kölliker's *Microscopical Anatomy* (p. 46).

undergone no change either by evaporation or pressure, is as follows: 'The corpuscles differ very strikingly in their form

Fig. 399.

Amœboid leucocytes (after v. Recklinghausen).

from those from which the ordinary descriptions are taken. . . . No globular forms present themselves—only jagged ones, and the prongs vary both in length and number. But what strikes one even after very brief examination is, that each corpuscle is constantly changing its shape. While one prong withdraws itself into the body of the corpuscle, another juts out. Each prong is at first a delicate, homogeneous, somewhat shiny thread, but soon it thickens at the base, lengthening at the same time; then gradually the substance of the corpuscle tends more and more towards it, becoming smaller as the process gets larger, the whole thus assuming an oblong or protracted form. During this transformation . . . the tip of the process is rounded off and subsides into the contour of the corpuscle; or new delicate thread-like processes shoot out, which again undergo the same changes.' *

The ingestive power of leucocytes is proved by experiments in which Recklinghausen introduced milk into the lymph cavities of frogs, the result being that the blood leucocytes became 'choked with milk globules.' Subsequently he injected finely divided vermilion with like effect, and in this way introduced a method of research which has since been much employed by pathologists—that of distinguishing the blood leucocytes from those indigenous to the tissues, by feeding the former with some insoluble colouring matter injected into the circulation.

The proof of the faculty of locomotion is derived from another experiment which is in fact only a continuation of that already mentioned. Vermilion having been previously injected into a lymph cavity of a living frog, a rabbit's or dog's cornea, taken from an animal several hours dead, is introduced into it, and

* V. Recklinghausen, *Ueber Eiter- und Bindegewebskörperchen.* Virchow's *Archiv*, vol. xxviii. p. 157.

left there two or three days. The lymph sac of course becomes inflamed, and it is found on removing the fragment that whereas the centre remains transparent, the more superficial parts are turbid. And if the preparation is immediately submitted to microscopical observation under conditions consistent with the maintenance of life (i.e. immersed in a nutritive liquid), it is found that the turbidity is due to the presence of amœboid leucocytes in the cavities of the dead tissue. That these elements are derived from the purulent liquid in which the fragment is bathed, and must therefore have found their way into the positions which they occupy by migration, is evidenced not only by their vital movements and their containing vermilion granules, but by their exhibiting the size and other characters of leucocytes of the frog.*

Even at this early stage in the investigation, v. Recklinghausen recognised the bearing of his discoveries on several difficult questions relating to inflammation; as e.g. in explanation of the tendency of pus to find its way to the surface or into the great cavities of the body, and of the mode in which disseminated foci of suppuration originate; and he also brought them into relation with the notions entertained by earlier pathologists as to the part taken by blood-leucocytes in the formation of new tissues. He even went so far as to attribute the collection or accumulation of leucocytes on the surfaces of inflamed serous membranes to migration; but assumed that they must have originated by 'proliferation' in the neighbouring connective tissue, and must have followed pre-existing channels in their wanderings. Thus, although it may be said that he did not himself, so to speak, complete his own discoveries, and left it to others to develop them to their necessary consequences, there can be little doubt that he gave a new impulse to patho-

* This experiment has been since repeated in a variety of ways. The most striking is that of Prof. Lortet of Lyons, who found that when any porous substance is introduced into a suppurating cavity, the leucocytes penetrate into it in the same way as they do into the dead cornea. And if the experiment is so arranged that the porous material encloses a liquid (as e.g. when the swimming bladder of a small fish filled with water or saline solution is inserted into an abscess), the liquid soon becomes peopled with leucocytes. The same thing happens when a similar pouch, shaped out of vegetable parchment, is used. Lortet's experiment, as well as those of v. Recklinghausen, have been repeated in the Physiological Laboratory of University College during the past winter. More full information respecting them will be found in the author's lecture on *Leucocytes*, already referred to.

logical research, the effect of which has shown itself in the more brilliant achievements of Cohnheim and Stricker.

The experiment by which Cohnheim first demonstrated the escape of the blood leucocytes in the early stage of inflammation is as follows.* A male frog, which has been paralysed by injecting under the skin about $\frac{1}{1000}$ grain of curare an hour before, is secured on a plate of glass of convenient size for the purpose. A vertical incision is then made in the abdominal wall about half an inch in length, extending from the lower edge of the liver downwards. As much of the small intestine is then drawn gently out of the visceral cavity as is necessary in order that the mesentery may be evenly spread on a disk of glass which is fixed in a convenient position for the purpose. If the operation is performed with care and skill, it may be effected without bleeding and without in the slightest degree deranging the circulation.†

In order to obtain a general view, it is best to commence the examination with a low power. It is then seen that the arteries are smaller than the veins, the latter exceeding the former in diameter by about a sixth, that the arterial stream is quicker than the venous, that it is accelerated appreciably at each beat of the heart, and that in every artery a space can be distinguished within the outline of the vessel, which is entirely free from corpuscles. The arterial stream is so quick that the forms of the corpuscles cannot be discerned, but in the veins both coloured corpuscles and leucocytes can be distinguished, and from the first it is noticeable that while the former are confined to the axial current, the latter show a tendency to loiter along the inner surface of the vessel, like round pebbles in a shallow but rapid stream. So far all is normal, and may

* Cohnheim, *Ueber Entzündung und Eiterung.* Virchow's *Archiv*, vol. xl. p. 27.

† The most convenient apparatus for the purpose consists of (1) A glass plate four inches long and two and a-half inches broad. (2) A common three-inch object-glass, to one side of which a glass disk four-fifths of an inch in width has been fixed with Canada balsam, in such a position that it projects by a third of its diameter beyond the edge of the object-glass near the middle. Around the adherent part of the disk there is an uncovered space of about one-tenth of an inch in width for the reception of the coil of intestine; and outside of this, an imperfect ring of cork to which the intestine may be pinned with fine needle-ends. The object-glass, with its disk, is fixed to a larger glass plate, at such a height above it that the free edge of the disk presses against the side of the frog's body, immediately below the incision, and is thus conveniently placed for the reception of the mesentery.

remain so for many hours, but in most cases changes occur in consequence of the exposure of the peritonæum, which are the beginning of inflammation.

The first abnormal phenomena observed have been already fully discussed—those of increased activity of the capillary circulation. On dilatation of the arteries of the mesentery follows a corresponding though less marked enlargement of the veins. During this stage the observer who desires to note the subsequent changes, must select for that purpose a vein of about $\frac{1}{800}''$ in diameter, the exact width of which it is desirable to measure either with the micrometer, or by marking its outline as projected on a sheet of paper with the drawing prism. For a couple of hours or more (the time varying in different animals) nothing whatever is to be observed except that although the vein gradually enlarges while the velocity of the venous current shows no abatement, the capillary circulation becomes more and more active; but sooner or later a change occurs which must be watched for with the utmost attention. This consists in a marked and almost sudden diminution of the rate of the current in the vein, in which that in the capillaries necessarily participates: it is the forerunner, and in some sense the cause, of the emigration which we desire to witness.

Simultaneously with the retardation, the leucocytes, instead of loitering here and there at the edge of the axial current, begin to crowd in numbers against the vascular wall, as was long ago described by Dr. Williams.* In this way the vein becomes lined with a continuous pavement of these bodies, which remain almost motionless, notwithstanding that the axial current sweeps by them as continuously as before, though with abated velocity. Now is the moment at which the eye must be fixed on the outer contour of the vessel, from which (to quote Professor Cohnheim's words) here and there minute colourless button-shaped elevations spring, just as if they were produced by budding out of the wall of the vessel itself. The buds increase gradually and slowly in size, until each assumes the form of a hemispherical projection, of width corresponding to that of a leucocyte. Eventually the hemisphere is converted into a pear-shaped body, the stalk end of which is still attached to the surface of the vein, while the round part projects freely.

* See Dr. Williams' Gulstonian Lectures, published in 1841, in the *Medical Gazette*.

Gradually the little mass of protoplasm removes itself further and further away, and as it does so, begins to shoot out delicate prongs of transparent protoplasm from its surface, in nowise differing in their aspect from the slender thread by which it is still moored to the vessel. Finally, the thread is severed, and the process is complete. The observer has before him an emigrant leucocyte, which in all appreciable respects resembles those which have been already described in the aqueous humour of the inflamed eye.*

The experiment I have described, even if the phenomena are not observed with that care which is necessary in order to obtain a satisfactory result, is yet very convincing. For even if one fails from want of patience to watch an individual corpuscle through the successive stages of its escape, there are other obvious facts which are too significant to be misunderstood. The accumulation of innumerable leucocytes round veins which were before entirely free, the absence in these bodies of the faintest indications of any process by which they could be supposed to be developed where they are, the obvious identity of the leucocytes outside with those inside, the pedicles by which at all stages of the process many of the corpuscles hang on to the outer surface of the vessels—all these are facts which make it impossible to admit either that the corpuscles have been formed in the situations which they occupy, or that they have migrated from any other quarter excepting from the blood-stream.

In his observation on the same process in the tongue of the frog,† Professor Cohnheim follows the method originally em-

* From the description given above, it might be inferred that the experiment is one of great simplicity, whereas in practice it is attended with very considerable difficulty; so much so, indeed, that most persons who have tried it have found failure more frequent than success. The principal sources of difficulty are, 1st, that the time occupied in the first stage of the process, during which the circulation is going on with unabated velocity, is extremely variable; 2ndly, that if, from weariness or inadvertence, the attention of the observer is diverted from the selected vein at the commencement of the process of migration, he is very unlikely to succeed in seeing what he desires to see afterwards; for, inasmuch as leucocytes are escaping simultaneously in various parts of the mesentery, they soon accumulate in such numbers that their mode of exit can no longer be distinguished. Yet, notwithstanding these difficulties, no one who has time and patience enough need fail; great care in manipulation is required, but no extraordinary dexterity.

† Cohnheim, *Ueber das Verhalten der fixen Bindegewebskörperchen bei der Entzündung*. Virchow's *Archiv*, vol. xlv. p. 333. From comparative observations made recently, I am led to recommend the tongue as decidedly a better subject for study than the mesentery.

ployed by Dr. Waller, excepting of course that the animal is curarized, and that in order to facilitate the observation the mucous membrane is partially removed. The process of migration goes on in the tongue just as in the mesentery; but (to quote once more from Cohnheim) 'with such promptitude and certainty, and if the expression may be allowed, with such elegance,' that he feels tempted to prefer the former to the latter as an object of experiment.

From these facts Cohnheim concludes 'that all such corpuscles as are formed in the first stage of an acute inflammation certainly originate from the vessels,' but admits that they do not enable us to arrive at any determination of the question whether or not pus-cells originate in other ways in the later stages (p. 350).

The bare fact of emigration when first announced took every one by surprise. Notwithstanding, it was very soon accepted by pathologists, partly because their minds were prepared by the previous discoveries of v. Recklinghausen, partly because Cohnheim's statements bore upon them the stamp of straightforwardness and accuracy. Unfortunately many of Cohnheim's adherents have not been content with receiving the fact, but, as so often happens in similar cases, have attributed to it a wider significance than that assigned to it by the discoverer himself. The passage I have quoted above affords evidence that the doctrine commonly spoken of as Cohnheim's—that pus-corpuscles originate entirely and exclusively from the blood, and that the tissues have nothing to do with their production—is in reality not his.* He evidently sees as plainly as others that, although in the commencement of every acute inflammation the first generations of pus-corpuscles may be emigrants, there is nothing in the facts which contradicts the previously accepted belief, supported as it is by so overwhelming a mass of evidence, that the later generations are the offspring of the inflamed tissues.

I have now said all that appears necessary on the subject of the migration of leucocytes. It remains to notice in few words the parallel process of exudation of liquor sanguinis. The idea

* I am aware that Prof. Cohnheim has since expressed himself much more positively than in the passage referred to. It is therefore the more important to show what impression was left upon his mind by the facts at the time he observed them.

that the escape of liquid from the blood into the inflamed parts is a main characteristic of inflammation is an old one; nor indeed is it very easy to see how it could be overlooked, for the swelling which is one of the four cardinal symptoms could not be otherwise explained. It is, therefore, not worth while to occupy space in stating evidence to show that every inflamed part becomes soaked with a liquid which is derived directly from the circulating blood. Nor is it expedient to refer to the doctrines which prevailed when the microscope was first used as an instrument of pathological research, as to the independent origin of pus-corpuscles and other cellular inflammatory products in exuded blood-plasma, excepting in so far as is necessary in order to explain that when we use the term exudation, we mean simply the *act* by which the liquor sanguinis sweats out of the vessels, not either the exuded liquid nor the structural elements which were at one time supposed to be spontaneously generated in it. The important relations of exudation with the other phenomena of inflammation will be fully considered under other headings.

Part III.—Stasis.

Another change occurs in the blood-vessels in inflammation, which, as it is subsequent as well as subordinate to those already mentioned, has not yet been adverted to. We have seen that in the mesentery as well as in the tongue of the frog, the vascular enlargement which is produced by irritation is for a certain time associated with an acceleration of the blood-stream, or at all events with no appreciable diminution of its velocity; but that, at an uncertain moment, the current begins to slacken, while the leucocytes hug the vascular wall and finally find their way out. If the part is arranged for observation in a manner conducive to the maintenance of the circulation, the retarded current may go on for a long time without any material alteration; but eventually it is apt to become slower and slower and more and more oscillating, until it ceases, in which case the condition long known as stasis is brought about. This does not, however, consist merely in arrest of the current, for it is observed that in those vessels in which stasis has occurred, the blood is not merely motionless, but much altered in its aspect. It appears as if it were made up entirely of coloured corpuscles without liquor sanguinis, and these are packed together in the choked capillaries in such a manner that their individual forms are scarcely distinguishable. The nature of this change was most carefully investigated by Professor Lister in his well-known paper 'On the Early Stages of Inflammation.'* The principal results of his inquiries are as follows:—He believes that the accumulation of the corpuscles is due to a property they themselves possess of cohering together; and that they attach themselves to each other in the inflamed vessels in exactly the same way that they cohere in rolls in ordinary blood after its removal from the body. He does not, however, suppose that this cohesiveness of the corpuscles is greater in the blood of inflamed parts than in other blood;† for he finds in the first place that vessels leading to or from areas of stasis manifest no tendency to cohesion on the part of the corpuscles; and, secondly, that blood taken from inflamed parts differs in no respect from healthy blood, as regards the mode in which its corpuscles arrange themselves on the object-glass. These facts seem plainly to indicate that the cause of the phenomena is to be looked for, not in the condition of the blood, but in that of the vessels—in other words, that the corpuscles draw to each other, not because they are themselves in an abnormal state, but because the living tissue by which they are surrounded is altered. This conclusion is rendered even more certain by the recent experiments of Dr. A. Ryneck in the Physiological Laboratory at Gratz. He has shown that all the phenomena of stasis can be produced by irritation in the webs of frogs, in which milk or defibrinated blood of mammalia has been substituted for the circulating fluid. To demonstrate this, fresh milk must be injected under a pressure of from two to three inches by a canula into the *bulbus aortæ* of a curarized frog, the *sinus venosus* having been previously opened. The milk having passed through the systemic circulation, finds its way out at the venous opening, completely displacing the natural contents of the vessels. If, then, the web is touched

* *Philosophical Transactions*, 1858, p. 645.

† Loc. cit., p. 669. 'The adhesiveness which the red corpuscles acquire in inflammatory congestion, though varying in proportion to the degree of irritation, is never greater than occurs in the blood of a healthy part when withdrawn from the body.'

with a rod moistened with ammonia, the phenomena of stasis occur in the irritated part; the capillaries become crowded with milk-globules, exhibiting the appearance of grey cords. When defibrinated blood is used, the results are even more striking, for in this case the choked vessels soon exhibit in every respect the same appearances as in ordinary inflammation.

These results seem to make it perfectly clear that the local changes which lead to the production of stasis must have their seat either in the walls of the vessels, or in the tissues which immediately surround them. To determine this more precisely, Dr. Ryneck varied this experiment by first filling the vessels with an indifferent liquid, such as solution of common salt of proper strength, so as to remove the blood; then subjecting their internal surfaces for a few moments to an agent which, by virtue of its chemical action, might be expected to modify or destroy its vitality; and finally, after replacing the injurious liquid by milk or defibrinated blood, observing the effects of local irritation. Solution of chromic acid, chloride of gold, and sulphate of copper, were found to be well adapted for thus acting on the vessels. The results were decisive. No stasis was produced by irritation in webs which had been thus treated.*

PART IV.—STRUCTURAL CHANGES IN THE CAPILLARIES.

UNTIL a few years ago, it was supposed that the capillaries take no part in normal or abnormal nutritive processes, excepting in so far as they act as passive filters through which liquor sanguinis transudes. This belief was first shaken by the discovery of Stricker that when the capillaries of the *membrana nictitans* of the frog are examined alive (i.e. when the structure is placed under the microscope in aqueous humour immediately after excision), they exhibit changes of form and size which can only be accounted for by supposing them to be contractile.†

* Ryneck, *Zur Kenntniss der Stase des Blutes in den Gefässen entzündeter Theile.* Rollett's *Untersuch. aus dem Institute für Phys. u. Histol. in Graz.* Leipzig, 1870, p. 103.

† Stricker, *Ueber die capillaren Blutgefässe in der Nickhaut des Frosches.* Sitzungsberichte der Wiener Akademie, 1865, vol. li. part ii. p. 16. *Studien über*

The activity of the life of the capillary wall has been more completely demonstrated by the further researches of the same pathologist, especially those carried out by him in combination with Leidesdorf, as to traumatic inflammation of the substance of the brain.*

In his first inquiry, published in 1866, Stricker showed that in the brain of the common fowl, when examined five or six days after mechanical injury, the vessels of the injured part exhibit changes which may be best described as consisting in budding or sprouting of the capillary wall. The structureless or hyaline substance of which the capillary appears to consist, is found to have undergone thickenings here and there of such a nature, that instead of being evenly cylindrical, it exhibits projecting irregularities or knobs. Of these knobs some retain their original form, while others grow out into branched or undivided processes, in a direction at right angles to that of the capillary, which sooner or later unite with similar outgrowths springing from other capillaries, so as to give rise to a connecting mesh-work of fibres. In the early state all these formations are beset with numerous fat granules, exactly similar to those which exist in the well-known exudation-corpuscles of inflamed brain substance; so that wherever the knobs are of a globular form, they look as if they were exudation-corpuscles, embedded in and continuous with the substance of the capillary. Very recently these observations have been repeated by Dr. Jolly of Munich,† under Professor Stricker's guidance, who has found that the alteration of the capillaries begins within a day after the injury. The first change consists in an infiltration of the capillary wall with fat granules, and has its principal seat in the neighbourhood of the nuclei. As the process advances, the granulation increases, and the alterations of form already described begin to manifest themselves.

Although a similar process has not been made out in other tissues when in a state of inflammation, there are various facts relating to the condition of the capillaries in such tissues, which are in accordance with it. Thus in the process of healing by the first intention, the formation of new vessels takes place by a

den Bau und das Leben der capillaren Blutgefässe, loc. cit. vol. lii. part ii. p. 379.

* Leidesdorf und Stricker, *Studien über die Histologie der Entzündungsherde.* Sitzungsb. der Wien. Akad. vol. lii. part ii. p. 534.

† Jolly, *Ueber traumatische Encephalitis.* Stricker's *Studien*, 1870, p. 38.

mode of budding from the old capillaries, which is very like that we have been considering. Little processes sprout out from capillary loops in the neighbourhood of the wound, which are still entire, and grow towards similar processes which spring from other loops. The two growing points, as soon as they come into contact, melt together, just in the same way as the growing ends of the marginal filaments which we studied before in the plasmodium of the myxomycetes. Thus the main difference between the process of healing and that of traumatic encephalitis lies in the circumstance that in the latter the outgrowths from the capillaries are apparently not tubular, and do not become vessels. So also in the pyogenic membranes of very small abscesses, the newly formed capillaries, although they become looped, originate by outgrowth in the same manner.*

Since the discovery of the emigration of blood leucocytes, it has often been argued that their escape from the capillaries would not be possible unless the capillary membrane were porous; and then, this being admitted, the fact that the capillaries can be filled to distension with transparent injection-masses (such as the so-called soluble prussian blue) without the slightest extravasation, has been used as a reason for regarding migration as an impossibility. There seems to me to be no doubt that if the porosity of the capillaries were a necessary inference from the fact of emigration, the objection made would be a valid one. But from the account which has been already given of the vital properties of the capillary substance, the reader will see that any such assumption would be premature. The capillary is not a dead conduit, but a tube of living protoplasm. There is therefore no difficulty whatever in understanding how the membrane may open to allow the escape of leucocytes, and close again after they have passed out; for it is one of the most striking peculiarities of contractile substance that when two parts of the same mass are separated, and again brought into contact, they melt together as if they had not been severed.†

* For a full description of this subject, see Wywodzoff, *Experimentelle Studien über die Vorgänge bei der Heilung per primam intentionem. Medizinische Jahrbücher*, 1867, p. 3.

† 'The griding sword with discontinuous wound
Passed through him, but th' ethereal substance closed
Not long divisible . . . .
. . . . for spirits that live throughout

Part V.—Summary.

We have now arrived at a point in our inquiry at which we may perhaps advantageously pause, and endeavour to bring the various parts of the process we have considered into closer relation with each other.

We have learnt that in inflammation the circulation is at first accelerated and increased, subsequently retarded and diminished, that the latter condition is attended with exudation of liquor sanguinis, emigration of leucocytes and stasis. In the study we have already made of these phenomena we have been led to believe that their origin is partly local, partly general. Thus, with respect to the leading vascular change in inflammation, viz. the acceleration of the blood stream, it has appeared to be established on satisfactory grounds that it is a consequence of an impression received by the centripetal nerves of the injured part, and reflected by the vaso-motor centre through the centrifugal nerves to the vessels; so that, although our understanding of the mechanism by which this result is brought about is as yet very imperfect, we can have little doubt that it is due to changes having their seat in the nervous system. On other grounds we have seen reason to suspect that most of the subsequent phenomena have no direct relation to the dis-

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Vital in ev'ry part, not as frail man,
In entrails, heart or head, liver or reins,
Cannot but by annihilating die;
Nor in their liquid texture mortal wound
Receive, no more than can the fluid air.
All heart they live, all head, all eye, all ear,
All intellect, all sense; and as they please,
They limb themselves; and colour, shape, or size
Assume, as likes them best, condense or rare.'

Paradise Lost, Book vi.

Since the above was written I have had the opportunity of witnessing the very admirable and ingenious experiments lately exhibited at the Royal Society by Dr. Norris. A membranous film is formed by dipping a m al ring, a foot or more in diameter and held horizontally, into a vessel containing solution of soap. It is then shown that soap-bubbles, glass rods, and other objects with wetted surfaces, can be pressed through the film without its being ruptured. The conditions of this experiment are so entirely different from those which exist in the living tissue, that I cannot regard it as affording any explanation of the passage of leucocytes through the walls of the capillaries.

turbance of the circulation as their cause, but rather to intimate changes in the properties of the living substance with which the blood comes into contact in its passage through the affected part. We shall probably best accomplish the end we have in view by assuming in the first instance that the essential phenomena of inflammation are referable either to disordered vascular innervation or to a local disturbance of the life of the inflamed part. We shall then be able to consider, with respect to each of them in succession, in how far it is referable to one or other of these proximate causes. The assumption, even if it do not turn out to be a true one, will materially help us in bringing facts into connection, and in determining their relative significance.

We have first to inquire into the causes of the slowing of the blood-stream which always succeeds the primary acceleration. Does it happen because the access of blood from the heart is retarded? or is it a combined result of the subsidence of the previous acceleration and of dilatation of the smallest vessels? The main reason for believing that it is due to diminished supply of blood from the heart, and therefore presumably to a condition of the arteries the reverse of that which leads to the previous afflux, is that in certain cases it is attended with visible narrowing of the arteries. The most positive observations on this point are those of Saviotti already referred to. He has made careful comparative experiments as to the vascular effects produced in the web of the frog by acids, alkalies, metallic salts, neutral alkaline salts, croton oil, cantharides, and other irritants; and he finds that in every case the diminution of the capillary circulation is attended with narrowing of the afferent arteries. Just as in the case of the previous dilatation, however, the relation between contraction of the capillaries and slowing of the blood-stream is not constant either as regards their degree or the time at which they occur. This want of correspondence is in itself sufficient to show that the former canot be regarded as the cause of the latter. And we are the more disposed to adopt this view when we consider that the contraction can be completely accounted for otherwise. During the primary afflux of blood the arterial dilatation extends not merely to those branches which lead directly to the inflamed area, but to those which convey blood to its immediate neighbourhood. Soon, slowing and stasis occur at the centre, the increased afflux still continuing, in consequence of which the collateral capillary channels become more and more enlarged. Eventually the arterial determination of blood subsides; less blood flows, but the capillaries still remain open, and therefore the artery which feeds them having less resistance in front, contracts to a diameter smaller than that which it originally possessed. In other words, notwithstanding the obstruction which exists at the seat of inflammation, the effect produced is not, as might be expected, dilatation, but contraction of the afferent artery, because the resistance is far more than balanced by the increased facility of circulation in the surrounding zone of congestion; so that the pressure of the blood against the inner surface of the artery, and consequently its expansion, is considerably lessened. The diminished circulation in an inflamed part is therefore not to be regarded as a consequence of the diminished afflux of blood *a tergo*; for the narrowing of the arteries is a merely secondary effect of the disturbance of the circulation. We must, therefore, in accordance with the assumption with which we started, look in the direction of the local changes for its cause.

It follows from what has been said that the slowing of the capillary circulation is merely the first stage of stasis, the beginning of the process of which stasis is the end. For if it be granted that they are both of exclusively local causation, it would be unreasonable to separate them; the more so considering that in all cases in which we have the opportunity of observation, the former is found to pass by insensible gradations into the latter. So far, therefore, as relates to the local changes in inflammation, i.e. to those which occur within the range of the immediate action of the injurious stimulus, we see that the process consists first in gradual arrest of the capillary circulation, and secondly in exudation of certain constituents of the blood. When we proceed further to inquire in what relation these two stand to each other, by comparing the circumstances under which they are actually observed, we come at once to the conclusion that they are so closely and inseparably associated that neither can be considered as consequent or antecedent, and hence that both must be dependent on the same proximate cause.

In the present state of our knowledge it is not possible to elucidate the nature of this cause completely. There are, however, certain experimental facts which enable us to approach its solution somewhat closely, and which will, on this ground,

serve as a basis for future investigation. Thus, if a ligature is tightened round the thigh of a frog, so as to arrest the circulation, and ammonia is applied to the web, the blood gathers from all sides towards the irritated part, until the capillaries within the area of irritation become choked with closely packed blood-corpuscles, and present all the appearance of stasis. If at the same time the other web is irritated in a similar manner, a comparison can be made of the effects produced. So far as the state of the capillaries is concerned, there is no difference whatever between them; the similarity becoming still more striking if the circulation is restored in the ligatured limb by removing the thread. Both webs then exhibit the ordinary results of irritation.*

In this experiment we have the process of inflammation reduced to its simplest form. Taken in combination with the observation of Dr. Ryneck, related in Part III., that neither exudation nor arrest of the capillary circulation can be produced in vessels through which certain poisonous metallic solutions have been passed, it shows that the agent in all the visible local effects is the living substance with which the blood comes into contact as it flows. Beyond this point we lose the guidance of direct observation, and must for the present content ourselves with stating that in an injured part the walls of the capillaries become so altered that the liquor sanguinis, instead of transuding from the smaller arteries in quantity just sufficient to balance the absorption, leaks abundantly from the vessels; and that in many cases this is subsequently associated with squeezing out of the leucocytes, or even of the coloured corpuscles.†

What the nature of this sudden change in the living sub-

* This experiment was first made by H. Weber in 1852 (*Experimente über die Stase an der Froschschwimmhaut.* Müller's *Archiv*, 1852, p. 361). It was repeated by Prof. Lister (loc. cit. p. 667) in 1857, and subsequently by other pathologists.

† That the change by which the capillaries become leaky has its seat in the vascular walls rather than in the adjoining tissue, is rendered probable by a recent observation of Dr. Ryneck, who has found that if an irritant is applied to the web of a frog, in which solution of common salt is circulating instead of blood, it becomes infiltrated at the seat of irritation to such an extent that a prominent tumour is formed, which eventually spreads over the whole of the division of the web acted upon. This experiment shows that exudation cannot be a consequence of increased attraction between the tissue and the circulating liquid, for in the case of salt solution such an attraction cannot be supposed to exist.

stance is, we know not, and should gain nothing by speculating. Apart from all theory the fact stands out clearly that, independently of any visible alteration either in the vessel itself or in the structural elements in its neighbourhood, the constant effect of injuring a living tissue is to divert a part of the liquor sanguinis from its natural course, and determine its soakage from the vessels which before held it, into the surrounding parenchyma.

Another question presents itself, the consideration of which cannot be left out, although it does not at present admit of any satisfactory answer—viz. that of the immediate cause of the emigration of leucocytes, and the relation of this phenomenon to exudation of liquor sanguinis. Why, as the blood-stream slackens in an inflamed part, leucocytes should separate from it and tend towards the internal surfaces of the veins and capillaries, we must admit ourselves altogether unable to explain. The fact that if blood freshly taken from the circulation of a frog or newt is received in a capillary tube or under a cover-glass, the leucocytes emigrate from the clot as soon as it is formed, and collect in numbers in the surrounding serum, affords an opportunity of watching the process under conditions much simpler than those which exist in the circulation.* As yet we are as little able to explain the one as the other. There can, I think, be little doubt that of the two stages in the process of emigration—viz. the long known loitering of leucocytes along the sides of the vessels, and the newly-discovered penetration by them of the vascular walls—the first is the essential one, and that whenever an explanation is found of the former, it will serve as a key to the comprehension of the latter.

* For a description of this process see my lecture on *Leucocytes*, already quoted.

SECTION II.—CHANGES WHICH HAVE THEIR SEAT IN THE TISSUES.

Introduction.

In the preceding section we have seen that the process of inflammation centres in the discharge of liquor sanguinis from the capillaries. We have now to consider the influence which the exuded liquid exercises on the elements of the tissues. The textural changes, although they may differ considerably according to the structure and function of the part affected, are all of such a nature as to indicate increased activity of cell life. Considering that the condition of an organ which is the seat of inflammation differs, so far as observation teaches us, from the ordinary state only in being soaked with exuded liquor sanguinis, it is natural to attribute the supervening over-growth and over-multiplication of cells to the exudation. As, however, many pathologists believe that these effects have an extremely different signification—that they are the results not of the direct stimulation of the cells, but of impressions reflected to them by an unknown nervous centre, supposed to preside over nutrition—we shall, after we have completed those anatomical descriptions which will constitute the most important part of the present section, place before the reader the grounds which exist for believing that whatever other influences may co-operate with that of changes in the nutritive medium in which the tissues are immersed, this is in itself sufficient to account for the textural germination. (See Part III.)

Part I.—Structural Changes which occur in the Connective and Supporting Tissues in Inflammation.

Under this title I include all those tissues which are not concerned in any function excepting those expressed in the definition. With reference to the present inquiry they are divisible into vascular and non-vascular. The vascular tissues include bone and the varieties of connective tissue in the strict sense; the non-vascular, cartilage, tendon and the cornea.

Non-vascular connective and supporting tissues.

In studying the process of inflammation in tissues which derive their supply of nutritive material from blood-vessels at a considerable distance, we have the great advantage of being able to separate entirely those phenomena which are proper to the tissue-elements from those which belong to the circulation. For this reason it is not surprising that the non-vascular tissues have at all times, since the earliest attempts to apply exact methods of research to pathology, been favourite fields for this investigation, and that of late years in particular, the most fruitful and at the same time decisive discussions which have taken place have related to the structural changes produced by artificial irritation in the cornea.

Inflammation of the cornea.—The reader who desires to know more of the earlier researches relating to traumatic keratitis will find the information he requires in special treatises on that subject. For the elucidation of the question which now engages our attention it appears scarcely necessary to carry our inquiries beyond the past ten years; for the better modes of investigation which have been introduced since 1863 by v. Recklinghausen and Cohnheim have given to subsequent observers so great an advantage over their predecessors, and placed the subject in a light so entirely new, that it has become necessary to begin the work afresh. The result has been to confirm the truth of the previous discoveries, and to establish the doctrine of textural pyogenesis, which was so admirably developed in the article on the process of inflammation contained in the previous edition of this work, on a more solid and extended basis.

Professor von Recklinghausen's method of examining the cornea of the frog is as follows:* The anterior chamber is first punctured so as to let out a drop of aqueous humour, which is placed on the object-glass; the cornea is then excised and placed in the drop with Descemet's membrane uppermost. The preparation thus obtained is examined without a cover-glass, in a closed chamber in which the air is saturated with

* V. Recklinghausen, loc. cit. p. 157.

moisture, so that no evaporation can take place, and consequently no alteration in the density of the liquid in which the cornea is immersed.* The healthy cornea is absolutely transparent, and when it is examined under the microscope in the manner described, no structure can be distinguished. This homogeneity, so essential to the function of the cornea, is a condition inseparable from life; if the observation is continued till the tissue begins to die, its structural elements gradually come into view—first the epithelia, then the lymphoid elements proper to the tissue, then the cornea-corpuscles. The explanation of the fact is, that whereas in life the elements of which the cornea is formed, affect light exactly in the same degree, their respective refractive powers are slightly altered in the act of dying.

If a cornea is examined in the same way, which has been irritated a quarter of an hour before by the application of a point of caustic to its surface, the conjunctival epithelial layer can at once be distinguished, along with a few leucocytes, underneath and among the epithelial elements. If an hour or two has elapsed, the proper cornea-corpuscles are visible, as dark stellate or spindle-shaped spots on a transparent ground. Of these some are homogeneous, and can be distinguished from the surrounding substance by a slight difference of shade. In others, which are finely granular, the processes or rays are subject to slight variations of contour. These amœboid movements of the rays, although very sluggish as compared with those of young protoplasm in general, are rendered much more active by subjecting the preparation to a stream of blood-serum; for which purpose Professor Stricker employs the serum of the same animal which has furnished the cornea.

In order to follow the inflammatory process in its further stages, another method of preparation has been found by Stricker to be advantageous. The cornea is immersed for a few minutes in a weak solution of chloride of gold (½ per cent). It is then washed with water slightly acidulated with acetic acid, and exposed to daylight. When the frog's cornea is examined in this way at various periods after irritation, the

* The following statement is founded on the admirable research of Mr. W. F. Norris, conducted in the Vienna laboratory under Stricker's guidance. Norris and Stricker, *Versuche über Hornhaut-Entzündung.* Stricker's *Studien*, 1870, p. 1. I retain the terms employed by the authors, although in the light of more recent anatomical discoveries they are open to criticism.

progress of the changes of which the beginning has been already sketched, may be studied with great advantage. In the normal cornea, when so treated, the stellate corpuscles with their nuclei can be very distinctly seen. The latter are irregularly defined, of large size in relation to the protoplasmic mass in which they are embedded, and contain one or two nucleoli. In the stellate masses themselves the caudate processes or rays are the most striking and obvious features. In a cornea excised three hours after irritation, some of the corpuscles exhibit no change excepting that their outlines are more strongly marked; in others there seem to be, in addition to the irregular nucleus above referred to, one or more spheroidal bodies which are embedded in some other part of the corpuscle. This appearance affords

Fig. 400.

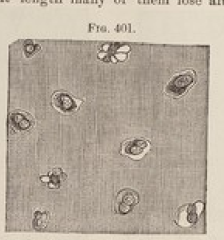
Cornea of frog excised three hours after irritation.

the earliest sign that the process which has been hitherto called 'proliferation' is beginning, that is to say that the mode of life of the protoplasmic mass is changing from the normal quiescent state which fits it to take part in a permanent tissue, to the state of reproductive or germinating activity—that new bodies are being formed within the body of the parent mass, to which such terms as 'germs' or 'offspring' are applicable. A part of the original living substance of the element begins a new life, much more active than it before possessed, and a new organic development. Since the introduction of the method of observing structural changes in living tissues, pathologists have learnt that it is a constant characteristic of the change we are considering, that the rejuvenescent part or

substance acquires the property of contractility. In other words, that all protoplasm when assuming new life, and beginning new organic development, is endowed with the faculty of amœboid movement. Of the words which have been employed to denote this change, viz. rejuvenescence, proliferation, germination, the last appears preferable as being the least technical and the most expressive.

We return to the further steps of the germinative process in the inflamed cornea. Between the fifth and twelfth hours after irritation the cornea-corpuscles become more and more distinct and granular, while their processes become thicker and shorter, until at length many of them lose altogether their

Fig. 401.

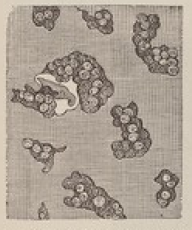


Altered corpuscles of cornea excised eight hours after irritation.

characteristic stellate, or caudate, outline, and are converted into irregular clumps. If the cornea is examined in this stage, after treatment with chloride of gold, it is seen that in those parts in which the structural changes are most advanced the normal character of the tissue is entirely lost. The beautiful network produced by the interlacing of the normal corpuscles is no longer visible; in place of it the field is scattered over with clumps of irregular form, in some of which the caudæ are represented by rounded knobs, while in others the outline is almost spheroidal. Most of these bodies are so granular that their contents cannot be distinguished, but in others the newly formed germs are plainly visible. The number of these germs varies according to the stage of irritation, so that in the same cornea, clumps containing a numerous offspring may be seen in one part, while in others the germination is only beginning.

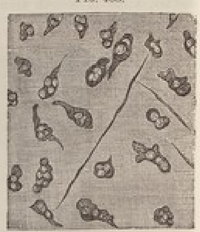
That the interpretation suggested by these appearances is the true one, that the clumps containing numerous round corpuscles are really of the nature of mother-cells, the observer can best assure himself by returning to the method of examination first described, that is to say by placing the inflamed tissue under the microscope alive, at the same time stimulating the elements in question to increased amœboid movement by irrigation with serum. It is then seen that the germs change their relative position with the movements of the mass of protoplasm in which they are enclosed, just in the same way

Fig. 402.



Cornea sixteen hours after irritation. Amœboid masses containing numerous newly formed elements.

Fig. 403.



Cornea about twenty-four hours after the insertion of a fine ligature. Masses containing young elements in the neighbourhood of the thread.

as the granules and ingesta do in the body of an amœba, rolling one over another in such a manner as would not be possible if they were not really contained in the mass.

Hitherto nothing has been said of pus-corpuscles, that is leucocytes, because in the stages of keratitis we have been considering they bear no part. The cornea has lost its perfect transparency, but is not as yet turbid; the few leucocytes which are met with are those which are indigenous in the tissue. There is as yet no suppuration; to arrive at an opinion as to how the formation of pus commences we must follow Professor Stricker a stage further. The difficulty of the observation lies in the rapidity with which the process takes place; for from the moment that the cornea begins to be clouded, its

tissue is so beset with growing and multiplying elements that it is difficult to distinguish them, so that the only way by which a conclusion can be arrived at is by combining the results of a number of observations made on different corneas at the stage of commencing opacity.* From such observations it may be learnt, first that the opacity is due to the presence of leucocytes which are so numerous that it is out of the question to regard them as the offspring of those which are indigenous in the tissue; and secondly, that among them there are many of the original branched or stellate corpuscles, some of which are only slightly changed in outline or aspect. But in addition to these more or less normal corpuscles, the masses above described, containing groups of germs, also occur in great numbers, while among them, and in the very parts of the cornea where they are most numerous, the leucocytes are most crowded and there are fewest fixed corpuscles. If these are facts it can scarcely be doubted that there is a relation between the metamorphosis of the stellate corpuscles and the formation of leucocytes; in short, that the little spheroids which are contained in the amœba-like masses are young pus-corpuscles, and that even in the cornea suppuration must be regarded as at all events in part, a process of germination.

We have next to consider to what extent emigration is also concerned in traumatic keratitis. According to Professor Cohnheim, it is the only way in which the pus-corpuscles are produced, this conclusion being mainly founded on negative observations. He has failed to observe the germination process we have been describing, and does not believe in it. He finds that in every form of traumatic keratitis, whatever be the nature or mode of application of the irritant, the stellate corpuscles remain absolutely unaltered, both as regards their position and arrangement, and their structure, and that the leucocytes to which the opacity is due are neither enclosed in other elements nor show any indication of being in different stages of development; whence he naturally concludes that they cannot have originated where they are found, and must have been introduced from outside. He has further observed, and

* The reader who is acquainted with Professor Cohnheim's researches on keratitis will notice that they commence at the point to which we have now reached, so that it is not difficult to understand that, although he employed the same modes of examination, he failed to observe the structural changes which have been described in the preceding paragraphs.

the fact has been confirmed by many other pathologists, that if keratitis is produced in a frog whose blood-leucocytes have been charged or fed with aniline or vermilion by injecting either of these pigments in a state of extremely fine division into the circulation, corpuscles can be distinguished in the inflamed cornea, which from their being pigmented must necessarily have migrated from the blood. In some cases the marginal part of the cornea, that part which of course is nearest to the vessels, is so full of immigrant leucocytes that the blue or red coloration can be distinguished even with the naked eye. These facts of course afford conclusive evidence of migration, but they contain no disproof of tissue-germination, for even Professor Cohnheim himself admits that in all stages of the process there are many leucocytes which are not coloured.

The only positive argument against germination is that founded on the remarkable experiment known as that of the 'salt frog.' This experiment consists in slowly injecting a weak solution of common salt by the abdominal vein, until blood ceases to flow from its peripheral end which is left open, so that the whole of the natural circulating fluid is replaced by the saline solution. In this condition the animal may be kept alive for several days. If then, immediately after the injection, the cornea is irritated, no opacity is produced, because, according to Cohnheim, the blood from which they spring is absent. During last summer this experiment was carefully investigated in the Vienna Pathological Laboratory. It was found that even if the injection is continued, according to Cohnheim's direction, until the liquid which issues from the open end of the vein appears colourless, the minute capillaries as seen in the mesentery still contain liquid which is rich in leucocytes, so that whatever be the explanation of the want of opacity of the cornea, it is not due to the absence of these structures from the circulation; it is much more reasonable and natural to suppose that the result of the experiment is to be accounted for by the peculiarly abnormal condition of the animal.

Inflammation of cartilage.—The changes which occur in the permanent structural elements in consequence of irritation, are much more easily studied in cartilage than in the cornea, on account of the facility with which this tissue can be prepared for microscopical examination; it has therefore from the commencement of the inquiry afforded to the pathological observer the most accessible evidence in support of the belief that such

structural changes form an essential part of the process of inflammation; and particularly as regards the question of the origin of pus, the obvious difficulty of supposing that the young elements which occupy its cavities when it is inflamed have penetrated into those cavities from some other quarter, has made the case of cartilage the strongest that can be cited against any exclusive doctrine of emigration. For this very reason it is unnecessary to devote much space to the discussion of inflammation in cartilage, for it is certain that if we can show that germination is the rule in every other tissue, no one will suppose that cartilage is an exception. The normal cartilage cell, like every other active cell, is a mass of protoplasm containing a nucleus. As in the case of the cornea, each mass is enclosed in a cavity of similar form to itself, which is hollowed out in the interstitial substance; the difference being that whereas in the cornea the cavities communicate with each other by the innumerable tubular prolongations which correspond to the rays of the stellate cells, in cartilage they have no such prolongations and are entirely closed. When cartilage is irritated, as for example by scraping its surface, the cells in the neighbourhood of the irritation enlarge and consequently expand their capsules. The protoplasm of which each cell consists becomes more granular, and soon it is found that the mass contains two corpuscles in its interior instead of one, and that each has a gathering of protoplasmic matter around itself. This process of division is repeated in each segment until every cavity contains a mass of nucleated cells, which at length assume characters corresponding with those of newly formed pus-corpuscles, while at the same time the original interstitial substance gradually wastes away, and is finally represented by a sponge-like stroma, in the holes of which the groups of young cells are contained. In this process we have a typical example of germination; the permanent cells which have for their function the maintenance of the unchanging life of the tissue, are replaced by a more numerous progeny of transitory mobile cells—i.e. leucocytes—which live at the expense of what remains of the tissue and eventually destroy it.

Inflammation of tendon.—As a non-vascular tissue, extremely rich in cellular elements, tendon has almost as great advantages as cartilage for the study of the inflammatory changes which such elements undergo at a distance from the vessels. The splits (*Henle'sche Spalten*) which exist in tendon between the parallel bundles, are lined with chains of staff-shaped, nucleated cells, each of which is in contact with its neighbours by its ends, and consists of a cylinder of protoplasm, including a nucleus. The changes which these cells undergo have been recently very completely studied in rats and guinea-pigs by Dr. Güterbock, who finds that by a kind of cleavage of the nucleus that body assumes a botryoidal or necklace form, to which the scanty covering of protoplasm models itself. Eventually new cells (young pus-corpuscles) are formed by the complete division of the nucleus. Here, as in other cases, the participation of the elements of the tissue in the germinative process, can only be judged of at the very beginning, i.e. about eight hours after the injury. At a later period it is so difficult to distinguish the results of similar changes in the connective tissue from those proper to the tendon itself, that no conclusion can be arrived at.*

Vascular connective tissue.

The most positive information we possess as to the nature of the inflammatory changes in vascular connective tissue is derived from the examination of the process in the frog's tongue, for there is no other organ in which the tissue in question can be placed under the microscope under conditions so completely normal. The mode of experiment has been already referred to.

The textural changes have been lately described with great minuteness by Professor Cohnheim,† and still more recently his description has been critically examined by Professor Stricker.‡ The curarized frog is conveniently placed on its back, the tongue being extended by a ligature attached to each of its two tips, and the ligatures so fixed that the organ can be set free and replaced in the mouth at the end of each period of observation. The tongue can thus be placed readily under the microscope, with its papillary surface upwards. As, however, the submucous tissues could not be well seen through the mucous membrane, it is desirable to strip this membrane off, over a small extent of surface, an operation which can be effected with scarcely any bleeding, and has the additional advantage that it affords a con-

* Güterbock, *Untersuchungen über Sehnenentzündung.* Stricker's *Jahrbücher*, i. p. 22.

† Cohnheim, loc. cit. p. 344.

‡ Stricker, *Ueber die Zelltheilung in entzündeten Geweben*, loc. cit. p. 18.

venient and practical method of irritating the parts to be examined. If these preliminary arrangements have been successfully carried out, and sufficiently high powers are employed in the examination, it is seen that in the meshes between the capillaries of the inter-muscular spaces there are bodies of the most varied form and slightly turbid appearance—the so-called connective-tissue-corpuscles. According to Cohnheim these

Fig. 404.

Connective tissue corpuscles of the tongue of the frog.

bodies take no part whatever in the inflammatory process, the steps of which, as he observed it, correspond to those we have already described when speaking of inflammation in the mesentery. To determine this question, of such importance in its general bearing on inflammation of connective tissues, Professor Stricker has subjected these corpuscles to the closest scrutiny. He finds that while some of them are oblong or fusiform, others are of the extremely irregular form figured by Cohnheim, and that corpuscles of the latter class may be watched for hours (in one instance in the same individual for ten hours continuously) without changing their place. But he does not admit that they are motionless; on the contrary, he states that they undergo changes of form of so marked a character, that there can be no mistake about their existence. Thus they swell at one part, shrink in others, sometimes budding out into processes, which are again retracted; at others assuming forms which seem to indicate that they are on the point of dividing. Yet notwithstanding the most careful and patient observations, Stricker has not succeeded in seeing a single act of division completed. In the oblong corpuscles the amœboid changes are less active, and limited to the extremities. Sometimes it was observed that the tip gathered itself up as if it were just about to separate from the rest, but again subsided into its original condition.

In several of Stricker's experiments the process of emigration was going on with great activity, and the tissue becoming fuller and fuller of leucocytes during the whole period that he was engaged in observing the phenomena above described in the connective-tissue-corpuscles. It therefore appears perfectly clear that, in the particular case of the tongue of the frog, these corpuscles have at first nothing to do with the process of suppuration. On the other hand it seems highly probable that if it had been possible to pursue the investigation to a later stage, the changes of form which he describes would have resulted in actual division. The lesson to be learned here, as in other cases, seems to be, that although in acute and rapid suppurations the leucocytes are mostly if not all emigrants, there is reason to believe that at later periods other modes of pyogenesis come into operation. At the same time it must be borne in mind, that in the present state of our knowledge this is rather a matter of inference than of observation. The grounds for believing it are in the first place the facts we have already considered with reference to the cornea, and secondly the structural alterations which are met with in examining tissues in the more advanced stages, and less acute forms of inflammation—all those anatomical facts, in short, which formed the original groundwork of the doctrine hitherto taught of the textural origin of pus; with reference to which many of Cohnheim's followers seem to have forgotten that they are quite as true and quite as significant as ever. For in every limited inflammation of the subcutaneous tissue, and in the neighbourhood of every subcutaneous abscess, a region is found outside of the focus of suppuration, in which the connective-tissue-corpuscles present alterations which are so distinct, that it is impossible for any one who is conversant with them to doubt that they signify that the tissue is germinating.*

* The experiment of M. Lortet, already referred to (p. 750), affords the pathological student the opportunity of satisfying himself, by a single observation, that in traumatic inflammation of the subcutaneous cellular tissue, pus is formed both by tissue-germination and emigration. The swimming-bladder of a fish, previously filled with solution of common salt, is inserted beneath the skin of a rabbit. After thirty-six hours the animal is killed, and the lesions are investigated. It is then seen that while the bladder is full of corpuscles which can only have migrated from the blood vessels, there is abundant evidence of the commencement of germinative pyogenesis in the surrounding texture.

Inflammation of muscle.—There is no vascular tissue in which the phenomena of germination can be more satifactorily studied than in muscle. The process was first examined by Waldeyer, and subsequently by Otto Weber (Fig. 405). Still more recently

FIG. 405.

Cross section of human muscle from the neighbourhood of a wound. The dark masses represent the remains of muscular bundles. In other parts of the section the sheaths are filled with young elements, which have displaced the muscular substance. The interstitial connective tissue is also in a state of germination. (After Otto Weber).

it has been made the subject of an extended series of experiments by Dr. Janovitsch Tschainski,* under the direction of Professor Stricker. In traumatic inflammation of muscle, the fixed corpuscles of the fibre-sheaths undergo alterations which resemble those we have already noticed in connective tissue, and, as in the other case, they are much better seen in parts at a little distance from the seat of injury than in its immediate neighbourhood. Thus, in experiments in which muscle was cauterised, Dr. Janovitsch found that the inflammatory changes could be studied most advantageously in the

FIG. 406.

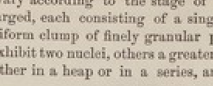
Multiplication of nuclei in the sheath of an inflamed muscular fibre.

outer zone of redness and swelling. In this situation the muscular substance, when examined twenty-four hours after irritation, is found to be for the most part unaltered, the transverse striæ

* *Ueber die entzündlichen Veränderungen der Muskelfasern.* Stricker's *Studien*, p. 86.

being well marked and of natural appearance. The aspects of the corpuscles vary according to the stage of change. Some are merely enlarged, each consisting of a single nucleus embedded in a fusiform clump of finely granular protoplasm. Of the rest some exhibit two nuclei, others a greater number, which are arranged either in a heap or in a series, and are generally

Fig. 407.



Empty sheath beset with young elements.

so close to each other that their opposed surfaces are flattened, the whole being held together and surrounded by the protoplasm already mentioned. In the later stages the young elements multiply to such an extent that they eventually occupy the whole of the sheath, the natural contents having gradually disappeared.

Part II.—Structural Changes which occur in the Epithelial and Glandular Tissues in Inflammation.

Epithelial tissues.—The appearances observed in suppurating mucous membranes have always been regarded as affording, next to those in the cornea and in cartilage, the strongest evidence of the textural origin of pus. For in a great many kinds of catarrh, large cells have been met with in the purulent liquid which is thrown off at the very commencement of the process, which contain groups of bodies entirely resembling young pus-corpuscles. These remarkable epithelial elements were considered by Buhl,* who first described and studied them, as mother-cells or brood-cells; and most pathologists since have regarded them in the same light. But more recently, since the discovery of emigration has induced a tendency in the minds of some persons to conform all the details of the inflammatory process to one type, Steudener and Volkmann † have maintained that the bodies in question are not the off-

* Buhl, Virchow's *Archiv*, vol. xvi. † *Centralblatt*, 1868, No. 17.

spring of the cells in which they are enclosed, but strangers which have intruded themselves from without. Here, as in so many other cases, the only way of solving the question was, if possible, to observe the phenomena in the living tissue, i.e. to see the process of intrusion or extrusion actually going on under the microscope. This, however, was evidently a matter of great difficulty. In Professor Billroth's * admirable essay on inflammation the reader will find an account of a number of efforts made by him for the purpose without satisfactory results. The question seems, however, to have been now settled in favour of the original doctrine of Buhl, by the very recent researches of Dr. Oser † in the Vienna laboratory.

Although in general epithelial structures derive their nourishment directly from the blood, there are some which are entirely remote from vessels. Of these the epithelium covering the cornea and that of the epiploa are the best examples. If the normal epiploon ‡ of the rabbit or guinea-pig be treated with weak solution of nitrate of silver, and then exposed to the light and examined without further preparation, it is seen that in the most delicate parts it consists merely of a network of hyaline fibres of connective tissue paved on both sides with flat epithelia; and that in the centre of most of them a little mass of protoplasm can be distinguished. We have here, therefore, an epithelial structure of the simplest kind which is entirely out of relation with the capillaries, and is thus remarkably well fitted for studying the independent behaviour of epithelial elements in serous inflammation. If a little iodine or solution of nitrate of silver be injected into the peritonæum, and the omentum be examined twenty-four hours after, it is found that the fibres of the network are no longer covered with a continuous pavement, but that a number of elements hang about it, most of which differ considerably from the original epithelia, though some still resemble them. The most striking difference is that of the increased size of the protoplasmic mass. Instead of a faintly granular body, scarcely so large as a leucocyte, you have a clump twice or three times as large, which if examined under the proper conditions displays

* Billroth, *Mancherlei über die morphologischen Vorgänge bei der Entzündung. Medizinische Jahrbücher*, vol. iv. 1869, p. 1.

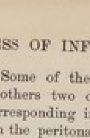
† Oser, *Ueber endogene Bildung von Eiterkörperchen an der Conjunctiva des Kaninchens.* Stricker's *Studien*, p. 74.

‡ Cornil et Ranvier, *Manuel d'Histologie pathologique.* Paris, 1869, p. 74.

amœboid movements. Some of these rapidly growing cells contain single nuclei, others two or a greater number of spheroidal corpuscles corresponding in form and size to those which either float free in the peritonæal liquid, or are enclosed in the coagulum of reticular fibrine with which the affected surface is more or less covered.

In this case it is at all events certain that some of the changes observed have nothing to do with migration, for the membrane in which they occur is non-vascular. But it is obviously only a matter of inference, that we have before us an actual formation of pus by epithelial germination. We shall find, however, additional ground for believing that this interpretation is the true one, by comparing the account given above with the very exact observations of Dr. Oser on the conjunctiva already referred to. In his experiments the

Fig. 408.



An epithelial element from the anterior surface of the cornea of a rabbit, which had been irritated two days before with weak ammonia. *a a*, Nuclei. One of the five young elements contained in the central cavity is in the act of escaping through the wall of the cell.

membrane in question was examined at periods varying from a few hours to four or five days after irritation with solution of ammonia of various strengths. The first stage of the process consists in the growth of the protoplasmic or living part of the epithelial element, and the consequent disappearance of the external investment; the second in the condensation of the granular material at one, two, or a greater number of points, into little spheroidal corpuscles, which as they become more distinct appear to detach themselves gradually from the remaining granular matter, until eventually they all lie free in one cavity. Finally the spheroids show themselves to be leucocytes by their amœboid movements; and, on one fortunate occasion, were seen by Dr. Oser making their way out of the mother-cell and then moving about in the surrounding liquid.

In both the cases referred to, it is to be borne in mind that the fact of germinative pyogenesis does not exclude that of migration. Indeed, as regards the serous membranes, there is the strongest reason for believing, that in certain forms of acute peritonitis the leucocytes contained in the peritonæal liquid are all emigrants. Thus in the peritonitis which is produced in the frog's mesentery by exposure, it is quite impossible to suppose that the dense layer of corpuscles which, in the advanced stage of the experiment, covers the surface of the membrane, can be derived from any other source than the circulating blood. Again, the anatomical appearances which present themselves in the most acute forms of suppurative peritonitis with which we are acquainted clinically, even though we may be disposed to admit that they might be brought into harmony with an opposite theory, can be explained much more naturally and easily in the same way.

Glandular tissues.—The question next to be considered is that of inflammation of glands. There is no glandular organ in which traumatic inflammation has been so completely studied as in the liver; some observers finding, in the anatomical changes which result from experimental irritation, proof of the dependence of these changes on migration, while others, particularly Holm, believe that the liver-cells undergo transformation into inflammatory products, and that pus-corpuscles may be produced in hepatic mother-cells. Dr. Hüttenbrenner has recently repeated the experiments of Holm,* as well as those of Koster,† (who may be regarded as the most important expositor of Cohnheim's doctrine in its relation to inflammation of glands), and has confirmed the results obtained by both of his predecessors. He concludes that in the liver, if the irritant is such as to produce alterations which are confined to the immediate neighbourhood of the injury, the liver-cells germinate, and believes that the few pus-corpuscles which are formed under these circumstances originate endogenously. If, however, an abundant suppuration is produced, as e.g. by the injection of ammonia, it is found that the pus corpuscles are collected round the blood-vessels in a manner which certainly indicates that they are emigrants, or that the capillaries are concerned in their production. In support of the same view

* Holm, *Experimentelle Untersuchungen über die traumatische Leberentzündung. Wiener Sitzungsb.* vol. lv part II. p. 439.

† Koster, *Entzündung und Eiterung in der Leber.* *Centralblatt*, 1868, p. 17.

we may refer to the observations of Billroth * on mastitis and orchitis. In a patient who died in puerperal fever with minute abscesses disseminated throughout the mammary gland, Billroth found that the young pus-cells occupied the inter-acinar vascular network just in the same way as Koster described in the liver. Again, in an inflamed testicle taken from a patient who had died of secondary pyelitis consequent upon stricture, and had often before been treated for gonorrhœal orchitis, the connective tissue between the seminal canals was the seat of interstitial irritation, and beset with an infinite number of young cells, the glandular structures remaining themselves unaltered. In both of these cases the accumulation of leucocytes round the vessels is certainly remarkable. But before we agree to the explanation offered by Billroth we must take other considerations into account. It is to be borne in mind, that in both instances the inflammation was of a secondary, that is to say infective, character. In all inflammations of this class it appears quite as reasonable to suppose that the limitation of the morbid changes to the immediate neighbourhood of the blood-vessels is due to the fact that the infective agent is introduced into the tissue by the blood-stream, as to attribute it to emigration of leucocytes.

For the present this question must remain open. All that we are justified in concluding is, that although even gland-cells under certain circumstances may be alienated from their natural secreting function, and excited into reproductive activity, this germination does not play any important part in the formation of pus.

Part III.—Influence exercised by the Form and Mode of Action of the injurious Agent on the Character of the resulting textural Changes.

Although if we be careful to distinguish what is essential to the process of inflammation, viz. the altered state of the vessels, from the phenomena which accompany it, and the textural germination which it produces, its characters will appear to us to present very slight variation, yet the visible results by which

* Billroth, loc. cit. p. 30.

it manifests itself differ widely in different cases. It is therefore necessary, in order to complete the present subject, to consider in what degree these differences correspond to differences in the causes which produce them.

Vesication.—If a hot iron is applied to the skin at a sufficient temperature, it at once destroys its vitality. If the temperature be a little below that which is necessary to produce this result, the blood contained in the vessels coagulates, and the tissue eventually dies. At a still lower temperature the skin retains its vitality, but blisters are formed at or around the injured part.

If the mesentery of a guinea-pig is touched with a heated surface, and the effect observed under the microscope, it is found that stasis is produced which is co-extensive with the surface of contact. It is tolerably certain that in like manner, in vesication of the skin by heat, the circulation of the heated part is abruptly brought to a standstill. As, outside of the area of stagnation, it goes on at first with unabated then with increased vigour, while the walls of the capillaries are probably acted upon by the heat in such a manner as to render them more permeable, we can readily understand how it happens that liquor sanguinis is exuded more rapidly and more abundantly than in ordinary inflammations. From the researches of Dr. Samuel of Königsberg it seems probable that the effects of liquid vesicants agree with those of heat in all the respects which have been referred to; so that the peculiarity of the mode of action of vesicant agents in general, would seem to lie in its suddenness and in the faculty which they possess of at once producing those changes in the capillary wall which in ordinary inflammation require a longer time and a more gradual process for their production. In this way the exudation of liquor sanguinis, instead of being deferred until the slowing of the circulation has commenced, begins immediately, and, favoured by the primary arterial afflux, and the increased intra-vascular pressure consequent on the sudden capillary obstruction, is so abundant that the liquid collects in blisters.

Relation between inflammation and the reparative process.—When the local injury is so intense as to destroy the vitality of the affected part at once, that part becomes surrounded with a zone of inflamed tissue from which it eventually separates, leaving behind it a granulating surface. To understand this process of demarcation and separation, it is in the first place to be borne in mind that the exuded liquid contains the fibrine-producing elements of the blood, and that contact with dead substance at once determines coagulation of all such fibrinogenous liquids. Accordingly, the first step in the process of reparative separation is the formation, in contact with the dead part, of a more or less solid stratum of fibrine, in which stratum the production of new capillaries and granulation-tissue commences.

What is this granulation-tissue? It consists entirely of young cells, which if they agree with leucocytes more or less in size, differ from them both in structure and arrangement. The neoplastic granulation- or embryonal-cell (as it is often termed) is a mass of protoplasm with a round or oval well-defined central nucleus. It exhibits very slight amœboid movements, and has a marked tendency to endogenous multiplication by division of its nucleus—a process which goes on with such activity that in carefully prepared sections of young granulations it may be studied in all its stages under the microscope with great facility. The arrangement of granulation-cells is determined by that of the newly formed capillary vessels around which they are grouped. At first irregular, it becomes more and more definite as the new growth is transformed into cicatrix, or into that adenoid texture which is the material of chronic inflammatory induration. It is scarcely necessary to add that the process we have been describing and that of healing by the first intention are essentially the same.

Suppuration.—Before endeavouring to explain how it is that leucocytes, after escaping from the vessels, tend to collect together in groups so as to form foci of suppuration, i.e. abscesses, I would refer to two of the vital endowments which they possess when in the active, that is amœboid state, as perhaps having an important bearing on the question; viz. the power of surrounding concrete matter with which they come into contact with their own substance, and secondly that unexplained tendency which they possess to escape from the blood-current, and to move away from it in a direction at right angles to the axis of the vessel from which they have escaped.

When a bit of fresh cellular tissue is inserted under the skin of a living animal, and allowed to remain there for several days, it becomes soaked with a liquid teeming with living amœboid leucocytes, all of which possess the ingestive faculty just referred to. It has not as yet been experimentally demonstrated that these leucocytes actually prey upon the nutritive constituents of the slough, but it certainly appears as if they determined its liquefaction.

Again, when an abscess is produced by embedding a thread steeped in an irritant liquid in some tissue, leucocytes collect in numbers around the foreign body, which soon floats loose in a collection of pus. As the cavity is considerably larger than the irritant, there must have been destruction of the natural tissue. It is surrounded by a zone of reproductive inflammation (pyogenic membrane), in which the neoplastic process already described is going on in full vigour.

In both of these instances the abundant genesis of leucocytes at and around the lesion, which gives rise to the formation of a suppurative focus, manifests itself in absorption or liquefaction of the original tissue. The two conditions stand to each other in a relation so close that we may venture to infer that the latter is a consequence of the former.

The growth of an abscess once formed is explicable on the same grounds as migration in general. Whatever cause determines the rapid filling of a bladder full of liquid inserted under the skin (p. 776), will also account for their accumulation in the cavity of an abscess, independently of any special action of its lining. With reference to this point, however, there is much probability in the supposition that the newly formed and dilated vessels of the so-called pyogenic membrane, favour by their structure and arrangement the extrusion of leucocytes.

Why one inflammation is suppurative and another not is a question we are unable to answer, excepting in so far as an answer is contained in the statement that on the whole those inflammations which are most intense and concentrated, provided that the injury done falls short of the production of instant stasis or necrosis, are most suppurative. In other words, so long as blood freely circulates, the quantity of pus produced in an injured part varies according to the intensity of the lesion.

The existence in leucocytes of a power of absorbing tissues with which they are brought into contact is probably the explanation of the destructive tendency which is so important a character of all intense inflammations.* The absorption and liquefaction of the original texture is as peculiar to and inseparable from the process of inflammation as the germinative

* On this subject see ADDENDUM, p. 789.

changes which we have been describing. That it is analogous to ordinary absorption cannot as yet be stated, for we do not yet know whether the wandering leucocytes which are found in healthy connective tissues have to do with that process or not. The only other kind of liquefaction which it could be compared with is the putrefactive, but with this it has not the slightest analogy. For in no single particular, excepting that both result in disintegration, do they resemble each other. Suppurating tissues, so long as they are protected from the influence of external media, do not show the slightest tendency to septic decomposition.

Part IV.—Direct Influence of abundant Supply and frequent Change of Nutritive Liquid in stimulating Cell Life.

It is well known that if a portion of living structure is removed from its natural position and inserted or engrafted into some other part of the same or of another animal, in such a manner as to be in complete contact with living vascular tissue, the ordinary nutritive changes may go on in the engrafted fragment independently of the direct influence of the nervous system. Hence it may be inferred that if an adequate supply of normal nutritive fluid is the only condition which is necessary to determine the continuance of the ordinary nutritive changes, it is by no means improbable that the modification of this process which goes on in inflammation, may be determined in a corresponding manner by subjecting the tissue to the action of such a fluid as is discharged from congested vessels. With some such considerations as these in view, Stricker * devised an experiment consisting essentially in the insertion of a fragment of living tissue into a cavity of which the walls are in a state of active inflammation. A somewhat similar experiment had already been made by v. Recklinghausen in 1863. He introduced the cornea of a frog, immediately after excising it, into a lymph-sac of the same animal, and observed that if the cornea were left in this situation long enough for the cavity to

* Stricker, *Ueber die Beziehungen von Gefässen und Nerven zu dem Entzün-ungsprocesse*, loc. cit. p. 31.

inflame and suppurate, the marginal part of it became charged with leucocytes, which, by virtue of their amœboid movement, penetrated in vast numbers into its tissue. But as v. Recklinghausen had not inserted his healthy cornea into a cavity already inflamed, and moreover had not observed the structural changes which took place, his experiment was not available for the solution of the question. The method adopted by Stricker is as follows:—He irritates one eye of a frog by cauterizing the cornea through, then excises the cornea of the opposite eye, and inserts it beneath the *membrana nictitans* of the irritated eye, finally uniting the edge of that membrane with the opposite margin of the cutis by ligatures. After twenty-four hours the transplanted cornea is removed and examined, and is found to exhibit inflammatory changes, which although they are on the whole less advanced than those found in an unexcised cornea, at the same period after irritation, are equally characteristic. In different experiments there were differences both in the degree in which the cornea-corpuscles were altered, and still more in the number of pus-corpuscles, but in all the appearances corresponded with the description which has been already given of the effects of direct irritation.

These results scarcely admit of misinterpretation; they are, however, rendered much more decisive and satisfactory by varying the conditions of experiment in such a way as to show that the changes observed are not due to the penetration of leucocytes from the liquid in which the cornea is immersed, and secondly that they are not a mere result of its transplantation into an unnatural position. The first of these objects is readily attained by dividing the cornea immediately after excision, plunging one half in water so as to kill it instantly, and then placing the dead and the living portion together, underneath the *membrana nictitans* of the opposite eye. It is then found that whereas the same inflammatory changes as before go on in the living half, the other half remains inactive. The second result is attained by the observation of what happens when, instead of first cauterizing the eye which is destined to be the recipient of the transplanted cornea, it is left uninjured. At the end of twenty-four hours the cornea-corpuscles are found quite unaltered, and so distinct that the plan is strongly recommended as a method of demonstrating their normal characters.

These varied results seem therefore to show, beyond the possibility of dispute, that the structural changes in the cornea of the frog cannot be dependent either on any influence exercised by the nervous system, or by transmission of the irritative effect from one structural element to another, so that we have good ground for concluding with Professor Stricker that they result exclusively from the stimulating influence of the exuded liquid. The precise physical or chemical conditions are as yet unknown, and are at the present moment subjects of further investigation.

CONCLUSIONS.

1. In every inflammation which attains its full development the changes which manifest themselves in the inflamed part are of three kinds, distinguished from each other according to the organs which are concerned in their production. They are either (1) effects of disorder of the vascular nerves and centre; (2) effects of alteration of the properties of the living walls of the capillaries; or (3) effects of the stimulation of the living cells by transudation of liquor sanguinis.

2. Of these three orders of phenomena the second only can be regarded as absolutely essential to the existence of inflammation, which may, therefore, in the strictest sense, be said to have its seat in and about the veins and capillaries, it being there that the earliest and most constant effects of irritation or injury manifest themselves.

3. The nervous and vascular effects of local irritation cannot be directly described as successive stages of one process; for the determination of blood to the seat of injury which is the sole result, and, if I may so speak, purpose of the vasomotor disturbance, has no relation to the local vascular changes, excepting in so far as it tends to make the exudation more abundant. Exudation of liquor sanguinis, although favoured by increased arterial afflux, may occur without it, and as a rule continues after the afflux has ceased. The vascular and textural changes, on the contrary, may be regarded as successive stages of one process, for they are connected by a causal relation—the exudation of liquor sanguinis, in which the former ends, being the determining cause of the latter.

4. The mode in which an injury changes the living substance of the vascular walls so as to make them permeable to the blood is unknown. The nature of the change itself is also unknown, the only clue which we have to its character being that afforded by the structural alterations to which it leads in certain organs, and particularly by those which are observed when the process of reparation, attended with the formation of new capillaries, is commencing. (See pp. 757 and 778.) From these appearances we are led to infer that the primary change consists in the transition of the material from the formed to the plastic condition; from a state in which it is resistant, because inactive, to one in which it is more living and therefore more labile.

5. In all living tissues the effect of inflammation manifests itself in a modification of the action and properties of individual cells. In cells which form part of permanent structures the protoplasm increases in quantity and becomes more or less contractile. Subsequently, it is converted entirely or partly into young cells, either by cleavage or by endogenous germination.

J. BURDON-SANDERSON.

Addendum.

The destructive effects of inflammation are traced with a master's hand in the following paragraphs, reprinted from the article on the same subject which appeared in the former edition of this work. After pointing out that both for pathology and practice it is needful that the student recognize the reality of destructive changes as an essential part of inflammation, Mr. Simon continues:—

'Let him examine inflamed muscle, as, for instance, in the post-mortem examination of a compound fracture or of a recently made stump:—He will find the structure weakened, so that it easily gives way with pressure or traction; he will see, under the microscope, that the substance tends to fall into irregular fragments; that its natural striation is more or less replaced, first by an almost homogeneous appearance, and afterwards by an appearance of aggregated granules; that, with these granules of albuminous matter into which the muscle has resolved itself, there is mixed, even from an early date in the inflammation, a noticeable quantity of minute oil-drops; that often these oil-drops appear before the disintegration of muscle has made much progress, and then arrange themselves in such mutual relation, transverse or longitudinal, as to suggest that the sarcous elements have changed themselves, particle by particle, into oil; that little by little the oil-drops multiply to such an extent as to be the chief visible objects—the limitary membrane of a fasciculus seeming now to be almost filled with finely-divided oil, diffused through some scanty connective albuminous material; that the limitary membrane, within which the muscular material is thus emulsionised, tends also itself to undergo dissolution, and let its proceeds confuse themselves with the similar débris of neighbouring fasciculi, till more or less bulk of muscle is reduced to a state of oleo-albuminous liquidity.

'And from this point, if the observer have opportunity of watching the changes which lead to convalescence, he will see that gradually the liquefied material diminishes in volume; that, in proportion as it vanishes, the adjoining parts adapt themselves to the altered relation; that eventually only a scar-like puckering of substance—a kind of tendinous intersection—remains to mark the place where muscular material has irrecoverably melted away.

'Let him examine inflamed bone, as, for instance, in a carious vertebra:—He will see that the structure breaks down under his finger, and offers scarcely any resistance to a knife; that the microscopical texture is rarefied—cancelli, canals, lacunæ, being all larger than natural, and the solid framework all scantier; that the material is tending to break into its component parts, and to undergo changes which admit of its being removed by the circulation. In many cases (for example, under the irritant pressure of an aneurism) he will find that a quantity of bone has thus gone, leaving no trace behind—gone of course, only after having first become liquid; and it appears that, when bone is inflamed, the first step towards this disintegration consists in a breach of the ordinary union between the mineral and cartilaginous constituents, with a primary removal of the former, and a chemical change of the latter. If there be discharge from the inflamed part, there will be found in it bits of bone, chemically and microscopically demonstrable.

'Let him examine inflamed nerve—as, for instance, near to where it has been cut in amputation:—He will find, says Dr.

Lent, the medullary cylinder of each nerve tubule falling, as it were by cross-cuts, into irregular pieces—at first large, but as the process advances, getting smaller and rounder, and assuming the character of oil; till at last the tube-membrane is filled with oily material, which gradually undergoes removal.

'Let him examine the hard textures of an acutely suppurating joint:—He will find the strongest ligaments in course of being reduced to an incoherent state—either actually pulpy and half-liquefied and in course of removal, or ready to break with the least traction; he will find (unless proper splintage have been used to prevent it) that dislocation is occurring from this cause; he will find, if the inflammation have been primarily synovial, that the cartilage is smoothly melting away at its surface into the fluid which bathes it, or, if the disease have begun subarticularly, that the cartilage, where superjacent to carious bone, is irregularly eroded and perforated; and throughout, with the microscope, he will find, wherever there are evidences of advancing disintegration, that the softening material is abundantly marked with oil-drops.

'Let him—not in post-mortem examinations, for which there are no opportunities, but during life—observe the results of inflammation of the sclerotic, and ask himself why it is that staphyloma so often follows this disease. He will infer that here, as with other cases which we have considered, the inflammation must have so disorganised the texture, and so enfeebled its normal rigidity, that it can no longer give sufficient resistance to pressure from within, or save itself from being bulged by what now becomes an almost dropsical excess of fluid secretion within the globe.

'Above all, let him examine the products of inflammation furnished by mucous and serous membranes, and by glands: the expectorations of bronchitis, the hawkings of common throat-catarrh, the urine of scarlatina, the acute effusion of serous cavities, and, after death, the inflamed organs themselves. Let him once thoroughly recognise the destructive acts of inflammation, as illustrated in the simple cells of gland or epitheliated membrane; and the whole of this argument will be compendiously before him. He will find cells (especially where they are squamous) shed as dead material, without their first undergoing any appreciable alteration. He will find all others undergoing change in a more or less marked degree—change, of which the essence consists in a loosening and eventually a disintegration of texture, with increased imbibability by fluid, and gradual accumulation of oil; so that the cell, while undissolved, appears of larger than natural size, its wall less defined, its nucleus dimmer, its contents more granular and oily than in health. Sometimes a cell is thus converted into a mere heap of oil-drops held together by little intervening or surrounding material; sometimes there will be more albuminous matter, perhaps in a granulated or dotty form; sometimes there will be more evident fludity of contents; but in any case the cell, if retained within the body, tends to break up and contribute with its neighbours to the making of an oleo-albuminous fluid, in which there exist but scanty and evanescent remains of the original cell structure.'

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ADDRESS

TO THE

BIOLOGICAL SECTION

OF THE

BRITISH ASSOCIATION.

EDINBURGH, August 3rd, 1871.

BY

Professor ALLEN THOMSON, F.R.S.L. & E., President.

In now opening the Meetings of the Biological Section, it is my first duty to express my deep sense of the honour which has been conferred upon me in appointing me to preside over its deliberations. I trust that my grateful acceptance of the office will not appear to be an assumption on my part of more than a partial connexion with the very wide field of science included under the term Biology. I should gladly have embraced the opportunity now afforded me of conforming to a custom which has of late become almost the rule with the Presidents of Sections, viz. that of bringing under your review a notice of the more valuable discoveries with which our science has been enriched in recent times, were it not that the subjects which I might have been disposed to select would require an amount of detail in each which would necessarily limit greatly their number, and that any attempt to overtake the whole range of this widespread department of science, even in the most general remarks, would be equally presumptuous and futile on the part of one whose attention has been restricted mainly to one of its divisions. I am further embarrassed in the choice of topics for general remark by the circumstance that many of those upon which I might have ventured to address you have been most ably treated of by my predecessors, as, for example, in the Sectional Addresses of Dr. Acland, Dr. Sharpey, Mr. Berkeley, Dr. Humphry, and Dr. Rolleston, as well as in the General Presidential Addresses of Dr. Hooker and Professor Huxley. I must content myself, therefore, with endeavouring to convey to you some of the ideas which arise in my mind in looking back from the present upon the state of Biological Science at the time when, forty years since, the Meetings of the British Association commenced—a period which I am tempted to particularize from its happening to coincide very nearly with that at which I began my career as a public teacher in one of the departments of Biology in this city. In the few remarks which I shall make, it will be my object to show the prodigious advance which has taken place not only in the knowledge of our subject as a whole, but also in the ascertained relation of its parts to each other, and in the place which Biological knowledge has gained in the estimation of the educated part of the community, and the consequent increase in the freedom with which the search after truth is now asserted in this as in other departments of science.

And first, in connexion with the distribution of the various subjects which are included under this Section, I may remark that the general title under which the whole Section D has met since 1866, viz. Biology, seems to be advantageous both from its convenience, and as tending to promote the greater consolidation of our science, and a juster appreciation of the relation of its several parts. It may be that, looking merely to the derivation of the term, it is strictly more nearly synonymous with physiology in the sense in which that word has been for a long time employed, and therefore designating the science of life, rather than the description of the living beings in which it is manifested. But until a better or more comprehensive term be found, we may accept that of biology under the general definition of "the science of life and of living beings," or as comprehending the history of the whole range of organic nature—vegetable as well as animal. The propriety of the adoption of such a general term is further shown by a glance at the changes which the titles and distribution of the subordinate departments of this Section have undergone during the period of the existence of the Association.

During the first four years of this period the Section met under the combined designation of Zoology and Botany, Physiology and Anatomy—words sufficiently clearly indicating the scope of its subjects of investigation. In the next ten years a connexion with Medicine was recognized by the establishment of a subsection or department of Medical Science, in which, however, scientific anatomy and physiology formed the most prominent topics, though not to the exclusion of more strictly medical and surgical or professional subjects. During the next decade, or from the year 1845 to 1854, we find along with Zoology and Botany a subsection of Physiology, and in several years of the same time along with the latter a separate department of Ethnology. In the eleven years which extended from 1855 to 1865, the branch of Ethnology was associated with Geography in Section F. More recently, or since the arrangement which was commenced in 1866, the Section Biology has included, with some slight variation, the whole of its subjects in three departments. Under one of these are brought all investigations in Anatomy and Physiology of a general kind, thus embracing the whole range of these sciences when without special application. A second of these departments has been occupied with the extensive subjects of Botany and Zoology; while the third has been devoted to the subject of Anthropology, in which all researches having a special reference to the structure and functions or life-history of man have been received and discussed. Such I understand to be the arrangement under which we shall meet on this occasion. At the conclusion of my remarks, therefore, the department of Anatomy and Physiology will remain with me in this room; while that of Zoology and Botany, on the one hand, and of Anthropology on the other, will adjourn to the apartments which have been provided for them respectively.

With regard to the position of Anthropology, as including Ethnology, and comprehending the whole natural history of man, there may be still some differences of opinion, according to the point of view from which its phenomena are regarded: as by some they may be viewed chiefly in relation to the bodily structure and functions of individuals or numbers of men; or as by others they may be considered more directly with reference to their national character and history, and the affinities of languages and customs; or by a third set of inquirers, as bearing more immediately upon the origin of man and his relation to animals. As the first and third of these sets of topics entirely belong to Biology, and as those parts of the second set which do not properly fall under that branch may with propriety find a place under Geography or Statistics, I feel inclined to adhere to the distinct recognition of a department of Anthropology, in its present form; and I think that the suitableness of this arrangement is apparent, from the nature and number of the appropriate reports and communications which have been received under the last distribution of the subjects.

The beneficial influence of the British Association in promoting biological research is shown by the fact that the number of the communications to the Sections received annually has been nearly doubled in the course of the last twenty years. And this influence has doubtless been materially assisted by the contributions in

money made by the Association in aid of various biological investigations; for it appears that, out of the whole sum of nearly £34,500 contributed by the Association to the promotion of scientific research, about £2800 has been devoted to biological purposes, to which it would be fair to add a part at least of the grants for Palæontological researches, many of which must be acknowledged to stand in close relation to Biology.

The enormous extent of knowledge and research in the various departments of Biology has become a serious impediment to its more complete study, and leads to the danger of confined views on the part of those whose attention, from necessity or taste, is too exclusively directed to the details of one department, or even, as often happens, to a subdivision of it. It would seem, indeed, as if our predecessors in the last generation possessed this superior advantage in the then existing narrower boundaries of knowledge, that it was possible for them to overtake the contemplation of a wider field, and to follow out researches in a greater number of the sciences. To such combination of varied knowledge, united with their transcendent powers of sound generalization and accurate observation, must be ascribed the widespread and enduring influence of the works of such men as Haller, Linnæus, Cuvier, Von Baer, and Johannes Müller. There are doubtless brilliant instances in our own time of men endowed with similar powers; but the difficulty of bringing these powers into effectual operation in a wide range is now so great, that, while the amount of research in special biological subjects is enormous, it must be reserved for comparatively few to be the authors of great systems, or of enduring broad and general views which embrace the whole range of biological science. It is incumbent, therefore, on all those who are desirous of promoting the advance of biological knowledge to combat the confined views which are apt to be engendered by the too great restriction of study to one department. However much subdivision of labour may now be necessary in the original investigation and elaboration of new facts in our science (and the necessity for such subdivision will necessarily increase as knowledge extends), there must be secured at first, by a wider study of the general principles and some of the details of collateral branches of knowledge, that power of justly comparing and correlating facts which will mature the judgment and exclude partial views. To refer only to one bright example, I might say that it can scarcely be doubted that it is the unequalled variety and extent of knowledge, combined with the faculty of bringing the most varied facts together in new combination, which has enabled Dr. Darwin (whatever may be thought otherwise of his system) to give the greatest impulse which has been felt in our own times to the progress of biological views and thought; and it is most satisfactory to observe the effect which this influence is already producing on the scientific mind of this country, in opposing the tendency perceptible in recent times to the too restricted study of special departments of natural history. I need scarcely remind you that for the proper investigation and judgment of problems in physiology, a full knowledge of anatomy in general, and much of comparative anatomy, of histology and embryology, of organic chemistry and of physics, is indispensable as a preliminary to all successful physiological observation and experiment. The anatomist, again, who would profess to describe rationally and correctly the structure of the human body, must have acquired a knowledge of the principles of morphology derived from the study of comparative anatomy and development, and he must have mastered the intricacies of histological research. The comparative anatomist must be an accomplished embryologist in the whole range of the animal kingdom, or in any single division of it which he professes to cultivate. The zoologist and the botanist must equally found their descriptions and systematic distinctions on morphological, histological, and embryological data. And thus the whole of these departments of biological science are so interwoven and united that the scientific investigation of no one can now be regarded as altogether separate from that of the others. It has been the work of the last forty years to bring that intimate connexion of the biological sciences more and more fully into prominent view, and to infuse its spirit into all scientific investigation. But while in all the departments of biology prodigious advances have been made, there are two more especially which merit particular mention as having almost taken their origin within the period I now refer to, as having made the most rapid progress in themselves, and as having influenced most powerfully and widely the progress of discovery, and the views of biologists in other departments—I mean Histology and Embryology.

I need scarcely remind those present that it was only within a few years before the foundation of the British Association that the suggestions of Lister in regard to the construction of achromatic lenses brought the compound microscope into such a state of improvement as caused it to be restored, as I might say, to the place which the more imperfect instrument had lost in the previous century. The result of this restoration became apparent in the foundation of a new era in the knowledge of the minute characters of textural structure, under the joint guidance of Robert Brown and Ehrenberg, with contributions from many other observers, so as at last to have almost entitled this branch of inquiry to its designation, by Mr. Huxley, of the exhaustive investigation of structural elements. All who hear me are aware of the influence which, from 1839 onwards, the researches of Schwann and Schleiden exerted on the progress of Histology and the views of anatomists and physiologists as to the structure and development of the textures both of plants and animals, and the prodigious increase which followed in varied microscopic observations. It is not for me here even to allude to the steps of that rapid progress by which a new branch of anatomical science has been created; nor can I venture to enter upon any of the interesting questions presented by this department of microscopic anatomy; nor attempt to discuss any of those difficult problems possessing so much interest at the present moment, such as the nature of the organized cell or the properties of protoplasm. I would only remark that it is now very generally admitted that the cell-wall (as Schwann indeed himself pointed out) is not a constant constituent of the cell, nor a source of new production, though still capable of considerable structural change after the time of its first formation. The nucleus has also lost some of the importance attached to it by Schwann and his earlier followers, as an essential constituent of the cell, while the protoplasm of the cell remains in undisputed possession of the field as the more immediate seat of the phenomena of growth and organization, and of the contractile property which forms so remarkable a feature of their substance. I cordially agree with much of what Mr. Huxley has written on this subject in 1853 and 1869. The term physical basis of life may perhaps be in some respect objectionable; but I look upon the recognition of protoplasm which he has enforced as a most important step in the recent progress of histology, adopting this general term to indicate that part of the organized substance of plants and animals which is the constant seat of the growing and moving powers, but not implying identity of nature and properties in all the variety of circumstances in which it may occur. To Haeckel the fuller history of protoplasm in its lowest forms is due. To Dr. Beale we owe the minutest and most recent investigation of these properties by the use of magnifying-powers beyond any that had previously been known, and the successful employment of reagents which appear to mark out its distinction * from the other elements of the textures. I may remark, however, in passing, that I am inclined to regard contractile protoplasm, whether vegetable or animal, as in no instance entirely amorphous or homogeneous, but rather as always presenting some minute molecular structure which distinguishes it from parts of glassy clearness. Admitting that the form it assumes is not necessarily that of a regular cell, and may be various and irregular in a few exceptional instances, I am not on that account disposed to give up definite structure as one of the universal characteristics of organization in living bodies. I would also suggest that the terms formative and non-formative, or some other such, would be preferable to those of "living and dead," employed by Dr. Beale to distinguish the protoplasm from the cell-wall or its derivatives, as these latter terms are liable to introduce confusion.

To the discoveries in embryology and development I might have been tempted to refer more at large, as being those which have had, of all modern research, the greatest effect in extending and modifying biological views, but I am warned from entering upon a subject in which I might trespass too much on your patience. The merits of Wolff as the great leader in the accurate observation of the phenomena of development were clearly pointed out by Mr.

* Under the appropriate name of "bioplasm."

Huxley in his presidential address of last year. Under the influence of Döllinger's teaching, Pander, and afterwards Purkinje, Von Baer, and Rathke, established the foundations of the modern history of embryology. It was only in the year 1827 that the ovum of mammals was discovered by Von Baer; the segmentation of the yelk, first observed by Prevost and Dumas in the frog's ovum in 1824, was ascertained to be general in succeeding years; so that the whole of the interesting and important additions which have followed, and have made the history of embryological development a complete science, have been included within the eventful period of the life of this Association. I need not say how distinguished the Germans have been by their contributions to the history of animal development. The names of Valentin, R. Wagner, Bischoff, Reichert, Kölliker, and Remak are sufficient to indicate the most important of the earlier steps in recent progress, without attempting to enumerate a host of others who have assisted in the great work thus founded.

I am aware that the mere name of development suggests to some ideas of a disturbing kind as being associated with the theory of evolution recently promulgated. To one accustomed during the whole of his career to trace the steps by which every living being, including man himself, passes from the condition of an almost imperceptible germ, through a long series of changes of form and structure into their perfect state, the name of development is suggestive rather of that which seems to be the common history of all living beings; and it is not wonderful therefore that such a one should regard with approval the more extended view which supposes a process of development to belong to the whole of nature. How far that principle may be carried, to what point the origin of man or any animal can by facts or reasoning be traced in the long unchronicled history of the world, and whether living beings may arise independently of parents or germs of previously existing organisms, or may spring from the direct combination of the elements of dead matter, are questions still to be solved, and upon which we may expect this Section to guide the hesitating opinion of the time. I cannot better express the state of opinion in which I find myself in regard to the last of these problems, than by quoting the words of Professor Huxley from his address of last year, p. lxxxiii:—

"But though I cannot express this conviction of mine too strongly [viz. that the evidence of the most careful experiments is opposed to the occurrence of spontaneous generation], I must carefully guard myself against the supposition that I intend to suggest that no such thing as abiogenesis ever has taken place in the past, or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call 'vital,' may not, some day, be artificially brought together." And again, "If it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter." I will quote further a few wise words from the discourse to which many of you must have listened last evening with admiration. Sir William Thomson said:—"The essence of science, as is well illustrated by astronomy and cosmical physics, consists in inferring antecedent conditions, and anticipating future evolutions, from phenomena which have actually come under observation. In biology, the difficulties of successfully acting up to this ideal are prodigious. Our code of biological law is an expression of our ignorance as well as of our knowledge." And again, "Search for spontaneous generation out of inorganic materials; let any one not satisfied with the purely negative testimony of which we have now so much against it, throw himself into the inquiry. Such investigations as those of Pasteur, Pouchet, and Bastian are among the most interesting and momentous in the whole range of natural history, and their results, whether positive or negative, must richly reward the most careful and laborious experimenting."

The consideration of the finest discoverable structures of the organized parts of living bodies is intimately bound up with that of their chemical composition and properties. The progress which has been made in organic chemistry belongs not only to the knowledge of the composition of the constituents of organized bodies, but also to the manner in which that composition is chemically viewed. Its peculiar feature, especially as related to biological investigation, consists in the results of the introduction of the synthetic method of research, which has enabled the chemist to imitate or to form artificially a greater and greater number of the organic compounds. In 1828 the first of these substances was formed by Wöhler, by a synthetic process, as cyanate of ammonia. But still, at that time, though a few no doubt entertained juster views, the opinion generally prevailed among chemists and physiologists that there was some great and fundamental difference in the chemical phenomena and laws of organic and inorganic nature. Now, however, this supposed barrier has been in a great measure broken down and removed, and chemists, with almost one accord, regard the laws of combination of the elements as essentially the same in both classes of bodies, whatever differences may exist in actual composition, or in the reactions of organic bodies in the more complex and often obscure conditions of vitality, as compared with the simpler and, on the whole, better known phenomena of a chemical nature observed in the mineral kingdom. Thus, by the synthetic method, there have been formed among the simpler organic compounds a great number of alcohols, hydrocarbons, and fatty acids. But the most remarkable example of the synthetic formation of an organic compound is that of the alkaloid conia, as recently obtained by Hugo Schiff by certain reactions from butyric aldehyde, itself an artificial product. The substance so formed, and its compounds, possess all the properties of the natural conia—chemical, physical, and physiological—being equally poisonous with it. The colouring-matter of madder, or alizarine, is another organic compound which has been formed by artificial processes. It is true that the organized or containing solid, either of vegetable or animal bodies, has not as yet yielded to the ingenuity of chemical artifice; nor, indeed, is the actual composition of one of the most important of these, albumen and its allies, fully known. But as chemists have only recently begun to discover the track by which they may be led to the synthesis of organic compounds, it is warrantable to hope that ere long cellulose and lignine may be formed; and, great as the difficulties with regard to the albumenoid compounds may at present appear, the synthetic formation of these is by no means to be despaired of, but, on the contrary, may with confidence be expected to crown their efforts. From all recent research, therefore, it appears to result that the general nature of the properties belonging to the products of animal and vegetable life can no longer be regarded as different from those of minerals, in so far at least as they are the subject of chemical and physical investigation. The union of elements and their separation, whether occurring in an animal, a vegetable, or a mineral body, must be looked upon as dependent on innate powers or properties belonging to the elements themselves; and the phenomena of change of composition of organic bodies occurring in the living state are not the less chemical because they are different from those observed in inorganic nature. All chemical actions are liable to vary according to the conditions in which they occur; and many instances might be adduced of most remarkable variations of this kind, observed in the chemistry of dead bodies from very slight changes of electrical, calorific, mechanical, and other conditions. But because the conditions of action or change are infinitely more complex and far less known in living bodies, it is not necessary to look upon the phenomena as essentially of a different kind, to have recourse to the hypothesis of vital affinities, and still less to shelter ourselves under the slim curtain of ignorance implied in the explanation of the most varied chemical changes by the influence of a vital principle.

On the subjects of zoological and botanical classification and anthropology, it would be out of place for me now to make any observations at length. I will only remark, in regard to the first, that the period now under review has witnessed a very great modification in the aspect in which the affinities of the bodies belonging to these two great kingdoms of nature are viewed by naturalists, and the principles on which groups of bodies in each are associated together in systematic classification; for, in the first place, the older view has been abandoned that the complication of structure rises in a continually increasing and continuous gra-

dation from one kingdom to the other, or extends in one line, as it were, from group to group in either of the kingdoms separately. Evolution into a gradually increasing complexity of structure and function no doubt exists in both, so that types or general plans of formation must be acknowledged to exist, presenting typical resemblances of the deepest interest; but in the progress of morphological research it has become more and more apparent that the different groups form radiations, which touch one another at certain points of greatest resemblance, rather than one continuous line, or a number of lines which partially pass each other. The simpler bodies of the two kingdoms of nature exhibit a gradually increasing resemblance to each other, until at last the differences between them wholly disappear, and we reach a point of contact at which the properties become almost indistinguishable, as in the remarkable Protista of Haeckel and others. I fully agree, however, with the view stated by Professor Wyville Thomson in his recent introductory lecture, that it is not necessary on this account to recognize, with Haeckel, a third or intermediate kingdom of nature. Each kingdom presents, as it were, a radiating expansion into groups for itself, so that the relations of the two kingdoms might be represented by the divergence of lines spreading in two different directions from a common point. Recent observations on the chorda dorsalis, or supposed notochord, of some Ascidians, tend to revive the discussion, at one time prevalent, but long in abeyance, as to the possibility of tracing an homology between the vertebrate and invertebrate animals; and, should this correspondence be confirmed and extended, it may be expected to modify greatly our present views of zoological affinities and classification. It will also be an additional proof of the importance of minute and embryological research in systematic determinations. The recognition of homological resemblance of animals, to which in this country the researches of Owen and Huxley have contributed so largely, form one of the most interesting subjects of contemplation in the study of comparative anatomy and zoology in our time; but I must refrain from touching on so seductive and difficult a subject.

There is another topic to which I can refer with pleasure as connected with the cultivation of biological knowledge in this country, and that is the introduction of instruction in natural science into the system of education of our schools. As to the feasibility of this in the primary schools, I believe most of those who are intimately acquainted with their management have expressed a decidedly favourable opinion—it being found that a portion of the time now allotted to the three great requisites of a primary education might with advantage be set apart, for the purpose of instructing the pupils in subjects of common interest, calculated to awaken in their minds a desire for knowledge of the various objects presented by the field of nature around them. As to the benefit which may result from this measure to the persons so instructed, it is scarcely necessary for me to say anything in this place. It is so obvious that any varied knowledge, however easily acquired or elementary, which tends to enlarge the range of observation and thought, must have some effect in removing its recipients from grosser influences, and may even supply information which may prove useful in social economy and in the occupations of labour. Nor need I point out how much more extended the advantages of such instruction may prove if introduced into the system of our secondary schools, and more freely combined than heretofore with the too exclusively literary and philosophical study which has so long prevailed in the approved British education. Without disparagement to those modes of study as in themselves necessary and useful, and excellent means of disciplining the mind to learning, I cannot but hold it as certain that the mind which is entirely without scientific cultivation is but half prepared for the common purposes of modern life, and is entirely unqualified for forming a judgment on some of the most difficult and yet most common and important questions of the day, affecting the interests of the whole community. I refer with pleasure to the published Essay of Dr. Lankester on this subject, and to the arguments addressed two days ago by Dr. Bennett to the medical graduates of the University, in favour of the establishment of physiology as a subject of general education in this country with reference to sanitary conditions. It is gratifying, therefore, to perceive that the suggestions made some years ago in regard to this subject by the British Association, through its committee, have already borne good fruit, and that the attention of those who preside over education in this country, as well as of the public themselves, is more earnestly directed to the object of securing for the lowest as well as the highest classes of the community that wholesome combination of knowledge derived from education, which will duly cultivate all the faculties of the mind, and thus fit a greater and greater number for applying themselves with increased ability and knowledge to the purposes of their living and its improved condition. If the law of the survival of the fittest be applicable to the mental as well as to the physical improvement of our race (and who can doubt that in some measure it must be so?), we are bound by motives of interest and duty to secure for all classes of the people that kind of education which will lead to the development of the highest and most varied mental power. And no one who has been observant of the recent progress of the useful arts and its influence upon the moral, social, and political condition of our population, can doubt that such education must include instruction in the phenomena of external nature, including, more especially, the laws and conditions of life and health; and that it ought to be, at the same time, such as will adapt the mind to the ready acquisition and just comprehension of varied knowledge. It is obvious, too, that while this more immediately useful or beneficial effect on the common mind may be produced by the diffusion of natural knowledge among the people, biological science will share in the gain accruing to all branches of natural science, by the greater favour which will be accorded to its cultivators, and the increased freedom from prejudice with which their statements are received and considered by learned as well as by unscientific persons.

I cannot conclude these observations without adverting to one aspect in which it may be thought that the appreciation of biological science has taken a retrograde rather than an advanced position. In this, I do not mean to refer to the special cultivators of biology in its scientific acceptation, but to the fact that there appears to have taken place of late a considerable increase in the number of persons who believe, or who imagine that they believe, in the class of phenomena which are now called spiritual, but which have been known, since the exhibitions of Mesmer, and, indeed, long before his time, under the most varied forms, as liable to occur in persons of an imaginative turn of mind and peculiar nervous susceptibility. It is be regretted that a number of persons devote a large share of their time to the practice (for it does not deserve the name of study or investigation) of the alleged phenomena, and that a few men of acknowledged reputation in some departments of science have lent their names, and surrendered their judgment, to the countenance and attempted authentication of the delusive dreams of the practitioners of spiritualism, and similar chimerical hypotheses. The natural tendency to a belief in the marvellous is sufficient to explain the ready acceptance of such views by the ignorant; and it is not improbable that a higher species of similar credulity may frequently act with persons of greater cultivation, should their scientific information and training have been of a partial kind. It must be admitted, further, that extremely curious and rare and, to those who are not acquainted with the nervous functions, apparently marvellous phenomena, present themselves in peculiar states of the nervous system—some of which states may be induced through the mind and may be made more and more liable to recur, and to be greatly exaggerated by frequent repetition. But making the fullest allowance for all these conditions, it is still surprising that persons, otherwise appearing not to be irrational, should entertain a confirmed belief in the possibility of phenomena, which, while they are at variance with the best established physical laws, have never been brought under proof by the evidences of the senses, and are opposed to the dictates of sound judgment. It is so far satisfactory, in the interests of true biological science, that no man of note can be named from the long list of thoroughly well-informed anatomists and physiologists, who has not treated the belief in the separate existence of powers of animal magnetism and spiritualism as wild speculations, devoid of all foundation in the carefully tested observation of facts. It has been the habit of the votaries of the systems to which I have referred to assert that scientific men have neglected or declined to investigate the phenomena with attention and candour; but nothing can be further from the truth than this statement. Not to mention the admirable reports of the early French acade-

micians, giving the account of the negative result of an examination of the earlier mesmeric phenomena by men in every way qualified to pronounce judgment on their nature, I am aware that from time to time men of eminence, and fully competent, by their knowledge of biological phenomena, and their skill and accuracy in conducting scientific investigation, have made the most patient and careful examination of the evidence placed before them by the professed believers and practitioners of so-called mesmeric, magnetic, phrenomagnetic, electrobiological, and other like phenomena; and the result has been uniformly the same in all cases when they were permitted to secure conditions by which the reality of the phenomena, or the justice of their interpretation, could be tested, viz. either that, on the one hand, the phenomena were not essentially different from those well known to physiologists as modifications of the nervous and muscular functions under peculiar mental states; or that, on the other hand, the experiments signally failed to educe the results professed, or that the experimenters were detected in shameless and determined impostures. I have myself been fully convinced of this by repeated examinations; and I can scarcely doubt that the same fate awaits the fair scientific examination of the so-called spiritualistic phenomena*. But were any guarantee required for the care, soundness, and efficiency of the judgment of men of science on such phenomena and views, I have only to mention, in the first place, the revered name of Faraday, and in the next that of my life-long friend Dr. Sharpey, whose ability and candour none will dispute, and who, I am happy to think, is here among us, ready, from his past experience of such exhibitions, to bear his testimony against all cases of *levitation*, or the like, which may be the last wonder of the day among the mesmeric or spiritual pseudo-physiologists. The phenomena to which I have at present referred are in great part dependent upon natural principles of the human mind, placed, as it would appear, in dangerous alliance with certain tendencies of the nervous system. They ought not to be worked upon without the greatest caution, and they can only be fully understood by the accomplished physiologist who is also conversant with healthy and morbid psychology. The experience of the last hundred years tends to show that, while there are always to be found persons peculiarly liable to exhibit the phenomena in question, there will also exist a certain number of minds prone to adopt a belief in the marvellous and striking in preference to that which is easily understood and patent to the senses; but it may be confidently expected that the diffusion of a fuller and more accurate knowledge of physiology among the non-scientific classes of the community may lead to a juster appreciation of the phenomena in question, and a reduction of the number among them who are believers in scientific impossibilities.

* In consequence of several remonstrances made to me since the address was delivered, representing that the phenomena of spiritualism had not yet been subjected to a full scientific investigation, I have been induced to alter the two preceding sentences from their original into their present form. But I am still of opinion that these phenomena belong essentially to the same class as those of Mesmerism and Electrobiology.

D^r. Parkes. F.R.S. &c.

REMARKS

ON THE

CULTIVATION AND EXTRACTION

OF

China-grass-cloth or Rheea fibre.

BY

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Bœhmeria nivea, the plant known as China grass, from the fibre of which China grass cloth is made, is not a species of grass, but a nettle. It is indigenous to South Eastern Asia, and in China is known as Ma or Chú ma, while in Assam, where also it is utilized, it is called Rheea.*

2. *Other fibre-yielding Nettles.* A nettle indigenous to the lower slopes and base of the Himalayas from the Bhurrampootra to the Ganges, and known botanically as *Bœhmeria puya* (the *Urtica frutescens* of Roxburgh) is in appearance rather like the true China Grass plant, and yields a fibre but little inferior. It is distinguished from the latter by its narrower leaves. The vernacular name of this species in Gharwal and Kumaon is Pooe, which farther East changes

* The identity of Rheea with the Chinese Chú ma is now fairly established. (See Journal A. and H. Society of India, Vol. vii p. 91.)

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into Púya or Pooah. Its fibre is very strong, and is largely used by the hill men for making nets and ropes. Several other Indian nettles yield fibres which would probably prove valuable if utilized. Chief among these are the following :—

1. *Urtica crenulata.* This yields a white, strong, but not very lasting fibre. It is common in Eastern Bengal where it goes under the name of Chor Putta.

2. *Urtica heterophylla.* An annual plant, yielding a fine, soft, and lasting, fibre; of wide distribution in mountainous districts of India; known as Horoo Surat in Assam: in Ghurwal and Kumaon it is classed along with other nettles under the common term Bichchhú.

3. *Urtica virulenta.*

4. *Urtica salicifolia.*

The tract at the base of the Himalayas abounds in Urticaceous plants from which the hill men extract fibres in small quantity, but of the value of these I am unable to give an opinion.

3. *Character of the plant.* The true China-grass cloth nettle is a herbaceous plant, with large, perennial, spreading, much divided roots, from which rise a number (from seven to ten) of straight, slender slightly branching stems, from the bárk of which the fibre is extracted. Naturally twice, but under cultivation three, four, and even five times a year, according to climate and soil, a fresh set of stems shoots up from the root.

The proper time to cut the old twigs for their fibre is when they have begun to become brown at their bases, and the young ones are about an inch in height. In the Government gardens at Deyrah Dhoon, where the object aimed at has been the propagation of the plant, and not the extraction of its fibre, the stems have hitherto been cut down only twice a year. I think however that if well manured and watered, three crops (as is the case in China) might be obtained. It is in the moist climate of Assam that, as mentioned above,

four or five crops may be obtained in a year. The plant is a very hardy one, and thrives well in parts of India differing so much in climate and other physical conditions as Assam, Bengal, the North West Provinces, and the Kangra valley in the Punjab. It has also I believe been introduced with success in the Madras Presidency. In Deyrah Dhoon, some old plants throw up shoots from eight to ten feet high, and six feet is a common height. An eight foot shoot, if carefully manipulated, will yield a fibre six feet long.

4. *Limit of growth.* The garden in Deyrah Dhoon is about 2200 feet above the sea level, and the plantations in the Kangra valley are probably higher. There are no exact records known to me showing the height at which the Chinese nettle will thrive best. It grows however freely in the plains at very low elevations. At Seharunpore, which is about 1000 feet above the level of the sea, the plants are very green and healthy, and reach a height of 5 to 7 feet.

5. *Soil and Shade.* The Chinese prefer a rather stiff soil; but, I gather from a communication in a former number of the Journal of this Society that, in Assam a loose rich soil is considered the best. That in the Deyrah Dhoon garden is of the former description; whereas the patch of ground planted with *Bœhmeria* at Seharunpore is rather light and sandy. My own experience, which however is but limited, leads me to think moderate shade an advantage.

The finest plants in the garden here (Seharunpore) are a few grown under trees; and shade appears to be the only condition of growth in which they differ from less vigorous plants near them. Shelter from high winds is of great advantage, as, from the size of the leaves in proportion to the thickness of the stem, the latter is rather easily bent by storms.

6. *Moisture and manure.* A good supply of moisture is undoubtedly required, and regular irrigation would be necessary in the plains of upper India. But of all the requisites for

successful cultivation, I believe the first in importance to be *manure*, and this is the one least recognised in Indian Agriculture.

The Chinese manure extensively. They plant out in soil which has been carefully prepared and richly manured. They also use liquid manure, and in the cold season give a top dressing of stable litter after each cutting; whenever it is available they also apply night-soil.

7. *Propagation and Cultivation.* The plant being one of those in which the male and female flowers are separate, and situated on different parts of the stem, the production of seed is uncertain in localities where the insects are not indigenous by which fecundation is probably for the most part accomplished. In districts where Bœhmeria has been introduced, propagation has therefore been conducted not by seed, but by cuttings, and by division of the roots of old plants. By cuttings it may be propagated very easily, as with ordinary care scarcely one fails to strike. During damp weather, roots of old plants may be freely broken up into small pieces, and these if planted out into well prepared nurseries thrive well. This is the favourite mode of propagation in China.

Both cuttings and fragments of root should be planted about a foot or a foot and a half apart. This however is a matter in which the grower must be guided to some extent by local circumstances. The great desideratum is, that they shall be planted close enough to prevent their throwing out lateral branches, which are injurious both because they prevent the leaders from attaining the height they otherwise would, and because at every point where a branch leaves the stem there is a tendency, more or less great, of the band of fibre to break during the process of extraction; and the value of the fibre is in proportion to its length and equality. The soil between the plants should be frequently broken up so as to keep it loose, and ought of course to be kept free from

weeds. Top-dressing with manure is strongly insisted on by Chinese cultivators.

8. *Cost of cultivation.* In estimating the return to the cultivator, the plant being in the ground all the year round, both Rabee and Kureef land rent must be debited against the crop, and also Water-rent where irrigation is necessary. Besides this, allowance must be made for more manure than the native cultivator usually puts on his land. The amount of *labour* wanted in an established field would not be great, and the nature of it has already been indicated; watching against birds by day would not be wanted, but a guard against pig and especially deer at night would likely be necessary. Major Hannay and Capt. Dalton, two gentlemen who at the request of the Honble E. I. Company gave much attention to the growth of this fibre in Assam, give rather conflicting accounts of the cost of cultivation in that province. Major Hannay estimates the expense at five Rupees a maund (£ 14 a ton) and reports that "Rheea can be produced and sold with profit at as cheap" a rate as Russian *Hemp.*" This is stated by Dr. Royle to have been a mistake, and he says that ten rupees were meant.

Capt. Jenkins puts down the cost at ten rupees per maund (£ 28 a ton), and Capt. Dalton, Collector of Luckimpore, states that the lowest price at which it is likely to be procurable by *purchase from the cultivator* is six annas a seer, or about £ 42 per ton, but says that "when it is more exten-"sively cultivated, and the best method of preparation un-"derstood, so that women and children may be employed as "well as men, it ought not to cost more than four anna as "seer or £ 28 per ton."*

9. *Probable produce per Acre.* Not having had enough practical experience of the cultivation of the plant for the sake of its fibre, I cannot venture to say what the produce

* See Royle's Fibrous plants of India, quoting from the Journal of the A. and H. Society of India.

on well manured, well watered soil in upper India might be. It is stated by Dr. Royle that twelve maunds is the aggregate annual yield per acre in Assam, but there, as has just been stated, four or five crops can be gathered, and I fear only two or at most three could be looked for in upper India. I cannot however discover whether Dr. Royle's estimate refers to thoroughly cleaned fibre, or to fibre in the rough state before the softer vegetable tissues that surround it have been quite removed.

10. *Process of separation of the Fibre.* The methods pursued in China are all manual, and they seem to vary in different parts of that country. One way is, to remove the leaves immediately on cutting the stems, to soak the latter for a short time in water cold or tepid according to the season of the year; after this to bend them in the middle so as to loosen the fibrous portion from the woody and cellular tissue of the stalk at that point, and to remove the fibre by introducing the finger at the opening thus made and stripping it off. The amount of soaking to which the stems are subjected varies with climate and temperature, but is never long extended. Another way is, after soaking to cut off the roots, separate the fibre at the root extremity only, and strip it off by drawing it over a pin fixed in a plank. In a third method the stems are split longitudinally by a knife and the fibre peeled off each half separately.

11. *Cleaning and Bleaching.* The fibre thus removed is next scraped, when in a moist state, with a blunt knife. The knife is held in the left hand, its edge being opposed to the left thumb. The strips of hemp are then drawn between the thumb and the blade, pressure meanwhile being made by the thumb. Scraping is often also done on a smooth board, the blunt knife being firmly pressed down on the fibre as it is drawn across it. By these means the softer matters which cling to it are removed, and the fibre thus cleaned, curls up. It is wiped dry, exposed to the sun for a day, and then

assorted according to quality. It is next subjected to bleaching processes. These consist in exposure to dew at night, and to the sun by day; excessive moisture being however carefully avoided as the fibre is discoloured thereby. Boiling with alkalies is also practised in order to secure whiteness. As however the details vary a good deal in different provinces of China, I offer no apology for transcribing the following translated extract from the *Imperial Treatise of Chinese Agriculture* quoted by Dr. Royle in his work on the Fibrous Plants of India.*

"The stems are tied up in little sheaves and placed on the "roof of a house, in order that they may be moistened by "the dew at night, and dried again by the Sun in the day.

"In the course of five to seven days they become perfectly "white. If the weather be cloudy or rainy, the stems are "placed under cover in a current of air. If they are wetted "by the rain they immediately turn black. Another author "says, after peeling the fibres they are tied in skeins, arranged "in a circle, and steeped for a night in a pan of water; they "are then spun on a wheel. This done, they are again "steeped in water containing the ashes of burnt mulberry "wood. Having taken them from the pans they are divided "into packets of 5 oz. weight each; the packets are placed "for a night in a tub of a mixture consisting of a cup of "pure water, and an equal quantity of powdered chalk to "each packet.

"The next day the chalk is got rid of, and the fibres are "boiled in water containing straw ashes, by which process "they become white and supple. Being now dried in the "sun, they are again boiled in pure water; they are then "stirred about in more water, which finishes the cleansing "process, and lastly they are dried in the sun.

* Page 362. See also Journal A. and H. Society of India, Vol. VII page 30 App.

"This done, the fibres are joined end to end on the wheel "so as to make long threads, which form the warp and the "woof, and are manufactured into stuff in the usual way.

"Another author says, after having spun the fibres of "*tchou-ma*, they are boiled in lime water, and when cool, "carefully washed in pure water. Then by means of a bam- "boo sieve, placed on the surface of the water, they are "spread out in equal layers in order that they may be as it "were half wetted below, and half dried above. As night "approaches, they are taken out, strained and dried; the "same process is repeated the next and following days, until "the threads are perfectly white. They are then, but not "before, fit for being made up.

"According to another process, the *tchou-ma* is first soak- "ed, then spun and made up, instead of being soaked after "the spinning. Other persons again take the fresh fibres, "expose them at night to the dew and in the day to the sun; "then spin and weave, bleaching last of all. Others, lastly, "following those who employ the plant Ko, cut the stems, "soften the fibres in the steam of boiling water, then weave, "and do not bleach at all. Fibres thus prepared give a "material that is more supple and fibrous."

In Assam, Major Hannay cleaned the newly extracted fibre by tying it up in bundles, and soaking in water for a few hours. When softened, a bundle was put on a hook fastened to a post, and the operator standing in front of it, by taking one strand of fibre at a time in his hand and passing it quickly through his fingers, freed it from the softer vegetable matter, any tougher portion which remained being subsequently removed by a knife.

Following the directions furnished to me by several Chinamen, I made some experiments on the manual extraction of the fibre. The only stems at my disposal were however rather old and hard, and on that account unusually difficult to manipulate.

I however learnt enough to convince me that the extraction by hand processes is difficult, slow, and expensive. Steeping in water for an hour or two had no effect whatever in facilitating the separation of the fibre from the stem. I tried steeping in plain water, in water with various proportions of unslacked lime in it, and in solutions of potash of various strengths, and for various periods varying from a day to a week. The stronger alkaline solutions were the most effectual, but whether the use of chemicals has any deleterious action on the fibre, I am not prepared to say. Seeing that potash is used in the preparation of Russian flax, I should not anticipate any harm from its moderate use. I also tried beating the fibre out of the stems both in a fresh state, and after they had been steeped. Pressure I intended to try, but my experiments were limited by a scanty supply of material.

12. *Machinery.* In the Jury Report of Class IV. at the great London Exhibition of 1851, the process of Messrs. L. W. Wright and Co. for separating the fibre from the stem, is described with commendation, and a medal is awarded to Messrs. W. Wright for specimens of the fibre prepared by them. The process consists in an arrangement for boiling the stems in an alkaline solution, after they have previously been steeped for twenty four hours in water of a temperature of 90 Fht. The fibre is then washed with pure water, and subjected to a current of high-steam pressure till nearly dry.

The desideratum for the Indian grower is a chemical process or a machine which shall enable him to effect the rough separation of the fibre from the stem at a cheap rate. The English manufacturer prefers to buy the fibre in this rough condition, and to undertake all subsequent processes himself, as in doing so, lies his greatest profit. It was found that fibre in the rough state is apt to ferment during its passage to England; and to obviate the liability to this, must therefore be a prominent feature in any successful process of extraction. I think it probable that a machine on the principle of Hill and Bundy's for breaking and preparing the fibre of *raw Flax, Hemp, Sunn* and similar plants, without steeping or dew wetting might be devised without difficulty, for *Bœhmeria.*

The frame work of Hill and Bundy's machine can, I believe, be made of wood, and its principle (that of conical longitudinally ridged rollers revolving independently of each other) being very simple, I think it suitable for being both worked and made by natives. It is possible however that the extraction may be cheaply effected by some chemical process, involving the use of alkalies, and not requiring machinery. Experiments should be made on this point. I hear, that a gentleman in upper India has invented a cheap and effectual process, but as he has not yet made it public, I do not know in what it consists.

13. *Value of Produce.* There being as yet no means of working it into cloth in India, there is no demand for this fibre in the bazars, consequently a price cannot be quoted. In Assam, Rheea fibre is utilized for domestic consumption only, and is chiefly made into fishing-lines and nets. It is with difficulty obtainable by purchase, and naturally yields a high price. The Commissioner of Assam, writing to the Board of Revenue L. P. in 1868, says that in Tezpore, the bazar rate was then one rupee a seer.

Statements as to the price obtained for samples prepared for the English market vary exceedingly. Mr. Sangster, an English manufacturer, who seems to have turned his attention to this fibre, offered Major Hannay only £ 20 for Assam samples delivered in Calcutta. On the other hand, parcels of China grass fibre have been sold in England, since the Exhibition of 1851, at prices varying from £ 50 to £120 per ton. Mr. Marshall, a Leeds manufacturer and a consumer of *Bœhmeria* fibre of Chinese production, valued some samples of Assam Rheea as equal to second class China fibre, and worth £48 to £50 a ton delivered in England; but other

manufacturers considered it of higher value than the price quoted by Mr. Marshall.* Samples dressed so as to resemble floss-silk have been valued at the large sum of £ 280 per ton. The discrepancies in the prices realised for the raw fibre may be partly accounted for by the fact that it is apt to vary much in quality, the first cutting of the season in China being always coarser than the second, which again, though stronger, is not so fine as the third. Its value also depends on the treatment it has received during extraction, and on its freedom from a small black spot by which it is often disfigured. There is very little machinery in England suited to the manufacture of this fibre, which is as yet indeed little more than a curiosity in the home market; and although the few samples hitherto sent home have fetched high prices, a continuance of such rates could scarcely be expected, were it to be imported largely into Britain. As *Bœhmeria* fibre has the recommendations of being long, soft, strong, and capable of being bleached very white, it is probable that it would be well worth while for the English manufacturer to adapt machinery to it, were a regular supply forthcoming. *Bœhmeria* is about the strongest of known vegetable fibres. Dr. Royle's experiments made with equal weights and equal lengths of various unmanufactured fibres gave the following results :—

Petersburgh Clean Hemp,	broke with,	160lbs.
A fibre from Travancore called Wuckoo,	,,	175 ,,
Yercum fibre,	,,	190 ,,
Jubbulpore Hemp,	,,	190 ,,
China-grass from China,	,,	250 ,,
Rheea fibre or China-grass, from Assam,	,,	320 ,,
Wild Rheea, also from Assam,	,,	343 ,,

14. *Experiments in India, and chance of success.* So far back as 1811, some bales of this fibre, though under another name, were sent to England from the Calcutta Botanic Garden.

* Journal A. and H. Society of India. Vol. ix p. 44.

Further supplies were sent during several subsequent years, and within the past twenty several parcels have gone from Assam.

Some time ago small plantations were formed in the Government Gardens at Seharunpore and in Deyrah Dhoon, and it has been proved by these that the plant grows freely, and is easily propagated in upper India. Private attempts to grow it have been begun in Deyrah Dhoon, and in the Kangra valley. In the former locality, there is at present a good deal of interest being manifested in it, and I believe a good many Europeans there are disposed to try it, encouraged by the hope of finding a paying mode of extraction, and undaunted by the unfavourable results of the trials already made in the lower Provinces. The one great objection to this fibre is the difficulty of extracting it. The manual processes already mentioned are so very slow and expensive, and Indian labour is so very much inferior to Chinese, that until a cheap and simple machine be put within easy reach of the cultivator or a chemical process be invented, Indian-grown fibre can never, I fear, enter into competition with Chinese, and little progress can be made in extending its cultivation in this country.

SEHARUNPORE:
10*th September* 1869.

INDIAN ECONOMIC BOTANY AND GARDENING.

1. *Punjab plants, comprising Botanical and Vernacular names, and uses of most of the trees, shrubs, and herbs of economical value growing within the Province.* By J. Lindsay Stewart, M.D., F.L.S., F.R.G.S., &c., Conservator of Forests, Punjab. Lahore, 1869.
2. *Pharmacopœia of India, prepared under the authority of Her Majesty's Secretary of State for India in Council.* By Edward John Waring, M.D., Surgeon in Her Majesty's Indian Army, assisted by a Committee appointed for the purpose. London, 1868.
3. *A Manual of Gardening for Bengal and Upper India.* By T. A. C. Firminger, M.A., Chaplain, Bengal Establishment. Second Edition. Calcutta, 1869.

CONSIDERING the length of time that the English have been masters of India, they can hardly be congratulated on the extent or success of their efforts, either in making themselves acquainted with the vegetable productions of so noble a possession, or in utilizing and adding to them. Activity in the former direction, was indeed greater in times bye-gone, than it has been of late. Indian Botanists of recent days can point to no such results of their labors as the *Plants of the Coromandel Coast* of Roxburgh, or the *Flora Indica* of the same author (semi-obsolete as the latter book has now become), the *Plantæ Asiaticæ Rariores* of Wallich, or the *Icones Plantarum Indiæ Orientalis* of Wight. And we fear that few private *Herbaria* are now being accumulated that can compare with the magnificent collections of Wallich, Wight, Hamilton, Jacquemont, Griffith, Royle, Falconer, Strachey, or Thomson. And this apparent suspension of activity is not because material is exhausted, for there are hundreds of Indian flowering plants that remain yet unfigured, while the systematic illustration or even enumeration of Indian *Cryptogams* in a separate publication has never been attempted, except in the subdivision of Ferns, where Major Beddome has in very excellent manner broken ground by the publication of his "*Ferns of Southern India.*" Another most interesting department of Botany, in which of late the French and Germans have been pre-eminently busy, has, since Griffith died, received little attention from Indian Botanists. We refer to vegetable physiology and embryology, in which, as well as in the observation of the variations of individuals of particular species under domestication, and in varying conditions of life such as climate and soil, there are in India almost virgin fields open to any one who has the will and faculty to cultivate them. A wonderful example of what might be done in these fields is afforded by Mr. Darwin in his latest work, and the use that may be made of such observations by a skilful thinker is most happily illustrated by his wonderful hypothesis of *The Origin of Species*, of which the book just referred to contains the proof.

The late East India Company incurred the gratitude of all cultivators of Botanical Science by the munificent manner in which they encouraged both the accumulation of botanical material, and the illustration and distribution of the resulting collections. Without their aid, some of the great works which have just been enumerated could never have been published. The great desideratum for Indian Botany at present is the publication of a scientific and philosophical *Flora Indica*. This, one of the greatest of Indian Botanists, the late lamented Griffith, had set before himself as the crowning task of his life, but he died too soon even to begin it. Fourteen years ago, Doctors Hooker and Thomson issued the first volume of such a work, which, at the time of its publication, was noticed in the pages of this *Review*. Owing, however, to the ill-health of one of these distinguished Botanists, and to the pre-engagements of the other, no subsequent volumes have appeared, nor, we believe, is there any hope of any more ever appearing by the same authors,—a misfortune deeply deplored by all who are interested in Botany, either Indian or general. The completion of the *Flora Indica* in the manner in which it has been thus worthily commenced, is a work for which not only botanical talent and experience are essential requisites, but so also is the more gross element of money. The work is not one which can be crushed into a duodecimo, but would probably fill ten or a dozen goodly octavoes. But as the pursuit of either Botanical or Zoological Science does not among us lead to much worldly wealth, we fear there are few men competent for the task who are also rich enough to afford to engage in the undertaking, involving, as it would, the necessity of years of unremitting labor, with access to extensive herbaria and good botanical libraries, besides the risk (necessarily considerable) of the

commercial non-success of the book when published. Under these circumstances, it is scarcely unreasonable to expect that something might be spared from the public purse, not only for the publication, but also for the illustration of the *Flora* of an empire which yields a revenue of well-nigh fifty millions a year. The Colonial *Floras* are now in course of publication under the auspices of the Secretary of State for the Colonies, but the *Flora* of the greatest of all the British possessions remains represented, since the year 1855, by an introductory essay and half a volume of text. Without a *Flora*, the practical study of Botany by a European in India is beset by many difficulties which only a very considerable amount of enthusiasm can overcome, while to a native of the country it is next to impossible. It is true that even educated Bengalis have as yet shown little desire to acquaint themselves with either the physiology or classification of the plants of their native country. Botany forms, indeed, the subject of certain examinations in the Calcutta University course, but we have too good reason to fear that hardly one student has yet regarded it as other than a subject to be "passed in" and then forgotten for ever, the spontaneous intellectual activity of educated Bengal, where it has not direct reference to pudding, usually spending itself in metaphysics. It would be beyond the scope of this article to consider the value of the mental training likely to be derived from the practical pursuit of Botanical or Zoological studies. The subject has been sufficiently discussed of late in England, and with the result that in the English Universities and public schools completer arrangements than have heretofore prevailed are now being made for the teaching of these subjects.

In these days of competitive examinations, and of hard cramming in order to obtain places therein, a large proportion of the members of the various services land in India with a knowledge of the principles of botany sufficient to enable them to acquaint themselves with the plants around them, were the business of doing so more easy. The publication of a *Flora Indica* would speedily make it more easy; for in a few years local *Floras* would begin to be published by those more interested in the study. A few local lists, it is true, already exist scattered in the volumes of the Journal of the Asiatic Society of Bengal, and of the Proceedings of the Linnæan Society of London, but these are by far too inaccessible, and we fear too meagre, for popular purposes. We venture to say that hardly one of the large number of

Europeans and Eurasians employed in public offices and in general business in this country, knows or cares in the least about plants botanically, and that few among them know or care much even about gardening. Yet in Britain, men in like walks of life often acquire a very high measure of scientific botanical knowledge, while numbers in the pursuit of gardening while away, innocently and profitably, many hours that might otherwise be spent in vicious indulgence. It were idle to begin to prove that pursuits such as these have civilising and elevating influences, and that they are therefore worthy of all encouragement. The man who would attempt to deny this in words would hardly be listened to, however much the modern policy of cheese-paring Utilitarianism, which withholds substantial aid from the dissemination of such knowledge, may find secret applauders.

It is, we fear, not uncommon to imagine that the vegetable products of a country can be to their full extent utilised without the aid of scientific knowledge as a guide. This we utterly deny, and we maintain, on the contrary, that the truest and surest foundation of economic botany lies in pure botany. Numerous examples can of course be quoted of the utilisation of products without the guidance of science. It needs very little guidance of any sort to fell and bring to market timber that is known by experience to be valuable, or bark that has been found to possess medicinal properties, or to collect gums or dye stuffs for which there is a demand. As long as supplies of articles already known in the market last, traders will manage their utilisation, but when supplies of particular articles begin to fail, or when, for other reasons, similar substances become *desiderata*, it falls to the man of science to show how the former calamity might have been averted, and how it may be mitigated by the provision of substitutes. A scientific observer alone has the means of following up the botanical analogies which may lead to the discovery of products akin in properties to those which have already got into use. As of races of men, so of those of plants, certain properties are characteristic; but the anatomical characters denoting alliance in plants are not so evident that he who runs may read them. An ordinary trader would not recognise the handsome *Cinchona* tree which yields the specific for malarious fever, as belonging to the same family with the humble straggling *Cephælis* which yields *Ipecacuanha*, the best remedy for dysentery; nor would he see any impropriety in classing as nearly related to each other, because they have fleshy roots which in appearance are not readily distinguishable, the deadly *monk's hood*

which belongs to a family of which every Botanist knows all the members to be suspicious if not poisonous, and the *horse-radish*, which ranks with a group of which every single member is wholesome and anti-scorbutic. To many men engaged in the ordinary duties of official and mercantile life, the pursuits of pure science may seem but learned trifling, and of a nature calculated to disqualify and even incapacitate their followers for what are called practical matters, such as would come under the head of botanical economics, *e.g.*, forest conservancy. It is perfectly true that many who have worked in the abstruser departments of Botany, such as the study of the *cryptogams*, have not been men who would have taken kindly to the management of a forest division, or have entered with much zest into the question of the relative merits of different species of vegetable fibre as materials for the manufacture of cloth. These workers have their function in a different and higher sphere, and it is not proposed to insult the science to which they have chosen to devote themselves by making any apology for them. It is always unsafe to sneer at a scientific worker, because he may seem merely to be amusing himself with some curious trifle, for out of his quiet working a great discovery or invention may spring. A few years ago, Bunsen and Kirchoff might in this spirit have been described as the inventors of a new kind of kaleidoscope, but who will care to sneer now at the wondrous new mode of chemical analysis which has been developed from such a seemingly childish origin! In spite of the prejudices we have referred to, it is, we believe, not the less true, that scientific acquirements in themselves do not, in fact, tend to make one who has to deal with vegetable products a worse practical man, or in other words a worse economic botanist. We have numerous examples of the contrary in such men, for instance, as Royle, who did more for the utilisation of Indian vegetable products than any other man, and who was, as his book on Himalayan Plants shows, a thorough botanist withal.

If one thinks of the varied character of our Indian possessions in respect of soil, climate, and physical conditions generally, it becomes a matter of astonishment that the list of articles derived from the vegetable kingdom exported from them remains still so limited. There were indeed many reasons for this state of things in times past, but when it is considered how the country has been of late opened by railways, and over how much wider an area than ever heretofore peace and good government now prevail, the number of vegetable products exported has not increased as might have been expected. The axiom that "demand will create a supply" has but limited application to the trade relations of Europe and India. Cotton, jute, and such prominent articles, find a quick enough sale, but products that are little known, and especially such as are new to the home market, must be dealt with in accordance with a maxim the converse of that just quoted. Unless samples of such are persistently kept under the notice of the European merchant or consumer, and supplies are assured to him, he will rarely become a purchaser; and probably he can at first be induced to buy at all only at rates very much under real value. The capability of waiting for better results which capital gives, becomes therefore in some cases an absolute necessity. There may be reasons why private capitalists do not direct their attention to the products of a particular country; and where this is the case, it is the duty of the Government of that country to undertake to some extent their functions in respect of its undeveloped resources. The action of the New Zealand Government in respect of the flax indigenous to that colony (the produce of *Phormium tenax*) affords a good illustration of a policy which we conceive to be worthy of imitation. In 1856, we find the General Government offering "seven premiums, amounting in all to £4,000, "—the first or highest being £2,000, the second £1,000, and five "of £200 each,—to the person who shall, by some process of his "own invention, first produce from the *Phormium tenax*, or other "fibrous plant indigenous to New Zealand, one hundred tons of "merchandise,"—and we find the local Governments of Canterbury and Otago subsequently offering similar boons with like aims.

There is, indeed, a steady general demand for certain classes of raw materials in the marts of the West, though the particular *variety* may be undetermined, and it is in this indetermination that lies the opportunity for the introduction of new products. A supply of a fibre, a gum, or a dye-stuff, is a *desideratum*, but what fibres, gums, or dye-stuffs shall be chosen, may often be determined by what are offered. A new material for the manufacture of paper is a recognised want on the continent of Europe at present. Various substances have been had recourse to, and amongst other things wood shavings have been tried; a product called Esparto grass has of late come largely into use as a material for mixing, but a cheap workable fibre is still wanted. We fear, however, the question whether the paper material of the future is to be an Indian fibre,

is destined to be answered in the negative, although this country abounds in valuable and unutilised fibrous plants.

The latest contributions to the literature of the Economic Botany of India come to us from the Government of the Punjab, who ~~which~~ some little time ago, issued from their press at Lahore, under the editorship of Mr. Baden Powell, a very useful volume on Punjab Products, a large part of which is devoted to raw vegetable produce; and again, within the last few months, Dr. Lindsay Stewart's book on Punjab Plants. The scope of Dr. Stewart's volume may, perhaps, best be indicated by the following extract from his prefatory introduction. He tells us that "it comprises some notice of almost all the trees of the "Province, of most of the shrubs of some size, indigenous or "cultivated, and of the herbs, wild or cultivated, which are, or "are supposed to be, useful or hurtful, or are otherwise interesting. All of these that I have met with in the Punjab, or "that are mentioned in such books, reports and papers as "I have access to, get some notice, longer or shorter, according "to their apparent importance or interest. As a rule, with "the exception of trees of some size, but few plants are inserted "which are not considered by natives at least to be of note "in themselves or for their products, or are not cultivated as "flowers. As a rule, also, but with one or two exceptions, plants "which are cultivated only by Europeans are not inserted. "And, on the whole, I have tried to err rather on the side of "fulness than of scantiness of detail, so far as this could be done "without rendering the book too bulky."

From these sentences it is evident that the book in no way professes to be one by which a person, having a Punjab plant of which he knew nothing put into his hands, could, *secundum artem botanicam*, find out its affinities and name; in other words, it is not a *Flora*, although it would form a most admirable complement to one. The plants which it enumerates are arranged in accordance with the place they take in De Candolle's natural system, beginning with Ranunculaceæ, and ending with Lichens. The most modern or best known botanical name is first given, and, as ought always to be the case in such enumerations, the botanical authority for that name is indicated. All botanical synonyms are, however, as a rule, omitted, which, did the book profess to be a *Flora*, would be a grave fault. After the classical name, are given all the vernacular names known to the author. But, as vernacular nomenclature is one of the features of his book, it may be as well to let

Dr. Stewart describe it for himself. In his introduction he writes as follows :—

"Besides ordinary Punjabi and Hindustani names inserted, "the chief linguistic or dialectic varieties of which examples "occur are the following. Some Persian names are applied to "drugs, or are used in Affghanistan. The Pushtú names include "those in use in that country, and those employed in our "Trans-Indus territory and the Súlimán Range, &c. Numerous "Kashmír and Ladáki (Tibetan) names are given, and a small "number of Sind and Beluchistan. A few Arabic and still "fewer Greek terms are entered as applied to drugs, the "latter having filtered through the Arabian physicians and "*hakíms* to the Indian Bázárs, where they are not always "very recognizable. Many of the Lahouli names, included with "those of the Chenâb basin, belong to a branch of the Tibetan "language, as do those of Spiti."

To its native names, there is annexed for each plant a paragraph giving an account of its geographical distribution in the Province and on its confines, its season of flowering and uses, with other particulars of more or less value and interest. It might, however, in some cases have been useful had a few remarks descriptive of the appearance of the plant been made.

The descriptive paragraphs abound in evidences of the closeness of Dr. Stewart's observation, of the extent of his travels in Upper India and in the Western Himalaya, and of his untiring industry. As a specimen, we extract the following on the *Populus Euphratica*, a tree not uncommon in Western Asia :—

"This tree, which grows on the Jordan, Tigris and Euphrates is common wild in Sind, and in the Southern "Punjab in the low land near rivers. I have seen trees "of it as high as Dera Ishmail Khan, and on the Indus "it is said to be found occasionally in nooks up to Attock. Far "above that on the Indus river or its tributaries, it is found in "parts of Tibet (western) to 10,500 feet; and Aitchison men-"tions it in his "Lahoul List," but this specimen may have been a "Tibetan one, of which there appears to have been a few in the "collection. In the Southern Punjab (where planted specimens "occur in Multan, &c.), the tree grows to no great size, specimens "of five-feet girth not being common; but this may partly depend "on the excessive lopping to which it is subjected to provide "fodder for goats. In Sind, where it is better cared for, trees of "seven or eight-feet girth are not uncommon.

"The leaves vary in shape to a considerable extent, especially in "the plains, some being quite narrow, long, lanceolate, entire, and "knife-like, and others excessively broad with a comb-like edge. "The leaves of the Ladak trees vary much less. Thomson's "statement that the narrow leaves are found on young plants "and pollarded shoots, and the broad ones on old trees, is, "to a considerable extent, correct. These and intermediate "varieties occur on both male and female trees, the latter being "more common, so far as I have observed, in the Punjab "plains. In places where the tree is subject to inundations, "it is sometimes covered with short, horn-like roots to eighteen "inches from the ground. (I have seen a similar growth on "willows in like circumstances in Kashmir.) From the wood of "the tree on parts of the trunk, short spines project into the "inner part of the bark. The wood is generally white, soft, and "toughish, and, when unseasoned, is very subject to the attacks "of white ants. But in old trees there is usually a large pro-"portion of very dark, strong heart-wood. In the Southern "Punjab the timber is for the most part only used for wells, &c., "but in Sind it is largely employed for beams, &c. (not for "planks), and in turnery. In Sind also the smaller trees are "cut as coppice, and speedily spring again to furnish a fresh "crop of rafters. The wood being white (and so not flesh colour), "is preferred for constructive purposes by Hindus, and for the "same reason the twigs are used by them as tooth-sticks. The "wood is rarely used for boats in Sind, but is said to be largely "so employed on the Euphrates, &c. It is also employed for "fuel in the south (in part even for steamers, although from its "lightness it is not very suitable), and in parts of Tibet, where "it grows, it furnishes much fire-wood. In Sind the bark is "given as a vermifuge, and the liber is employed as a gun-"match."

The indices, three in number, appended to the volume are admirable, and the very varied information which it contains, is by their means made accessible to a reader having even the slightest clue to what he wants to find out. If he has picked up the botanical or English name of a plant, the enquirer can, by turning to the botanical and English index, discover all that Dr. Stewart has to tell him about its native names, uses, &c.; has he heard the native name, a reference to the vernacular index will put him in possession of one or all of its botanical equivalents; or is he desirous of knowing what vegetable products of the Punjab are capable of being, or have been, put to any particular use, a reference to the third index of "uses," where Dr. Stewart gives a synopsis of his book on a different basis than that of nomenclature, will guide him to the information of which he is in search. For example, under the head *Dyeing* are enumerated no less than forty-two plants, parts or preparations of which are used in that art, and after the name of each is given the number of the page where it is treated of. Under the heading *External* follow the names of seventeen species yielding preparations which are applied to the surface of the human body, medicinally or otherwise. It is needless to multiply examples. For the purposes of general consultation, the book is far more of a model than either Major Drury's *Useful Plants of India* or Balfour's *Cyclopædia*, valuable as both these works are.

Having in view the plan indicated by Dr. Stewart in the extract from his volume first made, we must congratulate him on the admirable way in which he has carried it out, and at the same time assure him that information conveyed in such a very workman-like manner, cannot fail to be widely appreciated and to become highly useful. The aspiring Deputy Commissioner, ambitious of garnishing a report with a few botanical names; the enquiring medical officer, desirous of extending his knowledge of bazar medicines; and the seeker after plant-lore from whatever motive, will, we feel sure, alike apply to this volume as a manual for the Punjab and indeed for Upper India.

Under the designation of "Minor Forest Products," a variety of gums, resins, dye-stuffs and medicines are annually collected in the Government Forests, a small annual revenue being paid to the Forest Department for the permission to do so. Amongst these are doubtless many substances that would be valuable in the arts, were they introduced into Europe. Dr. Stewart enumerates such as are collected in the Punjab; but in the more tropical forests of Bengal and the south of India, they are doubtless more numerous and valuable. Some of them have already gained a footing in the home market, but we are convinced that by a little attention the quality of such could be improved, and that many quite new ones might be introduced. Indian gums, for example, bring a small price at home, compared to those derived from other Eastern sources, the reasons chiefly being that the former are unequal in quality, and impure.

Many of these minor forest products are medicinal, and on that account are well deserving of further attention. The

"Pharmacopœia of India," which stands second in the list of books at the head of this article, does not, as might be imagined, consist of an enumeration of indigenous Indian medicines, but is a reprint of the British Pharmacopœia, with the addition of a certain number of Indian substances, chiefly vegetable, which are now formally recognised as officinal, together with rather copious lists and descriptions of non-officinal Indian medicines, which in some cases may be used as substitutes for the former, but which, as regards European practice, cannot be considered as more than on their trial. This, the newest contribution to Pharmaceutical Technology, however, departs entirely from the custom of Pharmacopœias, which is merely to enumerate and give the physicial characters of drugs, inasmuch as it supplies information regarding their medical properties, therapeutic uses, and mode of administration. The book thus more resembles a manual of Materia Medica than a Pharmacopœia, and in our opinion, becomes more useful on that account. It was undertaken by direction of the Secretary of State for India, and the work of preparation having been deputed by him to a Committee, consisting, with one exception, of Indian medical officers distinguished for their interest in the Indian *Materia Medica*, the combined result of their labors has finally been printed under the very competent editorship of Dr. Waring of the Madras Army. In carrying on their work, the Committee seem to have availed themselves of a good deal of help external to themselves, for in their preface they render acknowledgments to upwards of fifty gentlemen, mostly medical officers now in India, from whom they received reports.

In as far as this Indian Pharmacopœia is a reprint of the British, it would be out of place to criticise it here; we shall, however, venture to make a few remarks on that part which treats of articles officinal in it which are not contained in the British, and on that much larger section treating of medicines in daily use among the natives of this country, which remain still non-officinal in European practice in India.

With regard to the first of these two classes, the notable circumstance that first strikes us is their limited number. There are only forty. If to these be added the officinal Indian plants contained in the British Pharmacopœia, we find that preparations of only sixty-two plants and two animals are contributed by India to the recognised Materia Medica of her Anglo-Saxon rulers. And these sixty-two plants are not all indigenous to the country, though supplies of their products are derived from Indian bazars, assafœtida, for instance, being grown beyond the frontier.

Many medicines in common use in European practice owe their introduction to early voyagers, who brought home some of the more famous remedies used in the countries they visited. Originally a good deal influenced by a fanciful regard for things far-fetched, both patients and prescribers have come to pin their faith to many remedies which are probably no better than some that could be got nearer home. The reputation of others again has been handed down from a remote antiquity. We believe we are right in saying that no department of medical enquiry has been more neglected than the accurate appreciation of the action of medicines on the human body in health and disease. Without, however, pretending or attempting to enquire into the solidity of the basis on which the reputation of particular medicines rests, we would merely remark that whereas supplies of them are at present imported into England from all parts of the world, we have surely every facility for growing many of them in our Indian possessions, extending, as these do, from near the equator to the thirty-fifth degree of north latitude, and, if Kurrachee and Singapore be taken as extreme points, stretching over about as many degrees of longitude, and embracing within these limits almost every imaginable physical condition affecting plant-life. Supplies for the use of the army, jails and dispensaries in India might at any rate be grown in the country, instead of being, as at present, imported at great expense from Europe. The splendid success of Government in the introduction of the quinine-yielding species of Cinchona ought to be an incitement to the trials of other medicinal plants. Were the few Botanical Gardens that at present exist in this country supplemented by medicinal gardens, and were one or two new ones established, we see no reason why Ipecacuanha, Belladonna, Aloes, Jalap, Digitalis, Podophyllum, Quassia and other bitters, Logwood, Dandelion, Scammony, Mint, Lavender, and the species of Umbelliferæ of which the seeds yield volatile oils, should not be grown in India. The officinal Rhubarb grows on the other side of the Himalayas, and if tried on this side would probably do well; Squill might be grown on the sea-coast; and Colchicum would probably thrive in the Punjab. Dandelion and Senna used to be supplied to the medical department from the Botanical Garden at Seharunpore. Hyoscyamus of excellent quality is supplied still, and so doubtless could Belladonna were it tried.

Every one admits the immense amount of good effected by our dispensaries in India. This might, we are convinced, be indefinitely extended, were a larger supply of European medicines allowed for each. At present the orders, we believe, are that the consumption of these be as restricted as possible, and medical officers are directed to make use of bazar medicines as far as they can. Now, however much a native may value bazar medicines when prescribed by his own hakíms, he expects to get something else at a dispensary, and is disappointed if he does not. He is often sharp enough to find out when bazar medicine has been given to him, and obstinate enough not to use it.

The distrust of bazar medicine thus shown, whatever be the motive for it, is, we are convinced, well warranted by facts. In many cases bazar medicines are simple trash. Let any one only look at the system of storage followed in a *pansáris** shop, and *one* very evident reason of this will become apparent. His wares are of all degrees of staleness, the stock of many of them inherited from his father or grandfather, and long ago inert. Stoppered bottles are things unknown, and all substances are alike stowed in bags or earthen vessels, exposed to every variation of the atmosphere in respect of heat and moisture, and to the attacks of every kind of insect. All are more or less mixed with shop-sweepings, dust, and foreign matter of various sorts. Many are adulterated, and, as a matter of course, none are labelled. The vendor is often utterly ignorant of even the names of the contents of the bags that are stowed away in the remote corners of his shop, and when questioned, can answer only by guessing. Many of the medicines, even when fresh, do not possess any therapeutic properties whatever, and the really valuable ones are of too uncertain age and strength to be relied upon. The saving in money effected by supplying dispensaries from such sources as these is not very great, while the loss in efficiency and confidence is enormous.

Notwithstanding what we have just said as to the value of bazar medicines as at present supplied, we are fully convinced that amongst them are remedies of great potency, which might, with advantage, be substituted for many that are in vogue in Europe; and that the value of all of them that possess curative properties would be very greatly increased, were proper care taken in their collection, preparation and storage.

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* A *pansári* is a native druggist.

Experimental therapeutic enquiries, even when conducted with all the facilities afforded by large hospitals in Europe, and on patients possessing some degree of intelligence and docility, are attended by great practical difficulties, and make large demands on the patience and perseverance of the experimenter. How much more difficult must the prosecution of like enquires be in this country with the slight facilities afforded in Indian dispensaries, where but few of the patients (and these often chronic invalids) are inmates, and where the out-door patients are exposed to the influences and advices of Brahmans, fakirs, native practitioners, and ignorant relatives, who in a hundred ways prevent the doctor's orders from being followed, or his medicine from being swallowed at all, unless perhaps concomitantly with some farrago of their own concoction. Add to this, the exhausting effect of the climate on their mental energies, the small opportunity and the comparatively unsettled life of Indian medical officers, and it is not to be wondered that so li*tle comparatively has been done by them towards an accurate appreciation of the therapeutic value of the thousand-and-one substances known as bazar medicines. In a general way, not a little has been recorded of certain remedies, and perhaps enough to mark the particular ones to which attention should be directed, and to warrant the appointmemt of medical officers to the sole duty of conducting exhaustive enquiries as to their chemistry and therapeutics, with a view to their addition to the *Materia Medica* if found worthy. Experiments would also be useful which would settle in a definite way, once and for ever, the claims to be made officinal of certain drugs that now retain a doubtful reputation.

The sections which are devoted to these irregular though common remedies, constitute in our opinion by far the most valuable part of the new Indian Pharmacopœia. In dealing with these substances, the Committee note most of the properties currently ascribed to them, indicating also authorities, and where these are not traditional merely, giving references to them. In the matter of vernacular nomenclature, however, the Committee would have done well to have taken a leaf out of Dr. Lindsay Stewart's book. In their preface they explain that "amongst the returns received from India, was one from "Native Surgeon Moodeen Sheriff, of Madras, containing the "vernacular names of indigenous plants and drugs, in twelve of "the native languages of India, a work of immense labor, "reflecting the greatest credit on the intelligence and industry

"of the compiler. This catalogue having been submitted to "eminent Oriental scholars at home, and pronounced generally "correct, it was resolved to append it to the Pharmacopœia. It "was accordingly forwarded to Madras, for the purpose of being "printed under Mr. Moodeen Sheriff's superintendence. Un-"expected circumstances, however, having arisen there to delay "its publication, it has been deemed advisable, rather than to "defer the publication of this work, to issue the catalogue in a "separate or supplementary volume."

It is, we think, very much to be regretted that this course has been followed. Many of these non-officinal remedies, the introduction of which to regular practice is avowedly one of the objects of the publication of this Pharmacopœia, are dismissed without a single vernacular name for them being given. The recommendation, for example, of the Committee, that *Hymenodictyon excelsum* should be looked to as likely to prove a valuable specific for malarious fevers, is pretty certain to be quite thrown away on a medical officer who is not an expert in botany, for not a single native name for this tree is given either in the book itself or in its index, and though it might happen to grow in forests round his station, the Committee put him in possession of no means of recognising it. The native names of even such widely-distributed Indian trees as *Butea frondosa* and *Emblica officinalis*, not to mention many others equally common, are omitted, though they must have been well known to the Committee. This very grave defect in the Pharmacopœia cannot be removed by the publication of a separate catalogue of native names, as proposed. In a second edition we hope to see not only a full vernacular index, but to find, following the botanical name of each substance, as complete a list as possible of the vernacular synonyms for it which are current in all the three Presidencies.

We have as yet said little or nothing in support of the third statement contained in the opening sentence of this article, to wit, that Europeans, notwithstanding their long possession of India, have as yet done little in the way of adding to the vegetable products of the country. As introduced field-crops, potatoes and oats may be pointed to, and Cinchona may be mentioned with just pride; but in gardens, as might be expected, are to be met with our most numerous achievements in the way of acclimatization. They are not much to boast of, for, compared to a good English garden, the finest flower *parterres* to be seen in India have rather a poor and mean appearance; while European vegetables, though raised from the best seed, are lacking both in substance and in flavour. We quite agree with Mr. Firminger when he says that "under "the most favorable point of view, it can hardly be said that "horticulture has as yet made much advancement in India." The reasons for this are very obvious. Gardening is an art almost utterly neglected by the natives. Men of birth or money consider it quite beneath their dignity to take any greater practical interest in it than they do in agriculture. Fruit, indeed, they are fond of, but they are too supine to try to improve its quality and flavour. In their selection of flowers, considerations of beauty have no influence with natives. For them, the prime recommendation of a plant is that its flowers have a sweet smell, the second that they are of gaudy and distinct colours. Delicacy of shading, gradation of tint and grace of form, are unappreciated; and beauty of foliage and habit are still more utterly so. The common customs of gathering only the blossoms of plants for nosegays, and of stringing the corollas on pieces of thread like beads, show how little they appreciate floral beauty. Landscape gardening is unappreciated, nay unknown among them. Can it be wondered at that their gardens present the stiff, formal, unenticing appearance they do! A number of raised walks that are intended to be straight, running at right angles to each other, and all shaded by double rows of straggling, unpruned, orange or other fruit trees; a series of deep, damp, four-sided spaces marked off by the intersections of the walks, and in which straggling crops of country vegetables have been sown in irregular rhomboidal patches intended for squares; an irregular grove of mangoes, guavas, or pomegranates; a corner or two sacred to Tulsi, Jasmine, French marigolds, and various other honoured herbs, and a good many more such to obstinate weeds whose roots it is too much trouble to dig out; an untidy well, and perhaps a *chutree* or two; some tumble-down malis' houses, and a bullock-shed;—such are a few of the chief appearances that strike one in a native garden. Professional Indian gardeners or malis are usually ignorant in the extreme. They have little more education than coolies, and are often quite as lazy and careless. A few of them can graft and bud, but not one in a thousand can prune. Of the simplest principles of gardening they are ignorant. They know nothing, even empirically, about the necessities of cultivation in different kinds of soil. Rotation of crops and change of seed are practices of which they have not even yet discovered the advantage; and the skilful application

of manure is an art almost unknown. Malis cultivating on their own account turn their attention, as a matter of course, chiefly to vegetables; fruit being generally supposed to require no cultivation. Every one must be familiar with the flavourless melons and half-swelled grapes which appear on his breakfast, table during the hot season, and with the appearance at least of the small but amazingly odoriferous mangoes and guavas, which scent the morning air during a drive through a bazar in the early part of the rains, and also with the wonderful variety of insipid gelatinous messes, of varying degrees of slipperiness and unpalatability, that are put before him by his *Khansamah* during that season, with the assurance that they are country vegetables, and that no others are obtainable.

Most Europeans in India, probably because they had no leisure to make themselves ~~practically~~ acquainted with its details before leaving home, know nothing practically about gardening. The rigour of the climate, the press of official duty, but most of all the uncertainty of remaining long enough in one place to reap the full advantage of any labour expended, prevent many a man who is really fond of flowers from devoting any attention to his garden. The *mali* in whom such a Sahib puts his trust, is master of the situation, and he does his worst. He hates novelties and innovations, and especially in the shape of those troublesome *Wilayati tarkári*. The freshest and best imported seeds may be made over to him, but the chances are they don't germinate. The Sahib wonders why this should be so, and thinks bitter things of his seedsman. Had he seen the thorough drenching with water to which the seeds were probably subjected immediately after the sowings were completed, the seedsman's character would have been saved. Perhaps he has insisted on manure being given to his vegetables, and when the fine long carrots that he was led to hope for from the descriptive labels on the packets of seed, are represented in reality by squat truncate-looking abortions, he again wonders, and once more blames the seed or the climate. But had he seen that the manure he was so particular about getting, instead of being well dug into the ground so that the growing vegetable should pierce downward in search of it, had been merely scratched in, so as to remain an inch or two below the surface, he might have anticipated the peculiar form of the naturally spindle-shaped esculents.

As Mr. Firminger well remarks:—"No one should allow him-"self to suppose that he can have a well-kept, well-cultivated "garden without being to a considerable extent his own head-"gardener." To enable most people to become their own head-gardeners, such a manual as that of which Mr. Firminger has just brought out the second edition, is absolutely necessary. To meet, however, the want of knowledge which we have just indicated, such a book should treat, and treat at length, of the first principles of gardening, as well as of all its practical details.

Mr. Firminger's book consists of two parts: in the first of these, which is devoted to the "operations of gardening," he discusses, but, in our opinion, far too briefly, such matters as climate, soil, manure, the laying out of a garden, seeds and sowing, propagation, pruning, &c. &c. The second, and by far the most bulky part, treats of "garden plants," and gives short specific descriptions of those enumerated, with directions for their treatment. If Mr. Firminger intended his book as a complete manual of Indian gardening, he would have done well, had he extended his chapters on the general principles of horticulture so as to have made reference to any of the standard British works on the subject unnecessary. Pruning is an art most difficult to teach, and equally difficult to acquire except by practice; and although we admit that but little about it can be imparted by a book, we think Mr. Firminger might have spared more than two rather sparsely printed pages to its discussion; and concerning soils, we are sure he must have more to tell us than the few meagre facts which he has set down in the page and a half which he has devoted to a subject so important. Mr. Firminger has bestowed his chief care upon the second part of his book, and he has there given a very full enumeration indeed of the plants usually met with in Indian gardens, besides mentioning many that one does not often see. The notices of these are very likely indeed to be useful. Some specialities, such as orchids and caladiums, are but slightly treated of; and we confess we are rather disappointed with the chapters on roses and vines. Roses are plants of such universal cultivation in the gardens of Europeans, that fuller details as to the treatment of the various fine English and French sorts would, we are sure, have been most acceptable. Particulars are especially wanted as to the best mode of propagating *each* kind, whether by layers, cuttings, or buddings, and if by the latter mode, as to the best stock. Budding can be very successfully practised with almost all kinds in the Upper Provinces, but budded roses obviously require much more careful looking to than those that have been reared from

cuttings, and Mr. Firminger's experience on striking cuttings would have been most welcome. For his book, as a whole, however, he deserves the warmest thanks of all who are interested in gardening in India, and we are sure it will be gratefully referred to by many an amateur. His directions for cultivation are more particularly applicable to Bengal, but there is much in them that will be useful in any part of India, and we have much pleasure in recommending his book as the newest and best, treating specially of gardening in this country.

Before concluding, we cannot forbear from referring to the excellent work that has been done for Indian horticulture by the Government Botanic Gardens at Calcutta and Saharunpur. From the latter, the distribution of all kinds of trees and smaller plants, besides seeds,—until lately quite gratuitous—has for many years been very extensive. From the Superintendent's Report for the year 1865-66, we learn that during the preceding twelve months no fewer than 92,772 living plants had been distributed. The different Agri-Horticultural Associations that have of late sprung up in various parts of the country have also done, and are still doing, a great deal of good in the way of disseminating seeds of English flowers and vegetables.

Much, however, remains to be done even for the gardens of the *Sahiblog*, and almost everything for those of natives, the poorer classes of whom are too ignorant to profit by any efforts that are not particularly directed towards them. The necessity for improving Indian horticulture and agriculture, has begun to attract attention at home, and the present Secretary of State and Governor-General, themselves skilled in agriculture, are understood to be much interested in the matter. We venture to express a hope that one of the first results of this awakening interest will be the establishment of schools for the instruction of natives in both farming and gardening, the very elements of which are quite unknown to the mass of the cultivators of the soil, whether Hindu or Mahomedan.

from The Calcutta Review
Octr [illegible]

With the author's compliments

Reprinted from the Dublin Quarterly Journal of Medical Science, August, 1869.

ART. IX.—*Notes on "Mean Temperature in its Relations to Disease and Mortality,"*[a] *with Special Reference to the City of Dublin.* By JOHN WILLIAM MOORE, M.B., Ch. M., Univ. Dub.; ex-Sch., Trin. Coll. Dub.

IN the seventeenth number of the Journal of the Scottish Meteorological Society (New Series) is to be found an abstract of a

[a] The subject of which this paper treats has long since attracted attention. So far back as 1797, Dr. William Heberden, jun., F.R.S., published, in the 86th volume of the Philosophical Transactions of the Royal Society, a communication on the "Influence of Cold on the Health of the Inhabitants of London." The results arrived at seventy years ago are substantially the same as those obtained from the more accurate sources of information which are at present within our reach.

valuable paper by Dr. Arthur Mitchell, of Edinburgh, which was read before that association in the month of January, 1868. The subject of the communication to which I refer is: "Are exceptions in the distribution of temperature associated with exceptions in the distribution of disease and death?"

To this momentous question the author is enabled—so far, at least, as he has carried his inquiries—to give an affirmative answer; and in his paper he proves conclusively the connexion between low winter temperatures and excessive disease and mortality, referable to the respiratory organs.

Having recognized the importance of the subject, and having become interested in its further elucidation, I was led to apply, in the case of Dublin, the conclusions arrived at from an attentive study of the reports of the Registrars-General of England and Scotland. And this with so satisfactory a result that I felt encouraged briefly to bring forward the subject in the present paper.

In my investigations I have—so far as relates to *mortality*—depended entirely on the elaborate tables published weekly, quarterly, and yearly by the Irish "General Register Office." For the meteorological observations taken at the Ordnance Survey Office, Phœnix Park, I have, however, substituted those recorded by myself in the city—for this reason, that the temperature of the registration districts of Dublin is, perhaps, better represented by observations made in the city than by those taken at a considerable distance from it.

Respecting, then, the relations which exist between mean temperature on the one hand, and disease and mortality on the other, the conclusions which have been drawn fall naturally within the limits of the two following propositions:—

I.—*In* Summer *the tendency to morbility and death has reference to the digestive organs—diarrhea and dysentery being the affections which are especially prevalent and fatal during this season. In* Winter *a similar tendency is noticed in connexion with the organs of respiration—bronchitis, pneumonia, and pleuritis, being the affections which are principally met with at this season.*[*]

II.—*In* Summer *a rise of mean temperature above the average increases the number of cases of, and the mortality from*, abdominal

[*] Cholera and phthisis are omitted in the above classification as being in their nature *constitutional* rather than *local* affections, though the prevalence of both these diseases is much influenced by seasonal variations of temperature.

disease. In Winter *a fall of mean temperature below the average swells the numbers of cases of* thoracic *disease, and increases the mortality therefrom.*

The applicability of these propositions to Dublin may best be illustrated by a series of curves, representing the mean temperature, total mortality, deaths from thoracic disease, and deaths from abdominal disease respectively.

I have in diagram I. projected curves of this description for wo years, having divided each year into quarters, the first of which, embracing the months of January, February, and March, may be considered the winter season; and the third of which, including July, August, and September, may represent the summer season.

In the case of both these years (1867 and 1868) we accordingly perceive that the curve of the thoracic death-rate reaches a maximum in the first, or winter quarter; whereas that of the abdominal mortality attains its greatest height in the third, or summer quarter.

The same will appear, perhaps more strikingly, from diagrams II. and III., which represent *weekly* curves of a similar nature for periods of six months each.

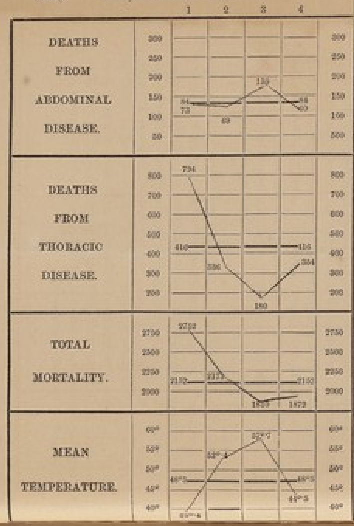
But a comparison of the corresponding curves of any two years will show that *in steepness* such curves are widely different, and this leads us to an investigation of the second proposition, which refers to the effect produced in the death-rate by *abnormal* mean temperature.

As before, we will take first the diagram which represents the quarterly returns, and afterwards those relating to periods of twenty-six weeks.

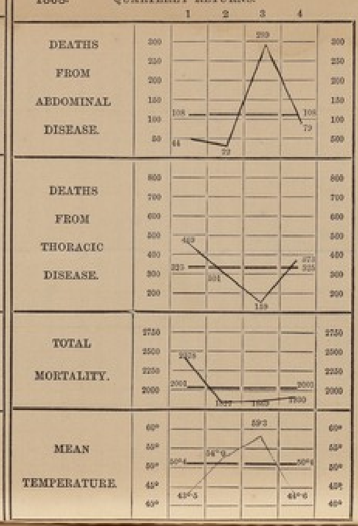
The *average* annual temperature of Dublin is 49·5°; the average temperature of the first quarter is 41·0°, that of the third is 58·7°.

It appears that in 1867 and 1868 there existed discrepancies between the mean and average temperatures, both quarterly and yearly. Thus while the mean temperature of 1867 was 1° below the average, that of 1868 was nearly 1° above it. And, while in 1867 the mean temperatures of the first and third quarters were respectively 1·6° and 1·0° below the average; in 1868, on the other hand, the mean temperatures of the corresponding quarters were 2·5° and ·7° respectively above it.

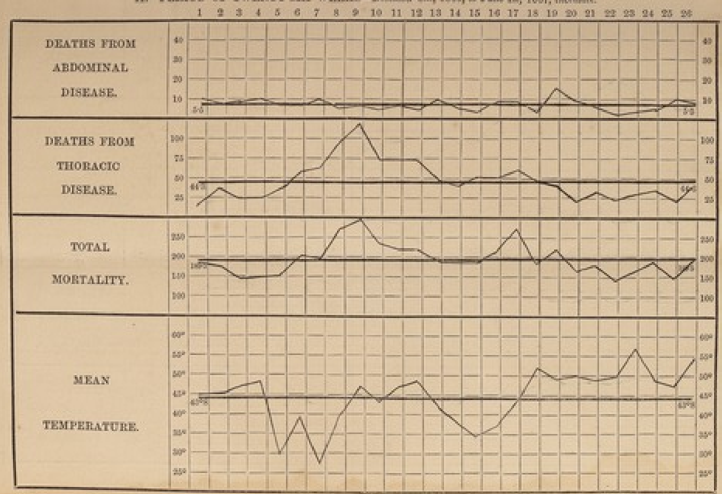
1867. I.—QUARTERLY RETURNS.



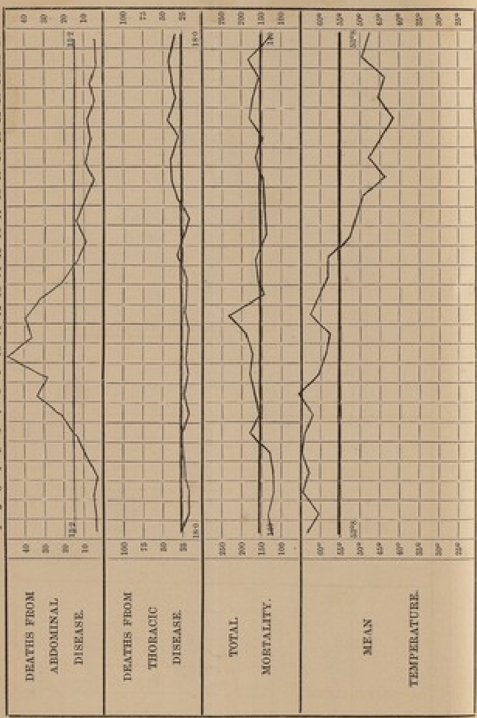
1868. QUARTERLY RETURNS.



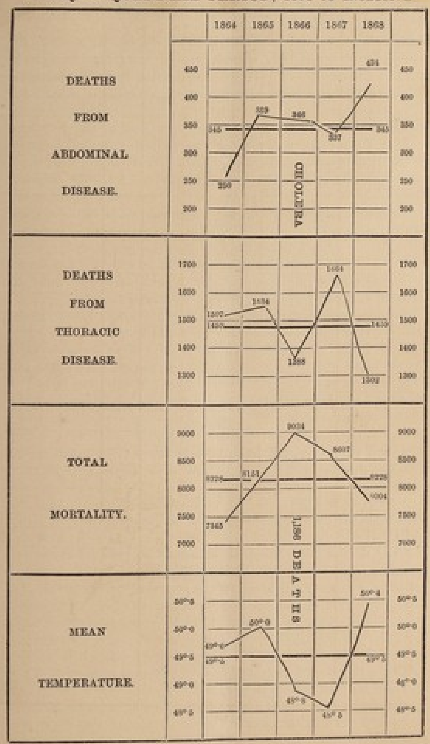
II.—PERIOD OF TWENTY-SIX WEEKS—*December 8th, 1866, to June 1st, 1867, inclusive.*



III.—PERIOD OF TWENTY-SIX WEEKS—*June 20th, 1863, to December 12th, 1863, inclusive.*



IV.—QUINQUENNIAL PERIOD, 1864–68 INCLUSIVE



Now, if the proposition which is under consideration stand, the thoracic mortality of 1867 should be high, more particularly so in the first quarter of that year; whereas the fatality due to abdominal affections should, in 1868, be well-marked, especially in the third or summer quarter.

And this is really the case, for it appears that in the first three months of 1867, from thoracic disease there occurred 794 deaths, or nearly half of the entire number (1,664) referred to that cause throughout the year; while in the like period of 1868 only 469 deaths happened from affections of the respiratory organs, being rather more than one-third of the whole number (1,302) occurring in the year.

So in 1867, of 337 deaths from diarrhea and dysentery registered 135 took place in the third quarter; while in 1868, of 434 deaths from these same diseases, 289, or considerably over one-half, occurred in the corresponding or summer quarter.

Turning now to the tables of weekly curves we recognize the same striking results.—(Diagrams II. & III.)

Thus, in the week ending August 22nd, 1868, a fortnight after the week of highest mean temperature of that summer which was so tropical in character, the fatality from diarrhea was excessive. The Registrar-General's note on the subject is as follows:—"The number of deaths from diarrhea *registered* during the week amounted to 49, showing an increase of 23 on the number registered during the week preceding, and *being* 35 *more* than the average deaths from this disease in the corresponding week of the four previous years."

Again, in the report for the week ending Jan. 26th, 1867, we come upon the following note in the "Weekly Return of Births and Deaths":—

"The deaths from bronchitis amounted to 80. The increased mortality from this disease is due to the extreme cold which prevailed; the mean temperature for the first three weeks of this year was 29·8°. During that period the thermometer fell to 2·8° on the 3rd instant, to 19·2° on the 12th, to 12·5° on the 16th; whereas in the corresponding three weeks of last year the mean temperature was 43·0°, and the deaths from bronchitis were only 31."

To the report of the ensuing week this note is annexed:—"The effects of the cold weather on pulmonary affections is again evidenced by the increased number (102) of deaths from bronchitis registered, being 22 more than the number registered during the week preceding."

We have so far succeeded in tracing cause and effect in the relations between mean temperature and mortality, as regards seasons and weeks. Do like relations hold in respect to years?

From a glance at diagram IV., in which are projected curves for a period of five years, from 1864 to 1868 inclusive, an affirmative answer may be given to this question, though here we experience some difficulties, in their nature more apparent than real.

Two of the five years, viz., 1864 and 1866, seem at first sight not to bear out our conclusions. Thus, while in 1864 the mean temperature was slightly above the average, yet the death-rate from abdominal causes was—contrary to expectation—below the average; that from thoracic affections having been a little above it.

Two reasons for these apparent contradictions of our theory may with justice be assigned.

First—If we analyse the elements of which the mean temperature of the year under review is made up, we shall find that though the mean temperature of the whole year was a little over the average, yet that of the first quarter was 1·5°, and that of the third quarter 0·3° below it. July indeed was warm, but August was unusually cold. In my record of the weather of this month, I find notes such as these:—"Cold northerly winds prevailed from the 8th to the 26th;" "*hail* fell on two days;" "on the 22nd the thermometer fell to 41°." September, though warm, was damp and overcast, with occasional storms from the S.W.

We may infer that before the effect of the returning heat could be felt, the falling temperature of the advanced season interposed, and prevented the occurrence of a high mortality from diarrhea.

To the great depression, 1·5°, of the mean temperature of the first three months of the same year we are to attribute the high thoracic death-rate. In addition to this, in the second place, we must bear in mind that this year was the first of registration in Ireland, and we can well understand that it took some time to set so extensive an organization as that of the registration system in working order.

With respect to 1866, two causes again combined to produce apparent anomalies in the death-rate. First, the low mean temperature of the year is to be referred rather to the coolness of the summer than to any great severity of the winter. In fact the mean temperature of the first three months was 41·3°, which is above the average; while the mean temperature of the third quarter was 56·3°, or upwards of 2° below the average. So, as was *à priori* probable, the mortality from thoracic disease was lessened.

Secondly,—To the prevalence of a terrible epidemic of cholera rather than to excessive summer heat is to be ascribed the high mortality from diarrhea and dysentery which characterized the year.

A third year, 1865, shows an anomaly in connexion with thoracic, while the high mortality from abdominal disease coincides with the high mean temperature of the summer. All discrepancy vanishes, however, when we learn that the mean temperature of the winter quarter in this year was nearly 2° below the average, January showing a depression in its mean of 3°.

In illustration of the circumstances which must, in cases of this kind, be taken into account, if we would work so as to draw reliable and trustworthy conclusions, the following extract from the *Times* of Friday, May 7th, 1869, will not be out of place:—

"The Registrar-General (of England) has to report that the state of the public health of the country, during the first or winter quarter of the present year, was not so good as in the winter of 1868. The season under review was warm, the mean temperature being nearly 3° above the average; but in addition to an epidemic of scarlet fever, trying and exceptional climatic conditions prevailed. The weather, which in the beginning of January was very warm, suddenly turned cold for about a week, and swelled the number of deaths from bronchitis and pulmonary diseases. Then a warm period set in, and lasted until the beginning of March, when there was another change to wintry and ungenial weather, which continued up to the end of the quarter, and cut off many of the very young, the weakly, and the aged." [a]

This extract shows well how careful we must be to inquire into details when drawing conclusions from seasons or years, for the Protean character of our climate, to which we may fitly apply the words "varium et mutabile semper," necessitates the attentive consideration of short periods of time.

About the years 1867, 1868, little need be said, for from the diagram the great effects of temperature on the different curves of mortality are at once evident. These effects may also be demonstrated by a statement of the respective ratios of deaths resulting from abdominal and thoracic causes to the total mortality in these

[a] In the first quarter of 1868, the mean temperature being 41·4°, 120,095 deaths were registered in England. In the same quarter of 1869, the temperature of which was nearly the same—viz., 41·3°, the deaths amounted to 133,437. The increase is due principally to the variable character of the weather in the latter year.

same two years. Thus, in 1867 there took place from abdominal affections 337 deaths, or a ratio of 1 in every 25·6 of the total number of deaths. In 1868 there resulted from abdominal causes 434 fatal cases, or a ratio of 1 in every 18·5 of the entire deaths registered. In 1867 no less than 1664 deaths from diseases of the respiratory organs took place, or a ratio of 1 in every 5·2 deaths. In 1868 1,302 fatal cases of thoracic disease were registered, or a ratio of 1 in every 6·1 of all the deaths.

It will be remembered that 1867 was the year of the *cold* winter, while 1868 was that of the *hot* summer.

Hitherto, it will be observed, I have spoken of mortality only. The influence of changes in mean temperature on morbility is no less strikingly manifested. In fact, all that has gone before applies to disease equally as to its sequel—death—the sole difference in the two cases being that the occurrence of disease is more speedily the result of changing temperatures than is the occurrence of death. Also a much more remarkable effect as regards *numbers* is apparent in the case of disease.

At this point I shall have to enlarge the borders of our inquiries, and by inference apply to Dublin such facts as have been attained by diligent investigations made elsewhere.

It is much to be deplored that, in the United Kingdom, there does not at present exist an official registration system in connexion with morbility; that, while statistics relating to mortality are elaborated with assiduous care, the even more important subject of the prevalence of disease receives little or no attention. In this we are surely far behind most of our continental neighbours.

Not many weeks since there appeared in the columns of a leading medical journal[a] an article on this subject; and we must hope that a new state of things will shortly be inaugurated, when not only will the importance of the registration of disease be generally recognized, but an organization for carrying it out will be set working.

Even now isolated statistics of morbility exist in different parts of the country, and the value attached to these should assuredly gain for the advocates of an extended system of disease registration an attentive hearing. Among such statistics those recorded from week to week in Manchester may be cited. One example

[a] Medical Times and Gazette, May 15, 1869.

drawn from this source will prove—conclusively I think—that abnormally low mean temperatures exercise a remarkable effect on disease connected with the organs of respiration. For the numbers following I am indebted to Table II. in Dr. Mitchell's paper, of which mention has already been made.

It will be remembered that I selected the period commencing December 8th, 1866, as illustrative of the effect of cold in producing a high mortality from thoracic affections. I will choose three weeks from the same period to prove that cold exercises a similar influence in raising the number of cases of thoracic disease.

In Manchester the mean temperature of the week ending December 29th, 1866, was as high as 44.0°; that of the week ending January 19th, 1867, was as low as 22·6°. From the tables above alluded to we find that the number of cases of respiratory disorders occurring in *public practice* rose from 246 in the former to 463 in the latter week; that, whereas in the first-mentioned week the ratio of thoracic to the total number of cases met with was 1 to 6, in the second week it had become 1 to 4.

Further, in the same city the mean temperature of the week ending June 15th 1867, was 58·0°, and we find that the total number of pulmonary cases which were treated in this week amounted to but 140, or in a ratio of only 1 to 9 of the total number of cases of disease.

Again, while in the five weeks ending February 2nd, 1867, 2,098 cases of pulmonary affections occurred, only 1,040 cases of the same happened in a similar period ending June 15th, 1867.

The mean temperature of the first of these periods was 33·9°; that of the second was 52·9°, or 19° higher.

The most unobservant can scarcely fail to be struck by the existence of cause and effect which is so evidently traceable in the relations between these figures.

To illustrate the intimate dependence of the tendency to disease of an abdominal type on mean temperature, I will venture to quote some facts furnished by the valuable statistics of morbility which are from year to year compiled in Sweden. In doing so, I will select more particularly the years 1865, 1866, and 1867, calling attention meanwhile to other years whenever additional proof is needed in support of our argument.

And first, it will be well to state briefly the method of arriving at these statistics, which has for some years past been in use throughout Sweden:—

It appears that in that country medical men, with very few exceptions, hold official or governmental appointments. A condition of their holding such situations is that they should send in periodically to the College of Health reports of what they have done, and statistical returns according to accurately-prepared forms. The brothers Wistrand have been the conscientious and diligent collectors and revisers of the very rich material thus obtained, which is afterwards made available to the medical public.[a]

From the tables of statistics to which I allude, and which are published from time to time in the "Hygiea," it appears conclusively that January, February, and March are the months in which the smallest number of abdominal cases is met with, while July, August, and September are those of the greatest prevalence of the same.

Thus, we find that in 1865 the number of *diarrheal* cases rose from 623 in March to 3,041 in August; that in 1866 the minimum number 663 again happened in March, while the maximum number 3,997 was attained in September; and that in 1867 the cases rose from 754 in February to 1,831 in August.

Regarding *dysentery*, the corresponding facts stand as follow:—

1865—	Min.:	40	cases in	January.
	Max.:	467	,,	September.
1866—	Min.:	19	,,	March.
	Max.:	99	,,	August.
1867—	Min.:	22	,,	February.
	Max.:	108	,,	July.

In the first part of a work by Dr. F. A. G. Bergman, on the Endemic Diseases of Sweden, which has just appeared,[b] there is given a tabular synopsis of all the epidemics of dysentery which have occurred in that country since the year 1452, together with a notification of the seasons of the year at which such epidemics prevailed. This Table shows that the epidemics occurred usually in July or August, lasting often until late in the Autumn; sometimes they continued sporadically during the winter, but almost always, at least on the approach of Spring, completely disappeared.

[a] For this information I am indebted to the courtesy of Dr. Edholm, editor-in-chief of the "Hygiea," who was so good as to send my father, Dr. W. D. Moore, the above account of the manner in which the data for Dr. Wistrand's valuable reports are obtained.

[b] *Om Sveriges Folksjukdomer.* Första Häftet. Upsåla. W. Schultz. 1869.

On some occasions May or June has witnessed the commencement of an outbreak of the disease. When, as has rarely happened, an epidemic of dysentery has appeared in winter or early in spring, it has soon ceased without having assumed considerable proportions.

The influence of *cool* summers in checking the ravages of dysentery is also well demonstrated in the same work. Thus we find that in 1783 and 1785 violent outbursts of the disease took place, while it was almost quite absent in the cool summers of 1782 and 1784. The years 1808-11 and 1813 were again noted for a great prevalence of the disease, while 1812—a cold year—intervened with a remarkable freedom from dysentery. To groups of years, such as the above, the author has applied the term "dysenteric period." The facts in connexion with the two periods of this kind just alluded to, are shown in the following table:—

TABLE I.—*Deaths from Dysentery, and Temperature in Sweden.*

Years	Deaths from Dysentery	Mean Temperature			Summer
		July	August	September	
1782	1,067	58·6°	59·0°	53·4°	57·0°
1783	8,828	64·4	60·1	54·9	59·8
1784	1,942	60·3	56·8	50·2	55·8
1785	4,436	61·2	57·4	47·8	55·5
—	—	—	—	—	—
1808	11,459	62·8	59·9	54·1	58·9
1809	11,503	63·1	63·5	53·2	59·9
1810	9,008	60·6	60·4	53·8	58·3
1811	7,204	64·6	60·3	51·4	58·8
1812	2,101	56·5	59·0	47·8	54·4
1813	6,613	63·5	58·8	54·9	59·1

So far, we have proved that summer is the season in which abdominal disease is especially prevalent. But the summer of one year differs much from that of another in point of morbility of

abdominal type. It is here that the question of mean as contrasted with average temperature engages our attention. From Table I. we have arrived at satisfactory conclusions on this point in relation to dysentery. As regards diarrhea, if we compare the years 1865, 1866, 1867—already alluded to—with 1859, we find the following results, which, for the sake of convenience, I have tabulated thus:—

TABLE II.—*Diarrhea and Temperature in Sweden*, 1859, 1865, 1866, 1867.

Years	Mean Temperature		Cases		Deaths		Ratio of Deaths to Cases.	
	Summer	Year	Summer	Year	Summer	Year	Summer	Year
1859	53·6°	42·8°	Not specified	7,000	Not specified	48	Not specified	1 to 146
1865	60·4	42·7	7,320	15,495	112	201	1 to 65	1 to 77
1866	58·6	43·1	10,011	19,531	69	170	1 to 145	1 to 115
1867	57·1	38·0	4,913	13,075	48	146	1 to 102	1 to 89
Average of the 4 years	57·4	41·6	7,415	13,775	76	141	1 to 104	1 to 107

1866 is an exceptional year, as the number of diarrheal cases was largely augmented by the cholera epidemic which raged from the end of June to the beginning of December. Leaving this year, accordingly, out of account, we see that with an increase of summer mean temperature equivalent to nearly 7°, a corresponding increase of diarrheal cases from 7,000 to 15,495 took place; that with a fall of summer mean temperature of rather over 3°, a corresponding fall of diarrheal cases from 15,495 to 13,075 occurred; and that the year 1867, which in the mean temperature of its summer approaches most nearly to the average temperature of the four summers under consideration, corresponds also most closely with the average number of cases of diarrhea.

If we may apply in the instance of Dublin the highest relative mortality of diarrhea to the number of cases occurring as deduced from the foregoing table, it will appear that in the week ending August 8th, 1868, so many as 3,100 cases of diarrhea occurred within the registration districts of that city.

We have now established, in the case of Dublin, the two propositions with which we set out—the first having reference to the relative death-tendency of seasons; the second to the influence of abnormal mean temperatures on the curves of disease and mortality of an abdominal and thoracic origin. It remains for us merely to inquire what are the classes of the community on which climatic variations exercise their baneful influence in the highest degree.

The answer to this question has been already given incidentally in the words of the Registrar-General for England, "the *very young*, the *weakly*, and the *aged*." The appended facts, culled from the Irish Registrar-General's returns, will prove this statement:—

In 1867 32·5 per cent. of all the deaths registered in Dublin were those of children under 5 years of age, and 19·5 per cent. were those of persons aged 60 and upwards.

The corresponding percentages for 1868 were 23·1 and 18·9 respectively.

In the first quarter of the former of these two years—so noted for its intense cold—the percentages were:—of those under 5, 24·9; of those over 60, 23·5.

In the third quarter of the latter year—that of great heat—the numbers were:—of children under 5, 41·5; of adults above 60, 16·9 per cent.

In conclusion, while claiming for the subject of "Medical Meteorology," in its hygienic relations, the attention which it deserves, I should be misunderstood if it were supposed that by it I hoped to explain everything connected with epidemics of various diseases. Of the causes and nature of these visitations we are still profoundly ignorant. Though this is the case, from a correct appreciation of the relations between temperature and disease much good has already resulted, much more will yet accrue.

We, it is true, have no control over changes of temperature, and the other weather phenomena which may be viewed in the light of *predisposing* causes of disease; but this is not so in respect of many *exciting* causes of the same.

These are, for the most part, tangible and remediable; and on us, as medical men, devolves the responsibility of averting and of remedying them so far as lies in our power.

THE

NORMAL PRODUCTS

OF

HEPATIC ACTION.

BY

DR JOHN G. MACVICAR,

MOFFAT.

(*Reprinted from the Edinburgh Medical Journal for August* 1871.)

PART I.—GLYCOGENIC AND FATTY MATTERS.

THE hepatic action, according to a communication made in this Journal for August 1868, has for its principal function to prevent the reconstruction of vegetable along with animal matter out of the digested food in the portal blood when depositing tissue, and thus to preserve the newly-formed animal cells free from that incrustation of cellulose which exists in the new-born cell or primordial utricle of the vegetable kingdom—an incrustation which, however valuable in that kingdom, imparting, as it does, durability to the plant-tissue or tree, would, if present in the animal kingdom, prevent that mobility and easy transformability of tissue which is essential to animal life. In a word, the view there given is to the effect that the hepatic function is emphatically that which maintains the difference between animal and vegetable substance—securing the continued reproduction of the former and the avoidance of the latter, under an otherwise inevitable morphological liability to lapse into it.

As a plant which has completed its growth, and is ripening its seeds, no longer continues to form cellulose, but disposes of the nutritive matter which continues still to flow into it, as sugar, oil, essence, resin, colouring matter, etc., in the same way, it is maintained in the communication referred to, the liver, like a fully developed plant or tree (already crowded with succulent leaves), disposes of that part of the inflowing plastic matter which would otherwise have been constructed into cellulose, as sugar, oil, bile, etc., instead; whence it comes to pass that cellulose in the animal kingdom occurs normally only in such animals as are very low in the scale—

only indeed where it is needed to give support or permanence to their otherwise too soft bodies; or if in the higher animals, then only abnormally in states of disease (amylaceous degeneration), when the hepatic action is not performing its function successfully.

According to that theory of the method of creation, indeed, which now begins to be popular—namely, the secular development of new and more highly organized animal species out of more ancient and simple ones—the limitation of hepatic action may serve another important purpose in the economy of nature. It may serve to preserve from a more speedy ascent in the animal kingdom those species in which, through the failing of the hepatic action, phyto-cellulose is abundantly deposited. Thus, while the most advanced advocates of this theory are disposed to regard certain *larvæ* among the tunicata as the ancestors of the whole vertebrata, we might explain the permanence of the *mature forms* of ascidians in nature to this day by the abundance of their cellulose, which retards their transformation; and so of many other orders of the invertebrata. But it seems to me to be building without a foundation, when men of science speculate on such a subject, and are yet both ignorant, and content to remain ignorant, as to whether there be two atoms of hydrogen, or only one, in an element of aqueous matter; or three atoms of oxygen, or only two, in an element of silica.

The theory of hepatic action now advanced, if accepted, has the advantage of accounting for a hepatic function and apparatus to the extent that it is met with in the animal kingdom, which, down to the present, has still remained among the desiderata of physiology. And if it admit of verification, it is surely worthy of it at the hands of such physiologists as pursue that study in a rational, and not a merely empirical, way. It does indeed take for granted that the characteristic material of plant-tissue (which, to save continually repeated definitions, we may call phyto-cellulose) is of easier construction than animal-tissue (which, for a similar reason, we may call zoo-cellulose). It assumes that, given a quantity of living germinal matter, having such a history as that in the portal blood, especially if it have previously existed as vegetable matter, it will, during the epoch which follows that of digestion or analysis—the epoch, namely, of reconstruction or synthesis—tend to form into phyto-cellulose rather than into zoo-cellulose; or, at any rate, to give the former as an incrustation upon the latter unless it be prevented. Now the demonstration of this, it must be admitted, cannot be given here, any more than that of one of the later propositions in Euclid's Elements without those which go before it having been first given. It merely presents itself in its own place in the new science of Molecular Morphology, and is demonstrable only to the few who have made a study of that new science. At the same time, independently of such demonstration, and on general chemical grounds, it ought to commend itself to men of science, because it is generally known that phyto-cellulose requires as material for its

construction only carbon and moisture, which abound everywhere; while zoo-cellulose not only requires these elements, but ammonia also, which is both a comparatively scarce substance in nature, and of such difficult construction out of its elements, that the chemist can scarcely accomplish it except in a roundabout way.

In the Molecular Morphology referred to, it comes out that the nucleus, or rather the axis of the least element of phyto-cellulose, is an atom of *common vapour*, fixed by three atoms of carbon placed symmetrically around it; while the corresponding part in the least element of zoo-cellulose is an atom of *ammoniacal vapour* fixed by six atoms of carbon.* But in these things, of course, I do not insist here. Nor is a belief in them necessary to our progress, nor to the acceptance of what follows.

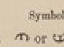
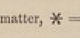
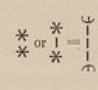
It is proposed in this communication to verify the theory of the hepatic function which has been stated, by showing that the hepatic products are in the highest degree analogous in structure to the vegetable products which have been already referred to, and which substitute cellulose in the ripe fruit and the full-grown plant or tree.

A more general object also is to give the structures and formulæ of the normal hepatic products, so far as they can be determined at present independently of any theory, and that as the first step necessary towards a true knowledge of the hepatic pathology, which I leave to others and the future.

And here, since it adds so much to clearness and distinctness of conception, let us have recourse to diagrams representing our molecular structures, so far as the types already in the hands of printers and a single plane surface such as that of the page (instead of space in three dimensions) will allow, which is not far. And let us take the same symbols as were taken in this Journal (for March 1870) in a paper on Urea and Uric Acid, modifying that for ammoniacal vapour, however, which is a dimorphous element, isamorphous in one of its forms, with a couple of atoms of common vapour on the same axis, bolted together by an atom of hydrogen, which stands concealed in the axis between them. For—

Substances.	Symbols.	Their meaning.
Hydrogen, . . .	|	A dash (say a figure 1, the atomic weight of hydrogen on the common scale).
Active Oxygen, .	∞	A figure 8, the atomic weight of a single atom of oxygen, the atoms in the aeriform state always going in couples, except in ozone.
Oxgen gas, . .	∞∞ (stacked)	
Carbon, ...	0	A cipher, its atomic weight = 6 when H = 1, and 30 when H = 5. Carbons also tend to go in couples, atomic weight = 12.
Coupled carbon, .	00 (stacked)	

* See A Sketch of a Philosophy, Part III. The Chemistry of Natural Substances, Chap. ix. The Tissue Element. Williams & Norgate, 1870.

Substances.	Symbols.	Their meaning.
Zote, . .		Azote, a coupled atom; the single atom named zote not insulable by itself, and therefore not a laboratory substance, but intensely active.
Azote, . .		
Aqueous matter,		A star of 6 rays, its literal symbol Aq, transformable into HO; eminently dimorphous.
Ammonia,		Dimorphous, and giving, on decomposition,  + III = Az H_3.

Glycogen, Dextrine, Sugars, Inosite, Glycerine, Glycocoll, etc.

And now as to those products of the hepatic action to which we shall first direct our attention, it is desirable to fix upon glycogenic or saccharine matter, and oil or fat. Not that these are the most characteristic products of the liver; but the former lies so completely at the basis of all vegetation, and indeed all organic chemistry, that the student cannot proceed with advantage in any direction if in ignorance of the structure of an element of saccharine matter; while with regard to oils and fats, they are so constantly associated with saccharine matter as its ultimate representatives when the latter has yielded up the most of its oxygen to the atmosphere again, that it is best to take the two in connexion.

Nor let it be thought that either is of small importance in the economy of nature. Molecular morphology shows that the most central part of the albumenoid or proteine molecule—that which (1) would remain if that molecule were to rot or dissolve away from the periphery inwards, and consequently that also which would (2) be required as a nucleus if an albumenoid or proteine molecule were going to be constructed, and that which (3) must remain in the blood and go out in the urine if the constructive power of the body be failing—is a group of atoms represented by the formula $C_{12}\,H_{12}\,O_{12}$, or, if dried in the laboratory, $C_{12}\,H_{10}\,O_{10}$, in short, a molecule, which, to use for it the most comprehensive term, we may call glycogen. As to fatty matter, again, it may be shown that, while it is of great value (after undergoing digestion by the action of oxygen) for constructing the carbo-hydrous part of the tissue element, it is precisely the material out of which the non-aqueous part of the neuro-cerebral apparatus is mainly constructed, being provided by nature (like other kinds of food) for this purpose, by the vegetable kingdom and the hepatic action. That it should be produced in greater quantities than are required for these purposes, and of kinds that cannot be utilized in this way, is only what is constantly occurring in the richness and variety of nature. Besides, are we to forget its use as

fuel to be burned by the oxygen of the atmosphere, brought into contact with it by respiration, on which so much stress is laid in modern physiology? Beautiful, surely, that the excess, or what is not fit for the primary use, should be thus got clean out of the system; especially if, at the same time, by raising the temperature of the organization above that of the ambient atmosphere, it does more good than harm. But to advocate mere burning as the principal end for which existence has been awarded or permitted to this, or any order of concrete substances, is to advocate one of those views (so prevalent in modern science) which, in consequence of their want of comprehensiveness, have gone far to bring the truly philosophical doctrine of final causes into disrepute.

The types used as symbols, which have been already adduced, will serve our purpose generally. But so much in all organic chemistry, and especially in most of the substances which we have in prospect, turns upon the structure of hydrogen, that it is desirable—almost necessary, indeed—to define here the structure of this element more particularly.

Let us then, according to our views, (1) affirm that there is a unit of weight or element of material substance which is common to all the chemical elements or indecomposable substances, each chemical atom consisting of a group of these material units, their number in the atom being its true atomic weight; and (2) that hydrogen is that chemical element of which the constitutive number of material units is the smallest that can give a structure which possesses symmetry, stability, and individuality when moving about among others—that is, a structure which has an axis and an equator evenly balanced between its two poles. These conditions imply five material units as the material of construction; two to form poles or terminals of an axis; and three to define a triangular equator evenly balanced between these poles, for no number less than three can determine any plane. It may be added, however, that this group of five material elements or centres of force is rather the nucleus of the atom of hydrogen than the whole of it; for these five material units, while they exist near each other, balanced *in equilibrio* by their mutual attractions and repulsions, are invested and surrounded to an unknown extent by their five atmospheres of æther, now confluent into one which envelops them as merely its nucleus.

Of the nucleus of the atom of hydrogen, the following diagram may serve as a memorial:—

Pole.

Hydrogen. Equator. • • • Equator. Atomic weight = 5.

Pole.

Hence hydrogen may enter into union symmetrically with other elements, so as to give insulable and separable substances by receiving either (1) one atom on each pole, or (2) three atoms on its equator, or (3) both. And this much of its structure it was necessary to adduce in order to explain the ratios with which a single atom of hydrogen is found united in a multitude of substances which are the products of nature or the laboratory. Thus a very stable and generally diffused substance in nature is marsh gas, its formula ($C = 6$) being $C_2 H_4$; and a very interesting substance in the laboratory is chloroform, its formula $C_2 HCl_3$. Now interpreting both formulæ in the light of molecular philosophy, we see with the mind's eye a structure of which the axis in both is an atom of hydrogen carrying an atom of carbon on each pole; and on the three points, for union on its hydrogen equator, carrying three atoms of hydrogen in the case of the marsh gas, and these substituted by three atoms of chlorine in the chloroform. The formula we may thus express in our symbols:—

Marsh gas, . . .  = $C_2 H_4$

But of these things hereafter. At first we have to do with still simpler combinations, even the simplest of all—that, namely, which results when an element of moisture and an atom of carbon enter into union with one another.

And here, unfortunately for us, we have to revert to a state of chemical theory which is no longer popular in this country. Thus, it used to be concluded from the experiments that, as all common vapour consists of one part by weight of hydrogen and eight of oxygen, and its specific gravity in the aeriform state was nine on the same scale, so the least particle of it should be represented by the literal formula HO, implying an atom of each element, the atomic weight of $H = 1$, and that of $O = 8$. More lately, however, it has been observed that oxygen does not move about freely, nor does it occupy the same volume as hydrogen in the aeriform state in weights of 8, but in portions weighing double this, that is 16. Hence it has been maintained of late that this latter number is the atomic weight of an atom of oxygen, and that the formula of the least particle of common vapour is—

$$H_2 O = 2HO.$$

For the same reason, it has been concluded that the weight of an atom of carbon is not 6 as it was long held to be, but 12. And this doubling of two primary and all-important elements has necessitated the doubling and taking doubles for singles of a great many more. The fallacy took its rise in neglecting the law of symmetry, which usually requires that all such light elements shall go and come, enter and leave, molecules in couples. But just because it is a homage to the law of symmetry, these double-weighted atoms and

their formulæ, when O and C are dedoubled, are often to be preferred to the old formulæ, since they give whole instead of half molecules.

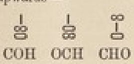
But to come into contact with molecular nature in her least particles, it is necessary to regard a single atom of oxygen as weighing 8, and a single atom of carbon as weighing 6, when a single atom of hydrogen weighs 1. Not but both oxygen and carbon tend to move and remain at rest in couples, the coupled units having therefore the atomic weights of 16 and of 12. But these elements are also found in action as single atoms; and thus we have, as the simplest and most elemental,

Hydrate of carbon, . . . CHO,

in which the letters are written in the order usual with chemists—an order which has no higher claims or pretensions than this, that when thus written the letters follow each other as in the alphabet!

But by adopting our symbols, we immediately obtain a somewhat clear and distinct conception of such a combination.

And first of all we see that, as it consists of three different members, so it may possess one or other of three different structures. And these, keeping the aeriform elements always above the fixed element on the paper, we may thus represent CHO in diagrams, as also in corresponding letters; the diagrams being supposed to be read from the bottom upwards—

Hydrate of carbon,  Trimorphous.

COH OCH CHO

Farther, we see that in none of its forms can this structure be symmetrical so as to possess two poles which are similar to each other, and an equator lying evenly between them. In virtue of the heat or other force, therefore, which actuates it in common with every mâterial structure whatsoever—that is, the palpitation, rotation, etc. of it, either in parts or in whole, it cannot remain *in equilibrio* isolating itself in space, but must ever tend to move hither or thither until it has fallen in with other structures, and sufficiently symmetrized its action by union with them. More shortly, and without attempting to give here the mechanism by which chemical activity and affinity exist and act, it is enough to say here that being unsymmetrical, CHO is uninsulable. Hence, such elements of hydrate of carbon, when constructed in the neighbourhood of each other, must tend to run together into groups or molecules. No substance, therefore, will be separable and cognisable in the laboratory whose formula is merely CHO. But everywhere that there is organic concretion or growth, especially among its first beginnings, we may obviously expect $C_n H_n O_n$, and ultimately $C_x H_y O_z$.

The question is as to the value of n. Now to this, molecular morphology gives a definite answer. The equator of the atom of carbon, as also that of oxygen (and, indeed of all the elements but a few), is pentagonal. That of hydrogen, as has been shown (also sulphur, selenium, and tellurium), is trigonal. Hence, when a group of atoms of hydrate of carbon aggregate around a common centre to form a symmetrical and insulable molecule, the number which must concur, and go to constitute the molecule, is determined. Thus, geometry shows that for pentagonal forms to concur symmetrically and completely in this way, precisely 12 are required; and the form resulting is one of the Platonic bodies, namely, the dodecahedron. For trigonal forms so to concur, on the other hand, 20 are required; and then there results another of the Platonic bodies—viz., the icosahedron. When, therefore, in a molecule of hydrate of carbon, the atom of C or of O is centrad, the multiple $n = 12$; when the atom of H is centrad, $n = 20$.

And here I may remark, in passing, that without any appeal to the special determinations of our molecular morphology, and simply on the hypotheses that the atoms of bodies overhead are spherical, and that those of the same kind are also of the same size, when placing them symmetrically around a common centre so that the group may be as symmetrical—that is, as spherical—as possible, the same numbers 12 and 20 are obtained!

But whether shall we adopt 12 or 20 as the number of atoms of CHO in our molecule? To this it is to be answered, that H is always so eager to be off, compared with O, and especially compared with C, that in all ordinary cases we must allow H a place on the periphery of molecule. We obtain, therefore, as our insulable

Molecule of hydrate of carbon, . . $C_{12} H_{12} O_{12}$.

Such then is the number of atoms in our smallest saccharine molecule, when that molecule is monometric, or most spherical and perfect in form; and when, consequently, its activity has most successfully accomplished its end; and when, therefore, its remaining activity is a minimum—more shortly, when it is most inactive or reposing. And such, it is well known, is the formula of that which has been called inactive or neutral sugar.

But in such cases the law of differentiation often comes into play; that law, namely, by which a structure is saved from explosion or solution, and is enabled to survive an ordeal as a concrete, by making some part of itself dissimilar to some other part. This law has hitherto indeed been recognised in physiology only, applied to the development and maintenence of organisms only. But in these it is only a particular display of the universal principle of dissimilarity in two or more parts or particles, as the condition of union, both initial and sustained. The embryo gains as a concrete upon the liquid in the midst of which it develops, and is enabled to survive the ordeal of the solvent power of that liquid by its own differentiation. And the same phenomenon comes into play everywhere as soon as there is chemical action and a molecule is produced.

When it takes effect upon an isometrical molecule with 6 equal axes, and of this $C_{12} H_{12} O_{12}$ is a formula, it usually does so by giving eminence to some one axis of the six, either by addition or subtraction of the same matter from both extremities or poles. Now in this case, without disturbing the fixed nucleus of C_{12}, this may easily be done by the subtraction or addition of a particle of moisture on each pole. Hence, along with that which we have found already, we may expect two other sugars, all three being—

1. The isometrical molecule, . . $C_{12} H_{12} O_{12}$
2. Differentiated by subtraction, . $C_{12} H_{10} O_{10}$ *
3. Differentiated by addition, . $C_{12} H_{14} O_{14}$

Now such are the formulæ of the three most eminent saccharine substances.

It is well known, however, that the chemical method of investigation (weighing the substances when in combination with another substance whose weight is known, followed by destructive analysis) gives these three as the formulæ, not of three substances only, but more nearly of thirty. There is manifestly therefore some secret about the molecules, which these formulæ stand for, which experimental chemistry cannot explain. One and the same formula manifestly stands for many molecules differing notably from each other, both in their physical and chemical properties.

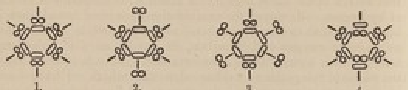
Well, then, let us see whether molecular morphology does not unfold this secret in a perfectly satisfactory manner, and bring us into acquaintance with the forms and structures of the molecules, which the chemical formulæ do indeed affirm, yet leave in concealment.

It has been shown that the simple saccharine element, CHO, is trimorphous. Suppose, then, these trimorphous elements to be aggregated together in a group of 12, with nothing to limit their permutations but the law of symmetry, acting in company with the law of differentiation, which demands a play and a variation of form between the polar and the equatorial parts of the molecule—how many dissimilarly constructed molecules, possessing dissimilar properties, may we not have, all of which the same formula $C_{12} H_{12} O_{12}$ will equally cover? Whatever that number may be, a still greater number will $C_{12} H_{14} O_{14}$ cover; and even $C_{12} H_{10} O_{10}$, in which the poles are merely atoms of carbon, fixed, naked, and invariable, may cover three.

Something of the variety and richness of nature, at all events in this respect, as in all others, the following diagrams may assist in explaining. They are as it were profiles of the dodecahedral molecule, or more shortly the dodecatom, composed of CHO, and show half the number of elements of which it consists.

* How the formula of cane-sugar comes to be $C_{12} H_{11} O_{11}$. I have elsewhere shown. See A Sketch of a Philosphy, part ii. p. 72: Williams & Norgate.

Inactive Sugars, etc.



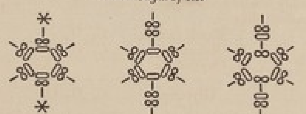
1. 2. 3. 4.

$C_{12}\ H_{12}\ O_{12}$

There are several others also. Moreover, except the first, all are differentiated in structure, though continuing isometrical in form! They will all therefore be more or less stable in the regions of nature in which they are produced, as also in the laboratory of the chemist. And thus they may possibly be detected and described by him. If the series had been continued, No. 5 would have resembled No. 3, only that the position of C and O in the poles would be inverted; and so on.

The molecule which is differentiated by the addition of an atom of moisture on each pole will give the same series increased by several additional members, as for instance the following, in which the body of the molecule remains the same in all.

Fruit Sugars, etc.



1. 2. 3.

$C_{12}\ H_{14}\ O_{14}$

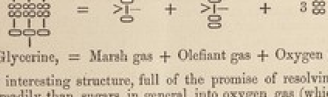
In No. 1 the molecule carries an atom of aq. (common vapour) attached to each pole. In No. 2 that atom of aq. is transformed into HO (the acid or basic state of moisture, according as the atom of H or of O is terminal of the axis of the molecule and presents itself for action). The aqueous matter in the pole of No. 2 is therefore, according to the fashionable notation, H_2O; and if that aqueous matter went off, or were drawn off, there would remain a molecule with the very frequently recurring formula $C_{12}\ H_{10}\ O_{10}$. In No. 3 each pole is a saccharine, viz., CHO. But here, as in No. 4 of the first group of diagrams, there is a liability of an atom of C on each pole to go off with H or HO, and so to reduce the carbon in the nucleus to C_{10}, and to give a start to a new series of substances.

But not yet to part with syrupy and saccharine substances, it may be shown that when single elements of CHO are set loose

along with nascent hydrogen, three of the former are apt to attach themselves to the three regions for union on the equator of an atom of hydrogen. Moreover, the combination thus constructed (each being very oblate in form) must tend to couple and to go in couples, as atoms of oxygen and of carbon, etc., which are also oblate, tend to do. Such a structure it is unfortunately difficult to represent on the plane of the paper as we have done the others. Perhaps some idea of its most symmetrical structure might be given by supposing the axis to be horizontal, and writing the formula thus :—

$$\text{Glycerine} = \quad H \begin{matrix}(CHOOHC)^3 \\ (CHOOHC)_3\end{matrix} H \quad = C_6\ H_8\ O_6$$

Giving all its constituent elements as if reduced to the same plane, and in the state in which the axes of all are parallel, we obtain—



Glycerine, = Marsh gas + Olefiant gas + Oxygen gas,

a very interesting structure, full of the promise of resolving itself more readily than sugars in general into oxygen gas (which is in fact already constructed within it) and olefiant matter, and light hydro-carbon, thus :—

1. Marsh gas, . .	$C_2\ H_4$
2. Olefiant gas, . .	$C_4\ H_4$
3. Oxygen gas, . .	O_6
Glycerine, . .	$C_6\ H_8\ O_6$

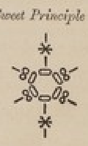
But such a structure is still far from having attained one of stability and repose. Each atom of it may be said to be a case packed full of oxygen gas, hydrogen, and hydro-carbon, needing only a change of form and a further supply of oxygen to go wholly off into the aeriform state with explosion.

But, in aiming, as every molecule structure does, at a greater degree of symmetry or sphericity, and therefore of stability or repose, such structures may, and generally do, double. And what do we now obtain? Plainly a completed syrup molecule resembling most closely that of fruit-sugar, but with this difference, that the terminal atoms of moisture are not now free to go off on the application of heat. They have now an atom of H half-occluded in each pole, which also belonged to the molecule during its genesis, and which, therefore, will be difficult to be expelled. Hence, for the entire molecule a remarkable degree of stability.

If, in fact, the polar form in this syrup—that is, an atom of vapour with an atom of hydrogen in each pole—could possibly be obtained free from carbon (which is the sedative principle among organics), the medicinal value of this H_2aq as a harmless stimulant would probaby be immense. And it does not seem impossible to obtain water aerated in this way to a certain extent.

At any rate, by the doubling of the chemical formula of glycerine, we obtain

The Sweet Principle of Oil.



$$C_{12}\,H_{16}\,O_{12} = 2\,(C_6\,H_8\,O_6)$$

We shall presently see how this beautiful molecule comes to be constructed during the elimination of oxygen from hydrate of carbon in the production of fats and oils. But yet one remark more on sugars.

In somewhat analogous circumstances, but in this case during the elimination, not of oxygen, but of nitrogen, another kind of saccharine matter shows itself, which, viewed in its chemical formula, indeed it seems strange that it should have anything to do with the sugars. That formula is $C_4\,H_5\,NO_4$, its name glycocoll or glycocine. But though this formula, as also its double, give symmetrical structures, either or both of which may exist in given conditions of existence, yet the culmination molecule must, as in other cases, be a dodecatom, and if we construct this molecule we obtain the following very interesting result :—

Sugar of Gelatine.

Polar matter, urea, . .	$C_2\;H_4\;N_2\;O_2$
Equatorial matter, sugar, .	$C_{12}\;H_{12}\;\;\;O_{12}$
Polar matter, urea, . .	$C_2\;H_4\;N_2\;O_2$
	$4(C_4\;H_5\;N\;\;O_4)$

The diagram is very beautiful, but I grudge to tax the patience of the printer more than is indispensable for illustrating our principal substances.

Glycocoll, then, is an atom of inactive sugar differentiated by one of urea fixed on each pole!

And here we may remark, that this process of differentiating the poles of a dodecatom, provided it be by additions on them of the same elements as constitute the body of the molecule, must soon lead to the construction of dodecatoms with double walls. Thus, when the axis

by successive differentiation has become too long for the equatorial part or the body, and when the latter has expanded itself to the utmost, then the body will also receive on itself such matter as had previously attached itself to the poles merely; hence, after the first isometrical molecule $C_{12}\,H_{12}\,O_{12}$, we shall have

Double-walled sugar, etc., . $C_{24}\,H_{24}\,O_{24}$;

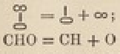
and, differentiating the poles by the omission of an atom of moisture from each, $C_{24}\,H_{22}\,O_{22}$.

Fats and Oils.

In what has preceded the various possible forms of the saccharine element, CHO have been placed in a certain order, whereof

 = CHO

is given as the ultimate form, or that towards which the various transformations tend, and in which they come to a close. It follows from the principles of our molecular morphology (and here merely experimental science will not refuse to go along with us), that in this combination all the affinities are most fully satisfied. Let it not be concealed, however, that here our new science finds in chemistry the application of a principle which has hitherto been recognised only in physiology, namely, the tendency of a structure (when under construction or change) to redintegrate an antecedent or primal form, that is, heredism. The genesis of carbon, according to the data of our molecular morphology, is always accompanied by the genesis of attached hydrogen at the same time. A molecular structure which was previously a unity undergoes segmentation or partitionment into an element of hydro-carbon. Hence, under the law of heredism, when at first in any combination, as in COH, an atom of hydrogen exists separate from one of carbon, these two tend mutually to come together. For the same reason, hydrogen and oxygen, though at first separate in a combination, tend to come together, so as to redintegrate primeval HO or vapour. Thus, we obtain the series that has been given in the diagrams on page 7, of which the issue, where the oxygen regains the aeriform state, is



$$CHO = CH + O$$

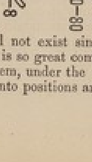
that is, the saccharine element is resolved into an element of oil and of oxygen. And hence the prevalent idea in chemistry is, that oily particles are nothing more than saccharine particles from which the oxygen has been mostly eliminated. And in accounting for fats and oils, it is usually not thought necessary to go farther back than to sugars.

Our molecular morphology leads us to suspect, however, that at least the more highly organized fats and oils—those with which we have to do in treating of the products of hepatic action, and those in the most perfectly organized animals generally—have a genetic history that goes beyond saccharine matter; that, though saccharine matter may be their immediately antecedent state, yet that saccharine matter is not a product of primary synthesis, generated directly by the union of carbon with moisture, giving CHO as the first element, but is a product of a transformation of matter, which has already either formed into the phyto-cellulose element or is tending to do so. And hence there is no permanent ground for surprise that, to all appearance, mere cellulose should be found to be fattening food for cattle and other animals who can digest it, or that oily and fatty matter should exist permanently in the liver. Nor is the existence of oil or fat there to be looked upon as disease, except in so far as it is produced in greater quantities than are eliminated by respiration or otherwise.

Moreover, this view of the chemical history of fats and oils, though doubtless it is a complication, ought not to be unacceptable to the experimental chemist; for it must be confessed that the popular theory, which regards these substances as having their origin in sugars merely as their first commencements, does not carry itself well through, nor go any way at all to explain the modes and regions of their occurrence.

The theory here advanced presents itself as a positive answer to the question, What is the transformation which an element of cellulose, or the equivalent material which is tending to form cellulose, is destined to undergo when the conditions of existence are such as to forbid the construction of cellulose, and these materials fall into the mode of union which is proper to these conditions? To answer this question, let us now proceed.

Unhappily, an element of nascent cellulose is difficult to represent in diagram. Its axis has for its middle part an atom of aq which is hexagonal, and in each pole of this atom there is inserted an atom of HO. The atom of aq is central, and around it, as an equatorial expansion on each alternate side, there is an atom of carbon attached by its edge, fixing the whole. Now, say that by any means these three atoms of carbon are forbidden to gain this truly vitalized position, or that they are obliged to leave this position, then forthwith the whole must resolve itself into three saccharine elements—

A minim of cellulose  A triplet of saccharines.

Such triplets, however, will not exist single as here represented. Their length of axis, which is so great compared with their equatorial diameter, must hurry them, under the law of symmetry, which is also a law of sphericity, into positions around a common centre,

to the end that their long axes may become equal radii, and thus offend no more against the law of sphericity. Now, one pole is hydrogen, and therefore trigonal; the other is carbon, and therefore pentagonal. Hence, if they aggregate around the carbon pole, which must give the most stable molecule, they must form dodecatoms; if around the hydrogen pole, they must form icosatoms. Supposing, then, that either in the course of nature or art, all the oxygen has succeeded in escaping, we obtain from the least element of cellulose the two most notable hydro-carbons, being the most perfect molecular states of $(CH)^3$—viz.,

$$\text{The stearic,}\quad C_{36}\,H_{36} = (C_3\,H_3)^{12},$$
$$\text{The melissic,}\quad C_{60}\,H_{60} = (C_3\,H_3)^{20},$$

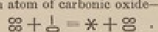
the former that which is obtained from the fats and oils of the more perfect animals, while the latter is the chemical formula of the most composite of the hydro-carbons—that which is obtained from bees'-wax, etc. It is not to be forgotten, however, that a dodecatom, consisting of $(CH)^5$ as the constituent member, gives $C_{60}\,H_{60}$, as well as an icosatom of which $(CH)^3$ is the constituent member. And we shall afterwards find that there is good ground for expecting $(CH)^5$ in nature as well as $(CH)^3$.

There is another reason also for the eminence of $C_{36}H_{36}$ in nature. Thus, the least particle of water, when given in terms of HO, is $H_{36}O_{36}$; substitute C for O, and there is given the stearic, etc., hydro-carbon. Nor is it to be forgotten that structures which, as members of molecules, can give icosatoms, can also, under a more loose development, give also tetratoms, which, in their turn, may compose themselves into icosatoms, giving a very tenderly constituted molecule, into which there enter no fewer than 240 atoms of hydro-carbon, distributed in sets of 12, too ready to degenerate into simple dodecatoms. Thus, looking to such a structure as that of the brain, we may possibly find as the

$$\text{Cerebral hydro-carbon,} = C_{240}\,H_{240} = 20(C_{12}\,H_{12}) \ldots \text{when softened.}$$

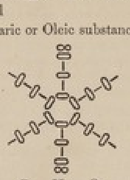
Now Liebreich found as the formula of his brain-stuff, $C_{232}\,H_{240}$. But all this is so purely conjectural, that we need not dwell upon it.

Not so problematical, however, is the structure of the molecule of the most perfect kinds of fat and oil. Not so easily broken down and softened must be the dodecatom in which the atoms of carbon are centrad, and of which the formula is $C_{36}\,H_{36}$. Suppose, indeed, that it exists free; then in this terraqueous globe, in which oxygen gas penetrates everywhere, an element of oxygen gas will no doubt soon attach itself on the extremities of one of its axes; and in this way it will be differentiated and its stability increased. The course of nature will be this: An element of oxygen gas—that is, two atoms—will attack the atoms of CH, which terminate one of the axes of the dodecatom $C_{36}H_{36}$. The terminal atoms of H, along with the single atom of O incident upon them, will then lapse into aqueous matter, which will go off when heat is applied, and there will remain on each pole an atom of carbonic oxide—



and we have the natural

Stearic or Oleic substance.



$C_{36}\ H_{34}\ O_2$.

But where such molecules are generated by the reduction of saccharine elements, we can only expect that on each pole of such a molecule there will be a saccharine, giving

$$CHO + C_{36}H_{34}O_2 + CHO = C_{38}H_{36}O_4.$$

Now we have already seen that atoms of CHO in hydro-carbon or fatty regions tend to form into atoms of glycerine, of which the ultimate or true syrupy form is $C_{12}H_{16}O_{12}$. The single dodecatoms of $C_{36}H_{34}O_2$, in like manner, will tend to form into composite dodecatoms of $(C_{36}H_{34}O_2)^{12}$. Thus ultimately there will result a beautiful and finely differentiated structure, its body consisting of oil, its poles, of a sweet principle. We thus obtain

Stearine.

$$C_{12}H_{16}O_{12}\ (C_{36}H_{34}O_2)^{12}\ C_{12}H_{16}O_{12} = C_{456}H_{440}O_{48} \div C_{38}H_{36.6}O_4.$$

Now, this when drawn and quartered according to the usual practice of chemists, so as to reduce the glycerine to a single atom, its formula supposed to be $C_6H_8O_6$—that is, dividing it by 4—gives $C_6H_8O_6 + 3\ (C_{36}H_{34}O_2)$, the tri-stearine of chemistry! Not but that a tri-stearine may be one of the states of true stearine when nascent, or when undergoing destruction in the laboratory—for it is a symmetrical structure of much power. But, according to morphology, we must look here as usually upon the composite dodecatom with differentiated poles as the culmination form towards which nature is ever tending, and which she generally succeeds in constructing.

The chemical custom of cutting down every formula till one of its constituents is reduced to unity, is utterly out of keeping with the entire procedure of nature. Why should all molecules be made to stand upon one leg, when the visible creatures formed of them have never less than two?

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THE

NORMAL PRODUCTS OF HEPATIC ACTION.

BY

DR JOHN G. MACVICAR,
MOFFAT.

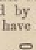
(*Reprinted from the Edinburgh Medical Journal for September* 1871.)

PART II.—Cholesterine. Choline, Neurine. Protagon. Dyslysine. Cholic, Glycocholic, and Taurocholic Acids. Acetic and Lactic Acids (a Digression). Tissue and Colorific Elements, Biliverdine, Cholophœine (coal-tar).

CHOLESTERINE.

IN what has preceded (see this Journal for August 1871, p. 128) we have the elements CHO when aggregating into a group of 12, giving what, for want of a more comprehensive generic term, we have called the glycogenic molecule. But this molecule results only when these elements aggregate with the carbon or oxygen centrad. When, on the contrary, the hydrogen is centrad, the molecule must consist of 4 members only, or possibly of 8, and ultimately and most perfectly of 20. Of the last, the icosatom, I shall say nothing here, because (excluding albumenoid substances) the products of hepatic action do not appear to give molecules of a more composite structure than the dodecatom or the tetratom. We have already obtained certain products which are dodecatomic; we shall have to return to the dodecatom, for it is constantly recurring; but, for the present, we must turn our attention to the tetratom, *i.e.*, the molecule of four members.

The simplest of the type of the tetratom is that of 4 atoms of hydrogen, one standing as an axis for the other three, which lie at right angles to it, on the trigonal equator of the axial atom of hydrogen, and form three equatorial radii or arms. As might be expected of such a highly volatile and elastic element as hydrogen, this tetratom of hydrogen by itself cannot be condensed or retained in any kind of groupings or molecules at the surface of our planet. But when the tetratom is steadied by an atom of such a fixed element as carbon on each pole we have it; for there is then—

Marsh gas, . . .  C_2H_4

This, when the three equatorial arms are substituted by chlorine, gives chloroform, and explains a multitude of phenomena of chemical substitution in the laboratory, in which three atoms of hydrogen, neither more nor less, are so frank in departing and submitting to substitution, while a fourth resists and remains.

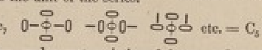
When brought into harmony with the oxygen (which is present universally in the terraqueous globe, except in a very few substances), by placing an atom of oxygen on each pole so that the poles are atoms of carbonic oxide, then, instead of merely carbon, we obtain the symmetrical or æriform state of—

Methylic alcohol,  $C_2 H_4 O_2$

And of this we shall soon see the occurrence as a polar element in a beautiful product of hepatic action.

Marsh gas in the free state is rather an ultimate type and redintegrate of primeval genetic molecular action than a substance belonging to the condensed, the concrete, the organized sphere. Viewed in reference to that sphere, so little carbon with so much hydrogen is unnatural. Rather we are to expect that, instead of escaping with 3 hydrogen points naked, each of these points will carry an atom of carbon, so that when all the oxygen has succeeded in escaping, we shall have as the lightest and least hydro-carbon of living nature, and as the product of the last vital analysis, a structure, the formula of its element $C_2 H_4 \times C_3 = C_5 H_4$.

Now the molecular structure corresponding to such a numerical ratio of carbon and hydrogen is polymorphous in a high degree. This formula will therefore stand for many substances of different properties. But all of these structures when single are oblate in form. Hence they will tend to aggregate again into such molecules as their far-extending equators determine. Now, these equators are trigonal. The molecules, provided they be isometrical, will therefore be tetratoms and icosatoms. Now, of these the simpler gives the well-known formula of $C_{20} H_{16} = 4 \, (C_5 H_4)$; and I have elsewhere[1] shown how completely this structure explains the phenomena of the non-oxygenated essential oils; with regard to which, indeed, M. Cahours has shown that, along with $C_{20} H_{16}$, it is necessary to assume the groupings $C_{15} H_{12}$ and $C_{10} H_8$, leading to our structure $C_5 H_4$ as the unit of the series.

An atom of essence,  etc. $= C_5 H_4$

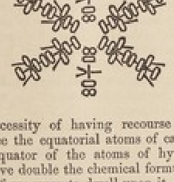
Such structures, then, under vast varieties of forms, and usually, no doubt, in coupled atoms, like the elements of the atmosphere itself, are everywhere escaping from the mature parts of vegetable nature into the atmosphere.

To their escape the vegetable kingdom mainly owes its fragrance. If, then, our theory of hepatic action be sound, we must expect

[1] A Sketch of a Philosophy, P. III., p. 77. Williams and Norgate.

some hydro-carbons of the same kind as its product. It is not to be forgotten, however, that there is such a vast difference between the organism of a plant or flower, so exquisitely expanded in the thin air under the genial radiance of the sunbeam, and that of the liver, crowded into a dark compartment inside the ribs, overlaid by the diaphragm, and by it cut off from all chance of communicating with the air, that it would be too sanguine to expect a hydrocarbon similarly to escape merely as fragrance from the liver. Rather we are to expect that in this viscus the fragrant elements will be packed together into some stable molecule, which will be persistently concrete, and therefore have little or nothing to give to the air, or to the olfactory nerves when snuffing the air.

Suppose, for instance, that the atoms of CH on the equator of $C_5 H_4$ were united so that they shall be parallel to the atoms of C and H, which form the axis of the combination, as (though not well) is figured on the right hand in the preceding diagrams, then each molecule of $C_5 H_4$ would be dissymmetrical, and therefore uninsulable by itself. Moreover, the equator would not project so far as to prevent their uniting into dodecatoms, that is, into the most stable kind of molecule. Suppose, then, that we have a dodecatom, composed of such elements in couples as its body, and differentiated on the poles by two atoms of oxygen, one to give fixity to the nucleus (by differentiation), and the other to protect from the further attacks of ambient oxygen, with an atom of marsh gas supporting each—that is, suppose we have for the molecule a body consisting of concreted essence, and poles of methylic spirit—we thus obtain a beautiful structure which gives the formula of—

Cholesterine,  $2 \, (C_{52} H_{44} O_2)$

And here the necessity of having recourse to a woodcut has enabled us to place the equatorial atoms of carbon more correctly on the trigonal equator of the atoms of hydrogen. Why the molecule should give double the chemical formula has been already explained. And if we were to dwell upon it, we might show how well this molecule explains the characters and modes and places of occurrence in the organism of this beautiful but formidable substance, which is met with, not only in the liver, the gall-bladder and its ducts, but in the brain, the blood, etc., as also in the vegetable kingdom.

If it could be resolved into its organic elements without the decomposition of these elements, and if the equatorial elements could be reconstructed into one molecule, and the polar elements obtained separate, an atom of cholesterine would give—

1. Atom of heavy essential oil, 20 $(C_5 H_4) = C_{100} H_{80}$
2. Atoms of methylic alchohol, 2 $(C_2 H_4 O_2) = C_4 H_8 O_4$

Cholesterine, . . . 2 $(C_{52} H_{44} O_2) = C_{104} H_{88} O_4$

According to this view, then, the habitual use of condiments, etc., is not a trivial matter in a hygienic point of view. And, in reference to hepatic action, the essential oils and resins, camphor, etc., when judiciously used, ought to be valuable medicines.

Choline. Neurine. Protagon.

It has been supposed, in what has preceded, that the only bond of union between the two atoms of essence $(C_5 H_4)$ which, when united, form the members or radii in the dodecatom of cholesterine is the dissymmetry of each, forbidding each to exist separately, and not in lesser groups than a couple, which is the simplest combination that gives symmetry. But the atom of essence may also itself have a symmetrical structure, as, indeed, it has in the first two diagrams that have been given of it above. And in this case, in order to bind a couple strongly together, some other element as a coupling joint is required; for the oblate form of each element of essence, though it must tend to bring them together in couples, and to keep them together while the conditions in which union took place continue to subsist, yet the merely coupled element must be liable to easy dedoubling and dissolution when these conditions are changed.

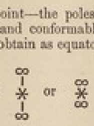
Hence an important part is played, both in nature and the laboratory, by a class of remarkable forms which may be generally designated *coupling joints*.

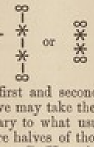
Of these, the most important is oxygen, whose form, were we to go into the genesis of the elements on the principles of our molecular morphology, we should find to come out so that it resembles a doubly concave lens, a blood-disk, or a life-buoy. It is, therefore, admirably suited to serve as a coupling joint for structures whose poles are carbons or similar forms, for these may be said to resemble a convex lens, so that, with regard to oxygen and them, each is a mould for the other, like the glasses in a triple achromatic arrangement in an optical instrument.

The atom of oxygen, when acting as a coupling joint, exists often alone, and, indeed, in this case often binds the two elements in its poles on opposite sides so firmly together that they are wholly undecomposable in the laboratory, and only secularly decomposable during the lapse of long ages under the sustained action of the analytical energy of nature.

But where, instead of merely dry oxygen, moisture is present, and still better, when moisture and ammonia are present, a more tender coupling joint is constructed. In this case the atoms of O in HO form the poles of the coupling joint, while the atoms of H are inserted in the poles of an atom of *aq* or of *am* which forms the equatorial

part of the coupling joint—the poles in both *aq* and *am* being negative or re-entrant and conformable for receiving the atoms of H of HO. We thus obtain as equatorial coupling joints or nuclei for axial forms—

The Aqueous coupling joint,  = OH aq HO = 3 HO

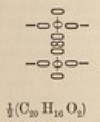
The Ammoniacal coupling joint,  = OH am HO = Az $H_2 O_2$

As examples of the first and second, consisting of pure oxygen and aqueous matters, we may take the camphors of nature and of the laboratory. Contrary to what usually happens, however, our formulæ in this case are halves of those of the chemist, he regarding the unit of essence as $C_{20} H_{16}$, while we regard it as the fourth part, viz., $C_5 H_4$.

In natural camphor the coupling joint is simply an atom of oxygen.

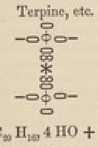
In artificial camphor (terpine, etc.), it is an atom of aqueous matter, such as has been described, which might be called basic moisture—

Camphor.



$\frac{1}{2}(C_{20} H_{16} O_2)$

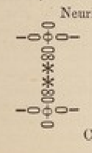
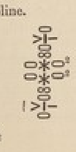
Terpine, etc.



$\frac{1}{2}(C_{20} H_{16}, 4 HO + 2 aq)$

And now, making the usual movement from the vegetable to the animal kingdom, let us substitute an atom of aquæform ammonia for the atom of moisture in the centre of the element of artificial camphor or terpine, and we obtain an interesting result, viz., a dimorphous element, whose formula is that of the lately-discovered

Neurine, or Choline.

$C_{10} H_{13} NO_2$

The diagram on the right hand represents the structure when assimilating itself to tissue. And if the carbon be doubled on the body (for which there is just room) it represents it as on the way for generating a colorific merely by oxidation or the discharge of hydrogen, as will soon appear more plainly.

The same translation of the 2 × 3 atoms of carbon from the poles to the equator may possibly take place in reference to the merely aqueous camphor, and the genesis of a colorific equally result.

But all such structures are capable of improvement by doubling, especially this neural and hepatic element, which when doubled gives a dodecatom of the simple hydro-carbon $(CH)_{12} = C_{12}H_{12}$, carrying an atom of that interesting substance aldehyde-ammonia on each pole—

1. Dodecatom of CH, . . $C_{12}H_{12}$
2. Aldehyde-Ammonia, . $C_8H_{14}N_2O_4$

1. Choline, ... $C_{20}H_{26}N_2O_4 = 2(C_{10}H_{13}NO_2)$

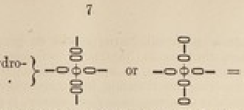
It is never to be forgotten, however, that whether taken as single or double, these structures are only single elements or members of dodecatoms (or possibly of icosatoms) which are the true molecules.

Protagon.

And here I am tempted to show that by constructing into a dodecatom these two isomorphous elements, that with the aqueous, and that with the ammoniacal coupling joint—the former as the body, the latter as the poles—of what is at any rate a most exquisitely constructed composite dodecatom, and by placing an atom of phosphorus as the coupling joint between two such structures, we obtain as nearly as could be expected or desired the formula of Liebreich's protagon, his element of the cerebral system. But to press such matters at the present moment is premature. The constructive faculty has not sufficient light to walk safely into the sanctum sanctorum of the temple of the organization. This much has been here mentioned, however, as serving to show the possible value of the liver to the brain, or the drag that it may be on it.

Dyslysine and Cholic Acid.

Hitherto we have met with no other products of hepatic action but such as are also produced by vegetation. But from the more vigorous synthetic activity of animal life, and of such a massive organ as the liver, are we not to expect some richer hydro-carbon than the essential oil element, whose minim formula is C_5H_4? Yes; let us suppose that the main part of the hepatic hydro-carbon is the same, but that in the liver the vegetable hydro-carbon receives the usual addition of C_2H_2, that is, CH on each pole. We thus obtain a dimorphous structure for—

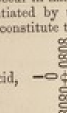
The Biliary Hydro-carbon, . . .  $= C_7H_6$

And now let us suppose that structures of this kind attach themselves around a glycogenic atom as a nucleus, the intermediate atom of hydrogen being common to both. We thus obtain at once the formula of

Dyslysine, . . . $C_{12}H_{12}O_{12} + 12(C_7H_5) = 2(C_{48}H_{36}O_6)$,

that biliary body which survives all others under the ordeal of the laboratory. Nor need we wonder at its so surviving, because in consequence of the dimorphism of the biliary hydro-carbon the dodecatoms may be differentiated in structure, and so rendered stable, while yet they remain isometrical in form. This substance does not, however, like the other dyslysine, more happily named cholesterine, occur elsewhere than in the laboratory.

Nor does it yet occur in nature, even when the polar elements are more fully differentiated by the accession of oxygen substituting hydrogen, so as to constitute them into what might be called—

Tricarbacetic acid,  $= \left\{ \begin{array}{l} C_4H_4O_4 \\ C_3 \end{array} \right. = C_7H_4O_4$

But we now obtain a dodecatom which presents itself in every case to the chemist, where the more exquisite differentiation of the poles by nature in the construction of the bile-molecules has been destroyed and substituted by moisture or atoms of H and O during the processes of the laboratory. For thus we obtain

Cholic, or Cholalic acid, . $\left\{ \begin{array}{ll} \text{The nucleus (sugar),} & C_{12}H_{12}O_{12} \\ \text{The body } 10(C_7H_6), & C_{70}H_{60} \\ \text{The poles } 2(C_7H_4O_4), & C_{14}H_8O_8 \end{array} \right\} = 2(C_{48}H_{40}O_{10})$

$$C_{96}H_{80}O_{20}$$

And now we reach the bile-acids themselves.

Glycocholic Acid, etc.

The two most eminent biliary molecules, with regard to which there is room for believing that they have not been much altered by chemical processes, are glycocholic and taurocholic acids, or rather, indeed, these when steadied by an atom of sodium in each pole substituting the terminal atoms of hydrogen. These names are meant to be expressive of the fact that, under appropriate chemical treatment, both acids dedouble, the former yielding glycocoll, and the latter taurine, along with cholic acid in both cases. Now, where there is to be glycocoll, nitrogen will be given by combustion. And where there is to be taurine, sulphur also will begiven. For these elements, then, we must provide a place in these acids.

Now, directing our attention, in the first instance, to glycocholic acid—which, as occurring in our own species, is the most interesting in a pathological point of view—and assuming that the body of the molecule is that which survives in cholic acid and dyslysine, we have only to determine the structure of the poles; for the poles, no doubt, contain the small quantity of azote which is found on combustion. But shall we suppose that this element exists as azote in a molecule before which there is such a long career of vital action as there is for a particle of bile? Azote is, of all known elements, the most truly residuary and inactive, nay, we might say, excrementitious. Doubtless, when functioning in the living organism, it exists along with its occluded hydrogen as ammonia.

Let us take, then, as the central element in the poles of the glycocholic molecule, an atom of aquæform ammonia.

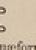
But ammonia by itself is a vapour, and must be held down and fixed somehow, else it will be off. How, then, shall it be fixed here?

Doubtless, in the same way as aqueous vapour is fixed when occurring, like ammonia, in the living organism; that is, by carbon.

But how shall atoms of carbon apply themselves to our atom of ammonia in the conditions of existence in which bile is generated?

To this the most considerate answer is, that the atoms of carbon may apply themselves in many ways, thus involving occasionally the construction of abnormal as well as normal bile, and consequently a pathological as well as a healthy action of the liver.

But when we consider that the ultimate hydro-carbon, which is at once simple and symmetrical in all the hydro-carbons that we have yet met with, is the essential oil element or marsh gas with its hydro-carbon or hydrogen wings gone (which might be called

Apterous marsh gas), . . . 

and that the exterior structure of an atom of aquæform ammonia, (being, as Laurent long ago supposed, a kind of hexagonal prism), is altogether suitable for receiving three such forms as buttresses to its alternate sides, we may meantime suppose that this is the way in which the atom of ammonia is fixed equatorially—an arrangement which appears to exist in the aspartic, and many other vegetable principles.

And what as to the poles of this beautiful ammoniacal structure? To this it may be replied at once, that saccharines (CHO), as, indeed, we have seen already, when they cannot construct a dodecatom, ever tend to go in sets of 3, while, at the same time, in every region of vital dissolution followed by reconstruction, saccharine elements

are always present. This would give us, as the material of which the poles of the molecule of glycocholic acid are constructed, the remarkable numbers for the two poles—

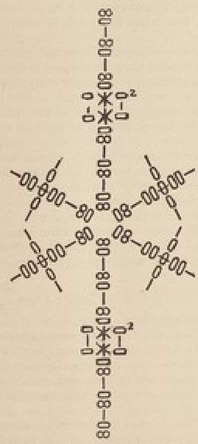
$$2 \times 2 \times 3 = 12 \text{ Saccharine elements, . . . } C_{12}\,H_{12}\,O_{12}$$
$$2 \text{ Hydro-carbo-ammoniacals, . } C_{12}\,H_{12}\,N_2$$

or, leaving the innermost saccharines on the poles to appear as belonging to the saccharine nucleus, and adding an atom of moisture on each pole, we obtain at once—

$$\text{Glyco-cholic acid,}\left\{\begin{array}{lllll} \text{The nucleus,} & C_{12} & H_{12} & & O_{12} \\ \text{The body } 10(C_7\,H_5), & C_{70} & H_{50} & & \\ \text{The poles, . . .} & C_{22} & H_{22} & N_2 & O_{10} \\ \text{Two HO, . . .} & & H_2 & & O_2 \end{array}\right\} = 2(C_{52}\,H_{43}\,NO_{12})$$

$$C_{104}\,H_{86}\,N_2\,O_{24}$$

Glycocholic Acid.



2 $(C_{52}\,H_{43}\,NO_{12})$

When this exquisite molecule is reduced by the consolidation of the polar matter, and the reduction of the ammonia to a nitrogen or urea state, it is easy to see that, by the appropriation of moisture, two atoms of glycocholic acid must give two of cholic acid and one of glycocoll in its mature or dodecahedral form (see p. 139).

With regard to taurocholic acid, the principal difference between it and glycocholic is that it carries also an atom of sulphur on each polar member.

There are also other acids of the bile in certain of the lower animals, in which there is a higher charge of carbon than in the human biliary acids when they are normally constructed. Thus, in the corresponding bile-acid of the goose there is $C_{22}\,H_{18}$ more than in glycocholic acid. And, by unnatural treatment of this bird, it is well known that the quantity of fatty matter in its liver may be increased enormously.

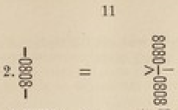
And most probably in ordinary cases in man pathological states of the bile consist in the main in an overcharge of hydro-carbon; for carbon is the universal sedative entering into the organism, first indeed to protract existence, and to prevent life from being merely ephemeral, but when in excess tending to fix altogether, that is, to kill.

But molecular morphology in its actual state does not enable us to determine for certain the precise order in detail of the elements constituting such a molecule as a biliary body, especially the order of those of the polar members, although almost the whole of the functioning depends on the structure of the poles of a molecule. Since, therefore, we are forbidden to attempt to state the exact difference between normal or healthy and abnormal or unhealthy bile, when both give the same elements on combustion, we need not carry our constructions farther. And with a few words on the tissue elements and colouring matters constructed by the liver, I will bring this memoir to a close.

But before entering on the colorifics, it will be desirable to see our way as to the nature of certain acids which form the polar elements of these colorifics, and which, indeed, are constantly recurring during organic transformations, I mean the acetic and the lactic acids.

The Acetic and Lactic Acids.

Immediately resulting from the solution and reconstruction of the saccharine molecule $C_{12}\,H_{12}\,O_{12}$ we obtain two acids—the acetic and the lactic—the acetic element consuming two, and the lactic three saccharines (atoms of CHO). These are placed on the same axis in both cases, which improves their symmetry so as to render them insulable, and therefore cognisable in the laboratory as separable or separate substances, at least when they have doubled. This doubling is in such cases the usual course of molecular synthesis in the procedure of nature. When, indeed, by so doing a tetratom of hydrogen, as in marsh gas (see the diagram on p. 1), may be constructed as the nucleus or equatorial body, it may be said to be universal. Thus we obtain—

Acetic 2.  Acid (æriform)

2 (CHO) = $C_4\,H_4\,O_4$

Thus, as the product of the dissolution of an atom of a neutral or inactive sugar, we obtain the equation—

1 Sugar $C_{12}\,H_{12}\,O_{12} = 3\,C_4\,H_4\,O_4$ Acetic acid.

But it is also well known that the saccharine molecule (when its stability is increased by placing it in congenial combination, as when cane sugar is made to unite with lead oxide) may be preserved when its formula reduced to the lowest numbers is $C_{12}\,H_9\,O_9$. Now we must look whether to such a sucrate there be not a corresponding acetate. And here it appears that by disposing of the moisture with the economy which the smallness of its quantity calls for, we obtain another acetic structure of great beauty, and which, if secluded from moisture, may be of great stability. In this case, for every 4 atoms of carbon there are 3 of moisture. Hence, in order to symmetrical structure, an atom of moisture must be the equator of the structure. Now, this it can be if the resulting structure is to be a symmetrical form (which a position in the centre demands) not as HO, but only as aq. Into each pole of this central atom of aq, therefore, let the two remaining atoms of HO be inserted by their H poles, and there will be constructed as the equatorial body of the whole structure the beautiful form which we have already met with as the coupling joint of terpine, etc. (see p. 5). And thus in certain acetates we shall have—

Acetate acid  $C_4\,H_3\,O_3$

Moreover, this acetic form, when in the molecular state, and in the presence of moisture, will doubtless immediately take on an atom of HO on each of its 12 members, and thereafter diliquesce and dissolve. Hence, when dried again, the atoms of HO continuing attached, there will be given as before the formula $C_4\,H_4\,O_4$.

But many cases must occur where the saccharine or hydrate of carbon molecule in suffering dissolution must give the consequent synthesis of saccharine elements not in sets of two, but in sets of three. From this there must result elements in each of which there must be 3C, and when doubled 6C, with more or less of H and O according to the conditions of existence and the particular reaction. Without going over in detail the possible forms of this new acidous structure, of similar origin, but one step higher than acetic acid, we may here notice that which the constantly recurring molecule $C_{12}\,H_{10}\,O_{10}$ must give when all the matter of it is engaged in this new and degraded arrangement.

Lactic acid —08080✱08080— $C_6\,H_5\,O_5$

which, like the other just mentioned, will, when set free in the presence of moisture, and in the molecular state, become moist all over by the incidence of aq on each of its members, thus giving as the formula of each $C_6\,H_6\,O_6$.

And here I may remark, that this acid is an intensely interesting substance in a physiological and pathological point of view; for our molecular morphology shows that the lactic spindle COCOC is nearly isomorphous as well as isobaric with an atom of chlorine. Hence there is in the animal frame, especially in the stomach, a play between chlorhydric and lactic acids, which it would be of great importance to understand; for while lactic acid, like chlorhydric acid, seems to be of great value in the stomach at certain times, it is undoubtedly a great evil in the blood and in the system generally.

Tissue and Colorifics.

The economy of Nature appears everywhere to be, first, to provide for The Useful, and when that is fully or more than fully done, to dispose of the surplus as The Beautiful.

The nascent element, which goes to the construction of tissue, and gives growth of parts, is composed of the same materials, and possesses the same structure as that which imparts the beauty of colour to these parts.

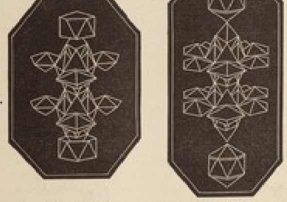
The difference between the two, both being taken in their vernal or nascent forms, consists in this, that in the colorific element the quantity of carbon is much increased — we may say, generally doubled. This appears merely by comparing the respective chemical formulæ, without claiming any insight into the structure of the molecules more than these formulæ impart. Thus the proteine albumenoid, or tissue element, as obtained by C. Schmidt from the muscles of insects, the most perfect of muscular creatures, when reduced to its lower terms (one of the members or radii, 12 of which constitute a dodecatom, or 20 an icosatom, that is, a proteine), when compared with the long-established formula of indigo, the most perfect of colorifics, gives—

Muscle of insects, .	$C_8\,H_5\,NO_2 + HO$
Indigo, ...	$C_{16}H_5\,NO_2$.

Unfortunately, it is impossible to give satisfactory diagrams of such molecules as these formulæ represent with types already in the hands

of the printer, and on a single plane such as that of this paper. But the following, which give—as white lines on a black ground—the ultimate resultants of the elementary forces, or material elements, may be introduced here. They make an approach, according to our molecular morphology, to the true forms of the molecular elements. That on the left hand represents an element of tissue when first nascent, and fixed by a minimum of carbon atoms (the atom of carbon in each pole being not shown). That on the right hand is the same, now become a colorific element in virtue of its being fixed by a maximum of carbon atoms. The axis in both is an atom of ammonia in its aquæform state, which has six edges or sides; and these in the nascent tissue element on the left carry an atom of carbon on each alternate edge, and in the colorific on the right one on every edge.

A tissue element (nascent).



A colorific element (nascent).

$C_8\,H_5\,N\,O_2$ $C_{16}\,H_5\,N\,O_2$

Now, if on the poles of this colorific element, instead of the remarkable spherule, 080 $=C_2O$, we substitute the more lengthened acetic element, $C_2H_2O_2$, and the still more lengthened lactic element $C_3H_2O_2$, we obtain—

1. The colorific body,	$C_{12}\,H_5\,N$	+ Aq
2. Acetic poles, .	$C_4\,H_4\quad O_4$	Biliverdine.
	$C_{16}\,H_9\,N\,O_4 + Aq$	
1. The colorific body,	$C_{12}\,H_5\,N$	+ Aq
2. Lactic poles, .	$C_6\,H_4\quad O_4$	Cholophœine.
	$C_{18}\,H_9\,N\,O_4 + Aq$	

These may, however, be regarded rather as the vernal or nascent forms of the colorifics than as their mature and most stable forms.

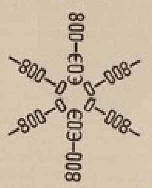
It appears that colorifics may indeed be preserved in these forms by being placed around so as to encase or enamel a metallic dodecatom, to differentiate which a tetratom of the colorific is placed on each pole. By this arrangement there may, for instance, be obtained a colophœate of calcium, the calcium being 6·86 per cent., while, from this or some such combination, Thudicum found 6·91, and Stoedeler 6·5 per cent.

But when left to the course of nature, all such structures tend to double; for by so doing they construct compact dodecatoms, sure to be duly differentiated on their poles by the reduction of the carbammonia to its carb-azotic state.

But what the actual arrangement of the successive elements in the constitutive members of the dodecatom from the centre outwards may be in any given conditions of existence, it would be difficult, if not impossible, to determine in the present state of molecular morphology. Taking them in their most fully differentiated forms, however, and as protected from the further attacks of oxygen by having their poles mailed in oxygen already, we obtain such diagrams as these—

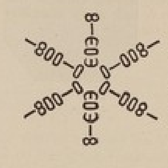
$C_{36}\ H_{22}\ N_2\ O_{12}$

Cholophœine.



$C_{32}\ H_{22}\ N_2\ O_{12}$

Biliverdine.



$C_{32}\ H_{10}\ N_2\ O_4$

Indigo.

The great repository of similar molecules is coal-tar, which, being the crude destillate of the most stable parts of the magnificent vegetation of a former world, is naturally full of colorifics and fragrant essences, or is at any rate in such a state that on the first opportunity of free motion among the particles, these colorifics and fragrant essences are redintegrated, with more or less of the perfection of nature. For though chemists have hitherto left physiologists in exclusive possession of the law of heredity, yet this law reigns among specific molecules no less than among the visible species that are composed of these molecules, and when attended to, explains those phenomena of varying chemical affinity in the same elements, which have hitherto been unexpected, and deemed to be arbitrary and anomalous.

Of the colorific evolutes of coal-tar our theory leads us to look for, as at once the simplest and the most stable and the basis for many, the pure dodecatum, consisting of twelve elements of what has been spoken of above as apterous marsh gas (see the diagram on p. 8), which, however, in the molecular state is dimorphous. We thus at once obtain—

$$\text{Benzine} \quad . \quad . \quad . \quad C_{24}\ H_{12} = 2\ (C_{12}\ H_6)\,;$$

and by any one who will take the trouble, it will be found that by the orderly differentiation of the poles of this dodecatom, the other substances with which chemistry and the arts are familiar present themselves.

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ON

THE NATURE

OF THE

INTESTINAL LESION OF ENTERIC FEVER.

BY

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DUNDEE.

(Read before the Medico-Chirurgical Society of Edinburgh, 15th March 1871.)

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INTESTINAL LESION OF ENTERIC FEVER.

THAT the intestinal lesion of enteric fever is specific in character and peculiar to that disease, and that the discharges thrown off from the bowels after the establishment of that lesion contain the poison of enteric fever, and are capable of imparting that disease to healthy individuals, are two propositions which have been abundantly proved of late years.

Whether the alvine dejections form the sole means of communicating the disease, is a question whose discussion is foreign to the object of this paper. Suffice it to say that they form at least one, and undoubtedly the most common, medium of communication.

How come the stools to have this virulent character? and in what part of them is contained the poison which produces such effects?

The source whence they derive their virulence is unquestionably the specific lesion of the solitary and agminated glands; and the sloughs and discharges resulting from these lesions are the particular parts of the stools in which the poison is contained.

The exact nature of this intestinal affection has been a matter of some difference of opinion. At the present day it is very generally regarded as being somehow or another connected with the elimination of the poison from the system. By many it is even looked upon as the result of a specific morbid deposit, the throwing off of which is Nature's mode of freeing the system of the disturbing element.

Such a view finds its chief support in the proved infectious properties of the intestinal discharges. It is, however, quite inadequate to explain many of the more striking features of the glandular lesion, to which reference will hereafter be made; and is at variance with the fact that mild cases of enteric fever do occur in which the glandular lesion terminates in resolution, and in which the so-called morbid deposit is re-absorbed into the system.

Moreover, there is no proof that the affected glands are eliminating organs at all. If they were so, it is probable that the work of elimination would be more equally distributed, and that the glands situated at the lower extremity of the ileum would not be overworked, whilst those high up escaped altogether.

According to this view, too, the throwing off and deposition of the morbid material ought to be followed by an amelioration of the general symptoms, which is notoriously not the case. Besides, destruction of the affected glands, which commonly ensues, is not a likely means of promoting any eliminatory functions which they may possess.

Regarding the nature of these glands there are two opinions, neither of which pretends to do more than in a general way indicate the nature of the work which they do.

One is that each gland is a secreting cell, which periodically bursts and discharges its contents into the intestine. The other is that they are absorbent glands, which have no share in the production of the intestinal contents, but are somehow engaged in the elaboration of the fluid which is ultimately carried away by the lacteals.

The weight of evidence is, I think, in favour of the latter theory. The appearance which they present under the microscope is also in keeping with it. "When examined with the microscope, these bodies present a remarkable similarity in structure to the so-called medullary cords of the lymphatic glands, and have recently even been regarded as really belonging to the system of lymphatic glands."—(*Stricker's Human and Comparative Histology.*)

The pathological phenomena presented by these glands in enteric fever also support this view, and afford very strong evidence that they are not secretory but absorbent.

The pathological arguments against their being eliminating organs have just been given. Those which bear out the opposite opinion, that they are absorbent glands, will appear further on. Suffice it now to state that the recognition of their absorbent properties greatly facilitates, if it is not essential to, the explanation and proper elucidation of the diverse phenomena of the disease, and especially of the intestinal lesion.

The question naturally arises, Why should these glands suffer more than any other part of the intestine? It has been answered, "Because they are more vascular, and of more delicate structure." A most unsatisfactory reason. Better far honestly to confess our inability to answer the question, and say, "I do not know, and that for two reasons—(*a*) I do not know what is the exact function of these glands; and (*b*) I do not know the exact nature of the poison of enteric fever."

When these two obstacles are got over, the reason why these glands suffer will not be hard to discover. Till then, all attempts at explanation can be but vain theorizing.

In the meantime, a careful study of the morbid phenomena presented by the intestinal lesion, and a correct interpretation of the

facts observed, may lead to a sounder and more satisfactory knowledge of its pathology than we at present possess, and may lay the basis of a rational system of treatment. To this task I would now direct attention.

The view which I take of the nature of this lesion is, that it is a specific inflammation of the agminated and solitary glands, accompanying every case of enteric fever, complicating and intensifying, but in no way shortening the general constitutional affection, and bearing to enteric fever much the same relation that inflammation of the tonsils does to scarlatina.

I believe also that the sloughs and discharges from the ulcerated glands carry the poison of enteric fever, and are capable of conveying that disease from one person to another, just as the discharges from the mouth and nostrils in scarlatina are capable of transmitting their peculiar poison. That this mode of conveyance is so much more common in the latter than in the former disease, is to be explained chiefly by the naturally greater tendency to spread which scarlatina exhibits; but partly also by the fact that, whilst intestinal discharges, in consequence of their offensiveness, are got rid of as soon as possible, no special care is taken to remove the mucous discharges in cases of scarlatina. Allowance must also be made for the greater contamination of the atmosphere by the passage of the expired air over the surface from which the discharges come.

I hold, moreover, that the infectious nature of these intestinal discharges is not done full justice to by a simple recognition of their power to convey enteric fever from one person to another, but that their propensity to evil is coincident with their existence, and that they can, and often do, impart the specific inflammation of which they are the product to some of the unaffected glands with which they come in contact in their course down the intestine.

Some of the most striking and characteristic features of the intestinal lesion of enteric fever have hitherto baffled all attempts at explanation. The above view of the nature of that lesion suffices to explain them all, as I hope to be able to show.

Louis pointed out two different forms of intestinal lesion—the *plaques dures* and the *plaques molles*,—a distinction which subsequent observers have recognised. They are distinguished from each other chiefly by the amount of deposit in the submucous tissue, and by the appearance of the overlying mucous membrane. In the *plaques dures* the deposit is more abundant, and the mucous membrane is smooth; in the *plaques molles* the deposit is scanty and the mucous membrane has a rugose appearance.

A more careful examination of the causes of this difference is necessary to the full appreciation of the importance of the distinction.

In the *plaques dures* there takes place, in and around the glands, a morbid growth, to the presence of which is due some of their most striking physical characteristics. It imparts to them the firmness and density to which they owe the name given to them by Louis, and tends also to give to their mucous covering the smoother appearance which distinguishes it from the rougher mucous covering of the *plaques molles*.

The precise day on which this growth commences has never been demonstrated; and, indeed, never can be, as the most virulent case of enteric fever does not prove fatal before the characteristic lesion of that disease shows itself. The probability is, that both the morbid growth and the increased vascularity which accompanies it are contemporaneous with the commencement of the disease.

Regarding its nature there has been some difference of opinion. Some look upon it as possessing no specific physical properties by which it can be distinguished from other growths. Others describe a specific "typhous cell." I am of those who regard it as a morbid growth rather than a deposit, possessing no microscopic characters peculiar to itself, and giving evidence of its specific nature only by the pathological results which it produces.

The notion of the morbid growth being a deposit *sui generis* not improbably originated in the idea that the intestinal lesion is Nature's means of eliminating from the system the poison of enteric fever. If the opinion to which I have given expression regarding the nature of that lesion be correct, there is no necessity for the supposition that any morbid material is thrown out. It is sufficient to suppose that the poison circulating in the blood causes irritation and inflammation of the glands and the surrounding tissue, and that the additional growth is only the necessary result of this action—the inflammatory products which constitute it presenting no microscopic characters by which they can be distinguished from the usual products of a non-specific inflammation.

The number of glands primarily affected, the extent of the abnormal growth, and the rapidity with which it takes place, probably bear some relation to the severity of the attack. In very mild cases it may be very slight, and recovery may take place by resolution without ulceration of the local lesions. In well-marked cases, in which there is considerable deposit, ulceration invariably ensues. The mode in which this takes place is noteworthy.

It is not by a process of superficial ulceration, or by the gradual disintegration of the glandular structure; but the glands, the morbid growth, and the submucous tissue in which these are contained, along with the overlying mucous membrane, form a slough which, on separating, exposes an ulcerated surface. Though this sloughing process may extend through the muscular, and even the peritoneal coat, there is nothing in the situation of the morbid growth to lead to its going beyond the submucous tissue; and in most cases it stops at that point, reaching but not penetrating the muscular coat.

Let us contrast with the above the formation and course of the *plaques molles*.

We have just seen that they are distinguished from the *plaques dures* by the rougher state of the mucous membrane, by their scanty submucous growth, and by their mode of ulceration. The difference in the submucous growth is one of degree only, and not at all of nature. Its scantiness accounts in part for the less smooth condition of the overlying mucous covering, though the granular aspect seen there may be due to other causes connected with the mode of production of these *plaques molles*.

In the manner in which they ulcerate, we have at once their most striking feature, and the clue to the real distinction between them and the *plaques dures*.

In the latter, as we have seen, the process commences in the interior, and the whole mass sloughs out bodily.

In the *plaques molles* the process of ulceration takes place from without inwards. "The mucous membrane may become softened, and one or more superficial abrasions may appear on the surface of the diseased patch, which may extend and unite into one large ulcer, and this ulcer may proceed to various depths through the coats of the bowel, and even to complete perforation."—(*Murchison*.)

After the sloughs and debris have separated, it is not possible to say from which form of lesion the ulcer has resulted.

No explanation of this difference has ever been given; and yet there must surely be some reason why, in the same patient, the morbid process should show two distinct forms.

The view given above of the nature of the intestinal lesion offers a ready explanation.

The *plaques dures*, in which the mucous membrane is smooth, in which there is considerably increased growth in the submucous tissue, and in which the whole diseased mass sloughs away at once, are the glands primarily involved.

The *plaques molles*, in which the mucous membrane has a granular aspect, in which there is little morbid growth, and in which the process of destruction is from without inwards, are those secondarily affected by the discharge from the previously existing *plaques dures*.

To the *plaques dures* I would therefore apply the term *primary lesions*; to the *plaques molles* that of *secondary lesions*.

The recognition of these two forms of lesion has a most important bearing on the pathology of the intestinal affection.

The explanation of the difference between the primary and secondary lesions is, that in the former the inflammatory product is very slowly deposited in the glands and the surrounding submucous tissue. This usually goes on from ten to fourteen days before the inflammation seems to reach its height, and terminate in sloughing.

In the latter (secondary lesions) the course is much more expeditious. The virus is applied directly to the glands, and (as might be expected under such circumstances) there is at once set up a smart inflammatory action, which speedily terminates in ulceration, before there has been time for the glands to become more than slightly enlarged, and before there is thrown out sufficient inflammatory product to impart to them any of the external features of the early stage of the primary lesion. The mucous membrane at once partakes of the inflammatory action, and is quickly the seat of an ulcerative process, which rapidly involves the submucous tissue, and may also involve the muscular, and even the peritoneal coat.

It will be observed that, in the primary lesions, the process of destruction seems to commence in the submucous tissue, and involves in the slough all the superficial structures, but that there is no reason why it should go deeper; whilst in the secondary lesions it commences on the surface, and extends inwards in such a manner that there does not seem to be the same reason for its stopping short of the muscular coat. I shall afterwards have occasion to refer to my belief that perforation and profuse hæmorrhage more frequently result from secondary than primary lesions.

From the mode in which these secondary lesions are produced, it will readily be understood that they do not all come out at once, like the primary, but are developed in successive crops, or one by one, much depending on the situation of the glands which become so affected, and the facilities afforded by each for its inoculation. In this circumstance we have, I believe, the probable explanation of the protracted duration of the febrile symptoms in some cases of enteric fever.

Louis thought that the *plaques molles* (secondary lesions) were more frequent. Murchison regards the *plaques dures* (primary lesions) as more common.

There can be no rule in this matter; for it is evident that the relative extent to which these primary and secondary lesions exist in a given case, must greatly depend on the number and situation of the glands primarily affected. If these are numerous, there remain fewer to be secondarily involved, and the primary lesions are likely to predominate. If not numerous, and especially if situated high up, the secondary lesions may be a majority.

The higher up in the intestine the primary lesions are situate, the more numerous, *cæteris paribus*, will be the glands liable to inoculation by the discharges on their passage down the gut, and the more numerous, in consequence, will be the secondary lesions.

The reasons already given for my belief in the correctness of this view of the nature of the two varieties of intestinal lesion are as follows:—

1. It is consistent with all pathological analogy that the discharge from a specifically inflamed organ, applied to a like organ in a state of health, should there reproduce that specific inflammation to which it owes its origin.

2. The manner in which the primary lesions are formed is more compatible with a specific cause acting from within.

3. The manner in which the secondary lesions are produced is more compatible with a specific cause acting locally.

4. The manner in which each form of lesion ulcerates, tends to a similar explanation of the mode of production of each.

There are other reasons for holding this view, to which I would now direct attention.

It may be stated generally that the existence of primary and secondary lesions is essential to the explanation of the diverse, and hitherto inexplicable, phenomena of the disease.

To these I would refer under the following heads:—

1. The glands of the lower portion of the ileum suffer more than those higher up.

2. The extent of the intestinal lesion, as revealed by examination after death, bears no relation to the severity of the abdominal symptoms.

3. There occasionally occurs a distinct relapse, with a renewal of all the symptoms of the primary attack.

The glands of the lower portion of the ileum suffer more than those higher up.—No explanation in the least degree satisfactory has ever been given of this circumstance.

On the above view of the nature of the lesion it is very readily explained. Indeed, the greater involvement of the glands of the lower end of the ileum becomes almost a physical necessity; for, in the first place, they are more numerous there than at the upper part, and the discharge from an affected gland comes more readily and continuously in contact with the neighbouring ones; and, in the second place, the discharge from all those above passes over and comes in contact with those lower down. The ones not hitherto affected thus run the risk of having set agoing in them an action similar to that which exists in those from which the discharge comes. Of course, allowance must be made for the greater likelihood of primary lesions occurring in that locality, in consequence of the greater abundance of the glands; but something more is required to account for the very great increase of the glandular lesions found in the lower part of the ileum. This something we have in the recognition of the virulent nature of the discharges.

Careful examination of the diseased intestine tends to confirm this explanation of the more extensive disease of the lower part of the ileum. On examining the intestine from above downwards, it is found that the line of demarcation between the healthy and diseased glands is generally abrupt, and that, when once a diseased patch is reached, a healthy patch is rarely if ever found below it. Dr Murchison thus expresses it:—"At the upper part, the transition between the diseased and the healthy patches is usually rather abrupt, *and, proceeding downwards, after the first diseased patch, all are usually diseased.*" (The italics are mine.)

The transition between the diseased and healthy glands is abrupt because the discharge from the highest primarily affected gland passes downwards, tending to inoculate in its course those below, but not affecting those above it; and hence, "after the first diseased patch, all are usually diseased."

The discharges from the secondary lesions are, of course, as potent for evil as those from the primary.

The further down the small intestine we go, the greater is the amount of specific discharge in a given quantity of the contents, and the less the likelihood of any gland escaping.

At the same time the band of muscular fibres, arranged like a sphincter at the lower extremity of the ileum, probably contracts more vigorously in consequence of the inflamed condition of the intestine, and so tends to retain the accumulated discharges longer in contact with the glands situated nearest the ileo-colic valve. The glands in this locality, which may have primarily escaped, thus run an unusual risk of being secondarily involved. The contraction of this sphincter ilei (if I may call it so) I believe to be a ~~fact~~ of considerable importance; and am disposed to think that what is called cæcal gurgling would often, perhaps, be more correctly termed ileac gurgling—the sound being produced in the ileum rather than in the cæcum.

In the cæcum the discharges are also probably retained for some time, and hence the glands of that portion of the large intestine suffer more than those further on.

It is true that the greater extent of disease presented by the cæcum is partly to be explained by the greater abundance of the solitary glands in that locality; but the other agency which I have indicated is not altogether without its influence.

The solitary glands of the large intestine, as compared with those of the small, suffer very little, though one might naturally expect them to be frequently inoculated by the discharges on their passage downwards. Their comparative exemption is to be partly explained by the diminished risk which they run in consequence of the dilution of the intestinal contents by the secretions of the large and numerous tubular glands of the large intestine, the fluid poured out by which may, to a certain extent, also afford a protective covering to the mucous membrane, and facilitate the passage onwards of the contents of the bowel, and may even by its acidity tend to counteract some of the noxious properties of the alkaline contents of the small intestine. Their chief safeguard, however, is to be found in their small size and sparse distribution, it being evident that the risk of a portion of slough, or a drop of virus-bearing discharge, resting in contact with a small solitary gland in the capacious large intestine, is very much less than the chance of its lodging over a good-sized patch in the attenuated small intestine. For not every gland over which the discharges pass is injuriously affected by them. No doubt they come into momentary contact with many sound glands without doing them harm. For the display of their virulent properties, it

is necessary that time should be afforded to the gland to take up the virus. When this is done, its absorption, and the consequent participation of the gland in the local mischief, almost necessarily result.

It may be objected to this view of the matter that post-mortem examination of fatal cases shows the ulcers near the cæcum to be as far advanced as those higher up. There is reason to believe, however (as has already been indicated, and as will be more fully explained when the subject of relapse is considered), that a gland which becomes directly inoculated in the above manner, inflames and ulcerates very rapidly, and is in three or four days as far advanced in its pathological course as its neighbours which were primarily affected a fortnight earlier. Just as a second vaccine vesicle, resulting from a puncture made several days after the first one, comes to maturity at the same time as the first, so these secondary lesions rapidly get abreast of the primary.

The extent of the intestinal lesion bears no relation to the severity of the abdominal symptoms.—This also admits of a ready explanation.

The severity of the abdominal symptoms is judged of very much by the amount of diarrhœa.

In cases in which the bowels are moved several times a day, the sloughs and discharges are carried off as quickly as they separate, and thus do their minimum of harm. Where constipation exists, however, they are retained, and so have greater scope afforded to them for the inoculation of glands not primarily involved. There may thus be produced in these cases an amount of local disease greater than would have existed had the abdominal symptoms been more marked, and the discharges carried off more speedily.

It is by no means an uncommon occurrence for a case in which the symptoms have all been very mild, to be suddenly terminated by perforation and speedy death. Such cases are every now and then being recorded.

Sometimes the symptoms are so equivocal that not till the onset of fatal signs is the real nature of the ailment recognised.

Occasionally this accident occurs after the primary fever has ceased.

Perforation, indeed, seems to occur in two classes of cases—those in which, prior to the accident, there are really no abdominal symptoms, or in which they are very mild; and those in which they are severe. The majority of cases in which this accident happens belong to the former class. How is this? It seems very like a paradox to say that perforation, the acme of all that is severe in the intestinal complication, is most frequent in cases in which the evidence of that complication is almost or altogether awanting. Paradoxical as it appears, it is nevertheless true. The explanation I believe to be, that in such cases the perforation is the result, not of a primary, but of a secondary lesion. For if in these mild cases the number of glands primarily involved is comparatively small (as

I believe it is), there remains a larger number of sound glands liable to secondary inoculation; and as the risk of perforation is directly as the extent and number of the secondary lesions, it follows that the probability of that accident occurring, as a consequence of such a lesion, bears a direct relation to the mildness of the attack.

I have already referred to the different mode of production of the ulcer in the two forms of lesion, and have pointed out that, while in the primary there is (especially in the comparatively benign cases to which I now refer) good reason for supposing that the sloughs will not extend beyond the submucous coat; in the secondary, partly in consequence of the more active inflammatory process, and partly in consequence of the manner in which destruction of the tissues is effected, there are not the same grounds for expecting the ulceration to stop at a given point. It is not unreasonable, therefore, to suppose that, in mild cases of enteric fever, the primary intestinal lesion partakes of the mildness of the general symptoms; and that, when perforation occurs in such a case, it is due to a secondary rather than a primary ulcer.

It is a question also whether the lodgement of a slough in a harmless or healing ulcer may not set agoing a fresh destructive action, and so lead to perforation, without the formation of a secondary lesion in the true sense of the term.

This view of the mode of production of perforation is quite in keeping with the fact that the accident generally occurs low down in the ileum, where we have seen that secondary lesions are likely to be most common and most severe.

To perforation occurring in severe cases, it is not necessary to apply the same explanation. The primary lesion, in a virulent case, may produce that result; though it is not unlikely that, even in such cases, a secondary lesion may sometimes be the offender.

The probability is, that when the accident occurs before the seventeenth or eighteenth day it is the result of a primary ulcer; when after that time, of a secondary. In cases in which the peritoneum is laid bare by the extension of the ulcerative process through the muscular coat, perforation seems occasionally to be the direct result of accidental rupture of the denuded membrane.

What has been said regarding perforation applies equally to peritonitis.

I have a distinct recollection that in my own case, in which the intestinal and general symptoms were mild, there occurred, at the end of the third week, slight but distinct symptoms of peritonitis in the right iliac region. This was the only anxious feature in the case; and, I have no doubt, was the result of a secondary lesion approaching dangerously near to perforation.

For some weeks after convalescence micturition was followed by a disagreeable dragging sensation in the lower part of the abdomen, which I attributed to the formation of slight adhesions between the peritoneal coat of the bladder and that of the intestine.

Profuse hæmorrhage too generally results from a secondary lesion: to this more detailed reference will be made further on.

I hold, then, that the extent of the primary lesion does bear a direct relation to the severity of the attack; but that it is not possible after death subsequent to the twentieth day to tell, in even a majority of cases, which glands were primarily, and which secondarily involved.

An incidental reference has already been made to the possibility of the intestinal lesion terminating in resolution. This result I believe to be more frequent than is usually supposed.

There is a form of fever very common in this country, in which the symptoms are all mild, and occasionally remittent in character, and in which convalescence commences from the eighth to the fourteenth day. It is possessed of no distinct eruption, though one or two rose spots may now and then be detected. The bowels are usually regular or constipated. The pulse is not unfrequently natural, or only slightly quickened in the evening. The tongue is clean, or thinly coated in the centre. The morning temperature is seldom above 101°, and is often lower; that of the evening is one or two degrees higher. Not unfrequently the temperature is, apart from the patient's complaints, the only evidence of the existence of "fever."

So slight indeed are the symptoms presented by many of these cases that it is often difficult to induce the patient to keep his bed, or even to abstain from following his usual occupation. They sometimes so closely resemble a bilious attack, or some temporary derangement of the digestive organs, that the use of the thermometer is necessary to the formation of a correct diagnosis. To such cases is usually applied the name "common continued fever."

In a paper on the Thermometry of Enteric Fever, published in the *Edinburgh Medical Journal* for August 1868, I expressed my conviction, founded on careful observation of the symptoms, and especially of the temperature of a number of such cases, that they were really mild cases of enteric fever. Subsequent observation has tended to confirm this view. Indeed, I think it may be stated generally that a febrile attack which is too long to be febricula, which is neither typhus nor relapsing fever, which is not ague, and which is not due to local disease, must be enteric.

In a recent number of the *Lancet* (10th December 1870) a similar opinion is expressed by so high an authority as Dr Murchison.

If convalescence commences by the tenth or eleventh day, we may safely conclude that no ulceration has taken place. If the febrile symptoms continue after that time, the patient cannot be too carefully guarded; though I am disposed to think that in such mild cases resolution of the intestinal lesion is possible at any time during the second week.

Cases of enteric fever which prove fatal during the second week are very severe, and we know that in such ulceration seldom commences before the tenth day. In these mild cases it is not likely to commence for some days later; and the probability is, that in every one of them which recovers within the fortnight, resolution of the intestinal lesion has taken place.

A faulty diagnosis in such cases is fraught with considerable risk to the patient. For if mistaken for a bilious attack, and treated by purgatives, the irritation of the intestinal glands may be so increased as to greatly diminish the chance of its terminating in resolution.

This mistaking of the early stage of enteric fever for a bilious attack is no imaginary error. I have on more than one occasion seen the most disastrous effects produced by such a fault in diagnosis. We have in the thermometer so ready a means of discriminating between the two that its employment in doubtful cases should never be omitted.

In the early stage of such cases the symptoms are so slight and equivocal that the physician is seldom consulted before the end of the first or beginning of the second week, by which time the patient has usually had recourse to the ordinary domestic purgative remedies, possibly with the effect of causing increased inflammation, and consequent ulceration, of the intestinal glands, and so transferring the case across the boundary line which separates the so-called "common continued fever" from enteric fever usually so called.

It may be objected that, with the absence of all positive evidence of the occurrence of inflammation of the intestinal glands, we are not justified in assuming its existence.

I hold, however, that the occurrence every now and then of such a case as I have described in a house in which others are suffering from well-marked enteric fever, the occasional presence of ochrey stools and symptoms of intestinal irritation, the appearance in some of them of lenticular spots coming out in crops, and the identity of the mode of defervescence, as evidenced by the thermometer, suffice to indicate their real nature. Even in cases in which constipation exists, as it very commonly does, it is found that a dose of castor-oil, which in health would have no effect, is sufficient to produce free evacuation of the bowels.

The early period of those long-continued but mild cases of enteric fever which are frequently met with, and in which perforation occasionally occurs, differs in no respect from these cases of so-called common continued fever. There is positively no means by which, prior to sloughing of the glandular lesions, they can be distinguished. It therefore behoves us to treat every case as if resolution were possible, till we are quite sure that sloughing has commenced. It is possible, seeing that the difference between them is only one of degree, that the lengthy course of the former may be due to the (possibly accidental) termination in ulceration of one or more glands; and that the continuation of the febrile symptoms in their mild form may be the result of a series of secondary inoculations of healthy glands.

How far these secondary lesions tend to increase and prolong the febrile symptoms is an interesting subject of inquiry, but one regarding which no satisfactory conclusion can be arrived at.

They commence before the primary lesions have run their course, and hence, whatever symptoms they produce, are so mingled with those of the primary lesions that no distinction can be made between them. At the end of the third week of the disease both sets are equally well developed.

Though its exact effects cannot be determined, it is extremely improbable that so important a lesion should produce no constitutional symptoms; and I am disposed to attribute to the constitutional irritation resulting from the secondary inoculation of healthy glands, those variations (or rather the rises which cause those variations) in the pulse and temperature which so frequently occur in the course of enteric fever; and which, be it observed, are most common and most marked in mild cases, in which the secondary lesions are probably most numerous.

Whether any of the other usual symptoms of that disease (other than the intestinal) bear any relation to the successive invasion of the individual glands by the local disease, it is difficult to say; but I cannot help drawing attention to the fact that the eruption comes out about the time that the process of sloughing commences in the primary lesions, and that the successive crops of lenticular spots may possibly bear more than an accidental relation to the successive ulcerative lesions of the intestinal glands. It may here be noted too, that in cases of relapse the abdominal and other symptoms are all developed very rapidly, and that the eruption comes out often as early as the third or fourth day.

Be that as it may, there can be little doubt that, in some cases at least, these secondary lesions tend to prolong the febrile symptoms. Their existence after the cessation of the primary febrile attack serves also to explain the very gradual manner in which defervescence sets in, and the consequent difficulty which is sometimes experienced in determining the exact day on which convalescence commences.

How come they to prolong the febrile symptoms? Is it by simply causing that amount of constitutional disturbance which would result from a non-specific inflammation of the affected glands? Or are the constitutional symptoms increased in consequence of the re-absorption into the system of some of the poison of the disease?

I think that such symptoms as would result from a non-specific inflammatory action may fairly be attributed to these secondary lesions; but that they are lost in those of the primary fever, or only manifest themselves by those variations in the pulse and temperature to which reference has already been made. Their extent also must depend on the number of glands secondarily involved. If only one or two are thus affected, the resulting disturbance will be slight. If eight or ten are involved, it will be correspondingly increased. As it is impossible in any case to gauge the exact extent of the secondary lesions, it follows that we cannot, with any certainty, say what symptoms are due to them and what to the primary. But where we find all the symptoms of the disease unusually prolonged, we may at least suspect the existence of numerous secondary lesions.

It is probable that in mild cases which run a short course, but in which sloughing of the glands has taken place, the primary ulcers are situated low down in the ileum, and are consequently less likely to give rise to secondary lesions; and that those in which the symptoms are prolonged have one or more of their primary ulcers situated high up in the intestine, and in consequence suffer more from the inoculation of healthy glands by the discharges on their course down the gut.

I do not think that re-absorption of the poison during the continuance of the primary fever (supposing it to occur) is likely to be followed by an increase of the febrile symptoms greater than may be explained by the increase of the intestinal affection; any more than a second inoculation of vaccine matter, made before the first is mature, tends to increase the constitutional disturbance which accompanies the maturation of the vesicle.

That this belief in the possibility of the poison being again taken into the system through the bowel is not altogether groundless, however, will be presently shown when considering the subject of relapse, which I now proceed to do.

There occasionally occurs a distinct relapse, with a renewal of all the symptoms of the primary attack.—The occasional re-appearance, after a short period of convalescence, of the symptoms which characterized the primary attack, forms one of the most interesting features of enteric fever.

These relapses have not been much studied in this country. In France and Germany they have attracted more attention, and have been the subject of some very interesting papers; but, so far as I am aware, no satisfactory explanation of their occurrence has been given.

They are by no means of frequent occurrence. I have notes of 128 cases, of which 13 relapsed. Many men of large experience have never met with a single instance. I saw a large number of cases before I ever met with a relapse, and the 13 cases which I have seen all occurred within a period of two years; and most of them during one outbreak of the disease, spreading over a period of fifteen months. The first case which I saw occurred in my own person, and no doubt led me to take additional interest in the accident occurring in others.

The rarity of these relapses, and their greater frequency in some outbreaks of the disease than in others, lead to the suspicion, if not to the inference, that they form no essential part of the disease, but are accidental, and due to some peculiarity either of the primary attack or of the period of convalescence from it.

One or two cases are recorded in which a second relapse—*i.e.*, three attacks of the fever—occurred. Such an occurrence is extremely rare; but one instance (Case III. of Appendix) has come under my own observation.

They are commonly ascribed to errors of diet, no doubt in consequence of their generally occurring at a period of convalescence, in which it is customary to make some change in the regimen; the *post hoc* being regarded as a *propter hoc*. This belief is probably strengthened too by the fact that symptoms of gastric irritation generally exist at the commencement of the second attack.

That they are not due to such a cause, in every instance at least, I am quite satisfied, as I have on more than one occasion observed a true relapse in a case in which there had been no departure from the milk diet which formed the regimen during the primary attack. But, indeed, it requires no elaborate argument to prove that an error of diet is incapable of producing such a disease as enteric fever.

The mode of onset of the relapse is usually as follows:—After convalescence from the primary attack has gone on for ten or fourteen days, and when both patient and friends are congratulating themselves on his satisfactory progress, and perhaps even after he has been up and moving about for some days, there comes on a feeling of cold and general discomfort, accompanied by headache, and pains or dull aching in the limbs; the patient complains of thirst, and loathes his food, or is actually sick; vomiting may even be troublesome for several days. The tongue becomes furred in the centre, and red at the tip and edges. As during the primary attack, the expression is languid, the sclerotics clear and pearly, and the pupils natural or dilated. The pulse and temperature get up very quickly; and generally within twenty-four hours of the first feeling of discomfort, the febrile symptoms are well developed. Indeed, I am not at all sure that a rise of the temperature does not take place before the patient begins to complain. In one of my own cases at least, in which the temperature was noted morning and evening all through the case, without the omission of a single day, the range had been at or below the normal standard at both observations on the 43d, 44th, 45th, and 46th days, and had been from 98° to 100° for nine days before that. On the 47th day it was in the morning 98·4°, and in the evening 102·6°, and yet the patient did not complain. On the following day, however, he felt the usual symptoms of commencing relapse; the temperature was 101·2° in the morning and 104·4° in the evening. In this instance a rise in the temperature was the first symptom of a recurrence of the fever. It may be so in every case, but this is the only one in which the fact was demonstrated by actual observation. The probability is that the temperature begins to rise coincidently with the onset of the other symptoms.

In the second attack the symptoms are all developed more rapidly than in the first. The patient seems to be as ill after the first day as in the primary attack he was at the end of a week. By the time that the relapse has lasted twenty-four hours the pulse and temperature are often as high as at any period of the illness. Diarrhœa sets in very speedily, and the eruption comes out on the fourth or fifth day—maybe even earlier. Wandering and delirium too, when they do occur, set in early.

The second attack is shorter than the first, generally lasting for about a fortnight.

Defervescence takes place in a manner very similar to that which is observed in the primary attack. Whilst the general symptoms show signs of improvement, there occur the very remarkable morning fall and evening rise of the pulse and temperature, which characterize the first few days of convalescence from enteric fever. After the relapse, as after the primary attack, the temperature in the evening does not fall to the normal standard for some days after the commencement of convalescence.

Seeing that the second attack occurs while the patient is still suffering from the debility and exhaustion resulting from the first, it might naturally be expected that the second would be the more severe and the more fatal of the two. Such, however, is not the case.

Murchison indeed says that he usually found the relapse more severe than the primary attack. My experience is just the reverse. I have generally found the second attack to be the milder of the two. Certain it is, that the relapse very rarely proves fatal. All my own cases recovered. In Murchison's one fatal case, death was due to abortion.

"Post-mortem examination of fatal cases discloses the recent intestinal ulceration of the relapse coexisting with the cicatrices of the first attack; but as those glands only are attacked which formerly escaped, the recent lesions are usually less extensive than after death in ordinary cases."—(*Murchison.*)

There have come under my own observation, as I have already said, thirteen cases in which a true relapse occurred. (See Appendix.)

The first few, I thought, might be attributed to some indiscretion or error of diet, and accordingly took every care to guard against the operation of such a cause in the future. The patients were kept in bed for an unusual length of time, and the diet was limited to milk, or milk and beef-tea, till all danger was considered over. These precautions seemed to be of no avail, for in several cases in which they were taken a relapse occurred.

There seemed, indeed, to be no external circumstance which could at all account for their occurrence. With the object of finding out whether there existed in these cases any peculiar internal circumstance adequate to the production of such a result, I took the cases of which I had notes, separated those which had relapsed from those which had not, and carefully studied these records to see whether the former presented any peculiarities not observable in the latter. This inquiry was not, I think, altogether fruitless.

The mode of conducting it was as follows:—I dissected each case into the component parts or symptoms, whose sum total made the case; took separately those presented by the pulse, temperature, tongue, skin, nervous system, bowels, and viscera, and tried to find out whether any of these presented during the continuance of the primary attack, in cases which relapsed, any peculiarity not observable in those cases in which there was no recurrence of the febrile symptoms.

To give the details of this inquiry would be superfluous. Suffice it to say that it was entirely negative, that no peculiarity was found in any one system. The varieties presented by each symptom in the cases which relapsed were (relatively to their number) as great as in those which did not. Apart from the relapse, indeed, there seemed to be nothing to distinguish them from the mass of ordinary cases of enteric fever met with every day.

The primary attack may therefore be regarded as presenting no peculiarity, and as having nothing to do with the later re-accession of symptoms forming the relapse.

To the period of convalescence intervening between the two attacks we must look for any peculiar symptom, or aggregation of symptoms, capable of producing the effect referred to.

Here was adopted the same mode of investigation. The symptoms presented by the pulse, temperature, skin, tongue, nervous system, bowels, and viscera, in the interval between the two attacks, were contrasted with those presented by the non-relapsing cases during a corresponding period of convalescence.

It was found that the mode of defervescence was similar in both; and that neither the pulse, temperature, skin, tongue, nor nervous system presented, in the cases about to relapse, any peculiarity not observable in those in which no such occurrence took place. The diet also was the same in both.

In the condition of the bowels alone did any noteworthy difference exist.

In the non-relapsing cases (so far as the scanty notes of a satisfactory convalescence indicate) they were regular, and there was no necessity for the administration of laxatives.

In the cases which relapsed they were invariably constipated, and castor-oil had to be administered, in some of them two or three times, during the interval between the attacks.

In every case, without a single exception, this condition of the bowels was noted as having existed for some days prior to the onset of symptoms of relapse. The administration of a small dose of castor-oil was always the first part of the treatment.

This is the sole peculiarity (other than the relapse) which these cases presented in common. And in this condition of the bowels we have, I believe, a clue to the explanation of the occurrence of these relapses.

We have seen that the retention of the sloughs and discharges in cases in which the bowels are constipated during the primary attack, leads to the more frequent inoculation of healthy glands, and the consequent extension of the intestinal lesion.

That which occurs in the relapse is similar in character.

It is impossible in any case to fix the exact time at which the sloughs and debris from the glandular lesions are all discharged from the intestine; but there can be no doubt that they are not quite got rid of for at least some days after the febrile symptoms have declined.

When convalescence commences, therefore, there are still in the intestine both recently detached sloughs and some in which the process of separation is not complete, and from which a discharge still takes place. If the bowels are moved once or twice a day these are not allowed to remain in one locality, but are soon carried off, and in a few days no injurious matter remains. If, however, they are constipated, this noxious material is retained; and, if it happen to lodge over an absorbent gland, may be taken up into the system, and so produce a fresh attack of enteric fever.

The risk of this accident occurring bears a direct relation to the quantity of noxious matter in the intestine, to the degree of constipation which exists, and to the number of unaffected glands situated below the discharges and sloughs, and which in consequence run the risk of inoculation.

This explanation of the occurrence and mode of production of the relapse is in keeping with, and corroborative of, the view already advanced regarding the nature of the intestinal lesion, and the function of the affected glands. Indeed, it was a careful study of these cases of relapse which led me to the larger and wider generalization regarding the specific lesion of enteric fever to which I have given expression.

Their rarity is to be explained, partly by the rarity of decided constipation after an attack of enteric fever, but chiefly, I believe, by the protective influence of the primary attack.

That one attack of enteric fever confers immunity from a second, is generally believed; and I think rightly so; for though instances of a second attack are not wanting, they are comparatively rare.

A careful study of the primary attacks of cases which have relapsed, fails to reveal any reason why they should not exercise the usual protective influence. That they do not have this salutary effect, however, is proved by the occurrence of the relapse; for this is really a second attack of enteric fever, whose peculiarities are all to be explained by the mode in which it is produced.

I believe that in enteric, as in other forms of idiopathic fever, there is a certain number of individuals on whom one attack confers either no immunity at all, or only a temporary one.

Such individuals form but a small minority of mankind; and the chance of their contracting a second attack of typhus, scarlatina, or variola, bears a direct relation to the smallness of their number

plus the remoteness of the chance of their being a second time exposed to such an influence as produced the first attack.

In enteric fever the chance of a second attack bears a direct relation to the smallness of the number of individuals on whom one attack confers no immunity, *minus* such remoteness; for the chance of being a second time exposed to the influence of the poison of the disease (in its most concentrated form, too) is almost a certainty, seeing that that poison exists in the intestine during the early part of convalescence, and runs a great chance of coming into direct contact with some of those absorbent glands which are specifically affected by it.

The risk of a relapse occurring also bears a relation to the severity of the primary attack. The more severe that is, the fewer will be the sound glands remaining, and the less the risk of inoculation. In none of my thirteen cases was the diarrhœa severe, and in most of them there was none at all. In only one case was the administration of an active astringent called for, and there it was to check hæmorrhage.

It is quite possible that those cases in which a relapse occurs, though not proof against inoculation of the disease, may be so far protected as to be insusceptible to the ordinary influences by which a primary attack is produced; the immunity to the disease conferred by the first attack not being complete, but quite sufficient to protect from the ordinary chances to which they may be exposed after the intestine is quite free from virus.

The view which I take of the matter is, that the proportion of cases in which a relapse occurs represents with tolerable accuracy the percentage in which one attack does not confer complete immunity from a second. At the same time, I believe that the risk of a second seizure is considerably increased by the existence of constipation during the early period of convalescence from the primary attack.

This view of the mode of production of the relapse is borne out by the fact that such an accident never occurs in those mild cases of enteric fever in which the intestinal lesion terminates in resolution, though constipation often exists all through the attack, and for some time after.

The relapse differs from the primary attack in some important points, all of which are to be explained by its mode of production.

The period of incubation in enteric fever is uncertain, and varies in different cases.

In the relapse, as in the primary attack, its duration cannot be fixed; but seeing that the second seizure is produced by the inoculation of the poison in a concentrated form, one would expect the period of latency to be shorter than in the first. From the somewhat analogous case of inoculated variola also, such an inference might fairly be drawn. My own opinion is that it is very short.

We know that in some severe cases of enteric fever, the intensity of the poison seems to strike the patient down at once, even when it is received indirectly through the atmosphere. It is not, I think, unreasonable to suppose that the latent period may be nearly as short when the poison, though less virulent, is introduced by inoculation.

The period of invasion is also short.

It has already been observed that in the second attack the symptoms are all as fully developed in two or three days, as in the first they are in eight or ten. The temperature may then be as high as at any other period; the abdominal symptoms are already manifest; and the eruption is generally out by the fourth or fifth day—occasionally even earlier.

The greater frequency of symptoms of gastric derangement, which is usually observed at the commencement of the second seizure, is probably a consequence of the more rapid production of the intestinal lesion.

When once established, the symptoms present no peculiar features by which they may be distinguished from those of the primary attack. As a rule they are milder. This mildness may be partly due to the diminished scope for intestinal mischief, in consequence of the destruction of a number of glands during the first attack. Or it may be that, as is the case in variola, the inoculated disease is milder than the natural.

The duration of the febrile symptoms is shorter than in the primary seizure. They generally last from ten to fifteen days.

This diminished duration, however, is more apparent than real; and is to be explained, not by the striking off of the third week, but by the omission of the first; in other words, the symptoms are all so rapidly developed that the period of invasion is curtailed by seven or eight days, and the patient leaps at once, as it were, *in medias res*. This more rapid development of the symptoms may be due, as already indicated, to the mode in which they are produced; or it may be that they are accelerated in the same manner as the vaccine vesicle is in cases of re-vaccination.

The mode of defervescence is very similar in both.

At this point the question naturally arises, "Are the glandular lesions in the second attack primary or secondary? are they *plaques dures*, or *plaques molles?*" The lesion of that gland through which the poison was absorbed must be a *plaque molle;* but in others there is no such necessity, for if the relapse be a true second attack of enteric fever (as it most undoubtedly is), consequent on the re-absorption into the system of the poison of that disease, there is no reason why its characteristic lesion should not be reproduced with the other symptoms. Indeed, the re-absorption of the poison through a gland is a surmise not at all necessary to the elucidation of the causation of the relapse. It is quite possible that the poison may be taken up by some other portion of the intestinal mucous

membrane than that situated over a gland, the glandular lesion running exactly the same course as in the primary attack. The rapid development of the abdominal and other symptoms, however, seems to point to re-absorption through one or more glands. From these glands, and from any *plaques dures* which might be formed, would come discharges which would produce the same specific inflammation in any healthy glands with which they might rest in contact. Obviously, however, the chance of this mishap would not be so great as in the primary attack.

What practical lessons regarding the management of those suffering from enteric fever are to be drawn from this view of the nature of its most important and characteristic lesion?

There is an amount of scepticism abroad in the profession regarding the benefits to be derived from treatment in febrile diseases, which is as unjust to medical science as it is likely to be injurious to the sick. I believe that in no class of diseases is the watchful care of an intelligent physician, cognisant of all the dangers with which the patient is threatened, more necessary than in continued fevers.

In enteric fever, in which the risks are so insidious and so varied, the value of such care cannot be over-estimated. In no disease is a correct knowledge of its pathology of more importance in treatment, and especially is this the case as regards the intestinal lesion.

It may naturally be supposed that the views which I have expressed regarding its nature are not without their influence on the treatment which I think it necessary to adopt. They do influence it very materially.

First, with regard to the very mild cases in which resolution of the intestinal lesion may be hoped for.

It is of the utmost importance in them to avoid (*a*) everything in the least degree calculated to increase the general excitement, and (*b*) every possible source of intestinal irritation. For this reason the diet should consist of milk, with such farinaceous articles as arrowroot, sago, corn-flour, etc., and even these should not be given too freely. No other solids should be allowed. All animal food is to be eschewed. The patient must be kept quietly in bed, and should, to ensure compliance with the instructions given to him, be warned of the risk attending any departure from them. Should there be any tendency to diarrhœa, all solids should be omitted, and nothing but milk be given. Even beef-tea should be prohibited. The addition of lime-water to the milk (nearly equal parts of each) is not ungrateful to the patient, and is of use in consequence of its slight astringent properties. Of this he may drink freely. If the looseness continue, the administration of frequently-repeated small doses of Dover's powder, with an additional equivalent of ipecacuan, has a most salutary effect; from 1½ gr. to 3 gr. of Dover's powder, with ¼ gr. of ipecacuan, may be given every two or three hours.

It is seldom, however, that diarrhœa occurs. More commonly we have to deal with the opposite condition; and here the utmost nicety is required.

It must be borne in mind that some of the glands of the small intestine, and perhaps also of the large, are inflamed, and consequently more than usually prominent and tender. Under these circumstances the contact with them of the insoluble indigestible matters of which the fæces are composed, is to be avoided as much as possible. The less fluid the fæces are, the more likely are they to irritate the tender glands, and diminish the chance of termination in resolution. It is necessary, therefore, when the bowels are constipated, to counteract that condition by the administration of laxatives. For this purpose nothing is so suitable as castor-oil: one, or if necessary two, drachms should be given, so as to secure a stool, if possible, every day. The rule which I adopt in these cases is to give to an adult one drachm of castor-oil on the evening of every day on which the bowels have not been moved; if, by the morning, there is no stool, the dose is repeated. Occasionally it is necessary to give double that dose. Ipecacuan in frequently-repeated small doses—$\frac{1}{6}$ to $\frac{1}{4}$ gr., with a little sugar or aromatic powder, every two hours — is often beneficial, not only by its action on the skin, but by acting also on the mucous membrane of the small intestine. If there is much heat of skin it may advantageously be given in the form of wine, along with Mindererus' spirit.

Such are the means calculated to favour the termination in resolution of the intestinal lesion.

It is not very often that the physician has the opportunity to put them in practice, as the patient is generally ill for some time, and has undergone an injurious amount of medication at his own hands, before he applies to a medical man. But when in attendance on one member of a family suffering from enteric fever, the opportunity sometimes offers of treating such cases from the very commencement; and I feel that it is impossible too strongly to insist on the immense advantage which the patient may derive from the recognition by the medical attendant of the possibility of the glandular lesion terminating in resolution, and the early adoption of all measures calculated to aid in the production of such a result.

All our endeavours to attain this end may be fruitless, and the glandular inflammation may go on to sloughing and ulceration.

Even when this has taken place, much may still be done by judicious treatment to modify the severity of the intestinal lesion, and promote the patient's chance of recovery.

In order to know by what means this may best be done, it is necessary to bear in mind what has already been said regarding the nature of the intestinal affection, and the mode of production of the primary and secondary lesions, but especially of the latter.

The opinion generally entertained (founded on unsound views, or insufficient knowledge of the pathology of the intestinal affection) is, that looseness is an unfavourable, and constipation a favourable symptom. To check the former, and favour the continuance of the latter, are regarded, within certain limits, as the main objects of treatment, so far as the abdominal complication is concerned.

If all the lesions were primary this would be legitimate enough; but the recognition of the occurrence of secondary lesions alters the whole question.

All that can be done to prevent the primary lesions going on to ulceration has already been done.

Our object must now be to limit as much as possible the production of the secondary.

How is this end to be attained? Certainly not by shutting up in the intestine the sloughs and discharges which produce those lesions. For what is taking place during this quiescent state of the bowels? The sloughs are separating, the discharges continue to be given off by the primary lesions, and the healthy glands situated further down the gut are being subjected to the influence of a poison which is hourly increasing in strength and in quantity, so long as constipation exists. They are, in short, entirely at the mercy of a virulent agent, which is having everything its own way.

By encouraging constipation, therefore, we actually favour the production of what we fondly fancy we are guarding against—an increase of the intestinal mischief; for the longer these sloughs and discharges are retained, the greater is the chance of their inoculating the glands not hitherto involved.

To husband the patient's strength, to check any tendency to diarrhœa, and to guard against the dangers of the third week, are generally regarded as the great principles of treatment. The means usually adopted for these ends are calculated to increase the dangers of the third week, and to prolong them into the fourth and fifth. By encouraging constipation we do all we can to promote the formation of secondary lesions, which I believe to be more dangerous and troublesome than primary.

What we ought to do is to make ourselves sure that the sloughs and discharges are being regularly carried off from the bowels as they are formed. When constipation exists it should be overcome by castor-oil. One teaspoonful generally suffices for an adult. To females and children less should be given.

It is well to secure a stool every day. *Under no circumstances* should a patient be allowed to go three days without one.

I regard as most favourable those cases in which there are two motions in the twenty-four hours. When there are three, I do not interfere, unless they are very copious and watery. But when they go beyond that it is too much for the patient, and it is well to check them by means of astringents. For this purpose I usually employ the milder means already indicated. When these are insufficient, I have recourse to acetate of lead, dilute sulphuric acid, or solution of pernitrate of iron. The two last are my favourites, the iron being called into requisition when the acid fails in having the desired effect. The administration of an acid astringent seems to me particularly well adapted for a disease in which the intestinal contents have lost their usual acid reaction, and have become alkaline. The combination of a small dose of tincture of opium, or solution of morphia, with the acid, is often beneficial.

From what has been said regarding the causation of the relapse, it may be inferred, and rightly so, that the prevention of constipation during the early part of convalescence tends, to a certain extent, to diminish the risk of a recurrence of the febrile symptoms. It is well, therefore, that the patient, during the first few weeks of convalescence, should not go two days without a stool.

Be it ever borne in mind that constipation, though a less apparent, is perhaps as great a source of ultimate danger as diarrhœa.

A few words on intestinal hæmorrhage.

The opinion usually entertained regarding this symptom is, that it is one of the most formidable complications of enteric fever.

Graves and Trousseau, on the other hand, have recorded their belief that it is to be regarded as rather a favourable symptom. To a certain extent both opinions are correct, and the truth lies between the two.

Bleeding from the bowel is to be regarded as a trivial or as a serious event, according to the time at which it takes place.

Occurring in the early part of the case it is usually slight, and is generally the result of capillary oozing, rather than the giving way of any one vessel. Such hæmorrhage is often productive of temporary good, by relieving the congested condition of the mucous membrane. It need never cause any anxiety unless it continues for some time, or is accompanied by bleeding from other organs (the nose, stomach, kidneys, or skin), indicating a general hæmorrhagic tendency (*fièvre putride hèmorrhagique* of Trousseau).

Coming on late in the case, however, when the patient is much exhausted, when it is due to the giving way of some vessel, and when there is a risk of its recurring, perhaps more profusely, the appearance of blood in the stool is always calculated to cause anxiety. It may be that the quantity passed does little harm—good it cannot do then—but its occurrence at that time indicates the existence of a still active ulcerative process, the result of one or more secondary lesions, the destructive tendencies of which are greater than those of the primary.

So long as the mucous and submucous coats only are involved, profuse hæmorrhage is not likely to occur, there being in them no vessels sufficiently large for the production of such a result. When the muscular coat is invaded, however, the risk is much increased, for between the transverse and longitudinal fibres there run vessels of considerably larger size than those found in the submucous

tissue. These vessels run a great risk of being opened into by an ulcer which eats into the muscular coat.

This it is which imparts to even a slight hæmorrhage occurring late in the case an importance which would not attach to it at an earlier period. It indicates a still progressive secondary ulcer; and there is no saying where such an ulcer may stop. It may open into other vessels, or involve the peritoneum in its destructive course.

As a prognostic sign, I put such hæmorrhage nearly on an equality with slight peritoneal symptoms; it is a threatening of a still greater danger, and bears to profuse hæmorrhage much the same relation that peritoneal symptoms do to perforation.

Cases occasionally occur in which, after the symptoms have continued in a mild form for maybe three or four weeks, the patient, when apparently progressing most favourably, is suddenly seized with profuse hæmorrhage, which may even prove fatal.

The explanation already given of the mode of production of perforation in such cases, applies equally to hæmorrhage coming on thus suddenly and unexpectedly. The bleeding is caused by a secondary ulcer opening into one of the muscular branches to which I have referred, or at least into a larger vessel than is to be found on the mucous side of the muscular coat.[1]

From what has been said, it will be apparent that I regard the secondary lesions as a greater source of danger than the primary. When a patient dies of enteric fever within the first fourteen days, he is killed by the severity of the fever, and the result would be the same (in most cases at least) were there no intestinal complication at all. No doubt, cases do occasionally occur in which perforation and profuse hæmorrhage result from the direct destructive action of the primary lesions. These are exceptional. In the vast majority of cases in which death is to be ascribed to the abdominal complication, the fatal symptoms are manifested late in the case, when the primary lesions have reached the full extent to which they are likely to go, when the local danger arising from them may be regarded as over, and when the secondary lesions, though well developed, may still be progressing; for, as these latter are not all produced at once, like the former, those that are last developed may still be a source of danger, after all the primary and many of the secondary ulcers are clean, and in the way to undergo reparation.

Such is the view which I take of the nature of the intestinal lesion of enteric fever. I claim for it the distinction of being the only one which is capable of explaining the diverse phenomena of that disease. It shows how the stools carry the infection of enteric fever, without in any way exaggerating the importance of the local intestinal affection, or depriving that disease of its title to be ranked among the idiopathic fevers, and yet without detracting

[1] In the treatment of such an accident I should be disposed to try the subcutaneous injection of ergotine.

from the serious nature of the intestinal lesion. It explains why that lesion is greatest at the lower end of the ileum, and is quite in keeping with, and indeed serves to explain the fact, that the extent of the intestinal mischief, as revealed by examination after death, bears no relation to the severity of the abdominal and general symptoms. It serves also to explain the phenomena of the relapse.

I should have wished to have applied to this view of the nature of the intestinal lesion of enteric fever that crucial test which post-mortem examinations alone can supply. Since the above view presented itself to me, however, I have only had the opportunity of making one such examination. In it the highest lesion was a *plaque dure*, and, so far as one case could, it bore out my opinion. I must delegate to those who have more extended opportunities than I at present possess, the task of determining whether the highest lesion is always a *plaque dure*, and whether *plaques molles* ever exist prior to the commencement of the sloughing process in the *plaques dures*. If it is found that the highest affected gland is always a *plaque dure*, and that *plaques molles* are never found till the inflammation of the *plaques dures* has had time to go on to sloughing, I hold that my case is proved. If such is not found to be the case, I would yield the point that *plaques dures* and *plaques molles* are synonymous with primary and secondary lesions, and would look for some other explanation of the difference between them, but would still maintain, what is the chief point in this paper, that the recognition of primary and secondary lesions is essential to the explanation of the varied phenomena of the disease. In making such post-mortem observations, it must be borne in mind that, according to my view, a *plaque molle* cannot be formed prior to sloughing of at least one of the *plaques dures*, and that, subsequent to separation of the sloughs, it is impossible to say from which form of lesion the ulcer has resulted. It is obvious, therefore, that many post-mortem examinations can afford no evidence in the matter. The cases most likely to be available for this purpose are those in which death takes place about the beginning or middle of the third week.

The above facts may thus be briefly summarized:—

1. The intestinal lesion of enteric fever is specific in character.
2. It may terminate in resolution or ulceration.
3. When it goes on to ulceration there are two sets of lesions, primary and secondary.
4. The former are an essential part of the disease.
5. The latter are accidental, and the result of the inoculation of healthy glands by discharges coming from the former.
6. The recognition of these two forms of lesion is necessary to the explanation of the diverse phenomena of the disease.
7. Their relative frequency varies in different cases.

8. The extent of the primary lesions bears a direct relation to the severity of the attack.
9. That of the secondary bears no such relation, they being more likely to predominate in cases in which the general symptoms are mild, and the primary lesions few.
10. One primary lesion is sufficient to produce, directly or indirectly, many secondary.
11. The discharges do not necessarily inoculate every gland over which they pass.
12. The longer they remain in contact with a gland, the more likely is it to suffer.
13. The higher up in the intestine the primary lesions are situated, the more numerous, *cæteris paribus*, will be the secondary.
14. Fatal abdominal symptoms are more often the result of secondary than of primary lesions.
15. Relapses are caused by a re-absorption of the poison into the system, probably by one or more absorbent glands which escaped during the primary attack.
16. Constipation is to be regarded as a source of ultimate danger.
17. No one suffering from enteric fever should go more than two days without a stool.

Appendix.

I.—A male, aged 30, had a well-marked but mild attack of enteric fever. The eruption came out on the twelfth day, and presented its usual characteristics. The pulse ranged from 96 to 108, and the temperature from 102·5° to 103·8°. There was no tympanitis or abdominal tenderness: the bowels were somewhat relaxed, but there were never more than three stools a day, generally only two; they were ochrey, but not very liquid. There was no delirium, but the patient did not sleep well at night. He was put on milk diet. There was no other treatment.

On the twenty-eighth day defervescence began. On the thirty-third it was noted—"Continues to improve; has had no stool for three days; to have two drachms of castor-oil; fish for dinner."

On the thirty-ninth day he complained of chilliness, headache, pains in limbs, and a feeling of sickness. The skin was warm; the tongue slightly furred; pulse, 96; temperature, 100·8°. Stated that he had had no stool for several days; he thought not since last dose of oil. Was ordered two drachms of castor-oil. To have milk diet. The febrile symptoms recurred. A fresh eruption of rose-coloured spots came out on the fourth day of relapse. The pulse and temperature rose to about the same height as during the primary attack. The bowels became slightly relaxed on the fifth day, but were never so much so as to call for remedial interference. After continuing for fourteen days the febrile symptoms again showed signs of abatement, and the patient slowly but steadily convalesced.

II.—A female, æt. 32, had an attack of enteric fever, characterized chiefly by the mildness of all the symptoms. She was ill for a fortnight before she took to bed. When first seen she had a scanty eruption of rose-coloured spots; the tongue was thinly furred in the centre, and red at the tip and edges; the pulse was 76, and the temperature 102·4°; the bowels were said to be regular. She continued much in the same condition till defervescence began. The pulse ranged from 72 to 88, the temperature from 101·8° to 103·4°: both showed throughout the whole duration of the febrile symptoms a tendency to rise in the evening and fall in the morning. The bowels were never moved more than once a day; on several occasions a day was passed without a stool. These were never liquid. On what was thought to be the thirty-first day defervescence began, and continued for fourteen days.

On the fifteenth day of convalescence it was noted—"Complains of cold, headache, and a feeling of sickness. Skin warm; tongue slightly furred; pulse, 84; temperature, 101°. Has been up for an hour the last two days; for last three days has had chicken for dinner. Has had no stool for four days. Ever since convalescence began the bowels have been very costive. To have two drachms of castor-oil; and to go back to milk diet." All the symptoms of the primary attack recurred, and in much the same degree: rose spots reappeared on the sixth day of relapse in a very scanty manner, five being the highest number which existed at one time, and that only on one day—the ninth. On the thirteenth day defervescence again set in, and was progressive.

III.—A male, æt. 27, came under observation on the eighth day. On the tenth the eruption appeared. The case proved a smart one. The pulse was generally from 130 to 140, and on one occasion was as high as 160. The temperature ranged from 103° to 104·8°. Epistaxis occurred on several occasions, but to no great extent. The bowels were troublesome for a time, but were kept in check by lime-water, and frequently-repeated small doses of ipecacuan and opium. After the third week they were not so loose, and the ipecacuan and opium were omitted. The patient, however, was very weak, and required to be pretty freely stimulated, taking for some time a daily allowance of six ounces of wine and four ounces of whisky. The nervous symptoms were not a source of anxiety. He wandered at night, but was rational during the day. Defervescence began on the twenty-fourth day.

On the thirteenth day of convalescence it is noted—"Has been shivering this morning; complains of headache and general aching; has no appetite. Skin warm; tongue slightly furred; pulse (which had never fallen below 112), 128; temperature, 101·2°; has had no stool for three days. To have milk and beef-tea only for diet. To have two drachms of castor-oil."

The febrile symptoms continued; the pulse did not reach so high

a standard as during the primary attack, its highest being 136; the eruption came out on the fourth day of relapse: the bowels again became loose, but were easily kept in check by the means formerly adopted; the stools were pale and ochrey. On the nineteenth day of relapse defervescence again began, and the patient continued to improve for six days. On the seventh there was again a fresh onset of febrile symptoms. The condition of the bowels prior to this second relapse is not noted, but it is to be presumed that they were costive, as two drachms of castor-oil were ordered. The pulse and temperature again rose; the bowels became slightly relaxed; the stools were ochrey; and the patient had the circumscribed flush, the languid expression, the pearly sclerotic, and the dilated pupil of enteric fever. On the ninth day from the third onset of febrile symptoms, or the sixty-seventh day from the commencement of illness, convalescence again began, and continued uninterruptedly. No eruption appeared during the last attack.

IV.—A male, æt. 12, regarding the duration of whose illness no satisfactory information could be got, had a smart attack of enteric fever. The pulse was generally 132; the temperature varied from 103·1° to 104·5°; the eyes were clear, and the pupils dilated; the tongue was dry in the centre, and the lips cracked and bleeding; the bowels were relaxed, and the stools ochrey; there was no abdominal distention or tenderness; the nervous symptoms were marked; the patient wandered much at night, but during the day was quite capable of understanding all that was said to him, though he took no notice of what went on around him. No eruption was observed. Eight days after he came under notice, and probably about the twenty-fifth or twenty-sixth day of the fever, defervescence began.

On the eleventh day of convalescence, after the patient had been up for an hour or two, but before the diet had been changed at all, the following note was made:—"Has been sick this morning; complains of headache and a feeling of cold; skin warm; tongue furred in the centre; pulse, 120; temperature, 101·8°; the bowels have not been moved for several days; to have one drachm of castor-oil." All the symptoms of enteric fever were again developed; this time with the addition of the eruption, which came out on the third day of relapse. The nervous symptoms were less prominent, but in other respects this attack presented very much the general characteristics of the primary seizure.

On the thirteenth day defervescence again set in, and the patient made a satisfactory convalescence.

V.—A female, æt. 20, had a mild attack of enteric fever. The eruption came out on the tenth day; the pulse ranged from 96 to 108, the temperature from 102·1° to 103·3°; the bowels were regular throughout the whole course of the febrile symptoms, and the stools were of fair consistence. There was no delirium or wandering. On the twenty-second day defervescence began. On the ninth day of convalescence it was noted—"Complains of headache and loss of appetite; tongue slightly furred; pulse, 100; temperature, 101·2°; has had no stool for some days; has not been up, and has had no change made in her diet. To have two drachms of castor-oil." The pulse and temperature had much the same range as during the primary attack; the bowels were again regularly moved once a day, but the stools had not so much consistence. The eruption did not reappear. After continuing for fourteen days the febrile symptoms again declined, and the patient made a satisfactory convalescence.

VI. A female, æt. 26, had a well-marked attack of enteric fever, without any alarming symptoms. The pulse was never above 108, and was generally between 80 and 90; the temperature ranged from 102·3° to 103·6°. Both pulse and temperature showed all through the case a tendency to fall in the morning and rise in the evening. The eruption appeared on the twelfth day. There were neither nervous nor abdominal symptoms sufficient to call for notice. The bowels were generally moved once, sometimes twice a day, but never oftener than that; the stools were never liquid. Indeed the case was in all respects mild, though somewhat prolonged, and presented little variety from day to day.

On the thirty-first day defervescence began.

On the forty-fourth day, or fourteenth of convalescence, it was noted—"Complains of headache, loathing of food, and chilliness; tongue slightly furred; pulse, 96; temperature, 101·4°. No stool for four days. Has been up for a short time for last five days; yesterday had a chop; on the two previous days chicken for dinner." The symptoms were generally similar to those of the primary attack; the chief difference being a slightly more relaxed condition of the bowels during the relapse. The eruption came out on the seventh day. On the ninth day, and again on the eleventh, there was slight epistaxis. On the nineteenth day of relapse, or sixty-second from commencement of illness, convalescence began and continued.

VII.—A female, æt. 18, came under observation on the fourth day of what proved to be a well-marked attack of enteric fever. The febrile symptoms ran pretty high, the pulse ranging from 120 to 132, and the temperature from 103·3° to 104·8°. There was considerable wandering at night, but the patient was quite rational during the day. The bowels were relaxed, but never so much so as to call for the administration of any astringent more potent than lime-water. The stools were ochrey. The eruption appeared on the ninth day. Once or twice epistaxis occurred to a slight extent.

On the twenty-fifth day defervescence began. On the thirty-sixth (twelfth of convalescence) it was noted—"Has been shivering, complains of headache and pains in limbs; tongue thinly coated; pulse (which has never been below 112), 124; temperature, 102°. For two days has been up, and has had fish for dinner. Four days ago had two drachms of castor-oil, which operated once. Since then has had no stool; to have again the same quantity of oil, and to go back to milk diet."

The second attack lasted for fifteen days, and presented much the same features as the primary seizure. The bowels were relaxed, and the stools ochrey; the eruption appeared on the fifth day. The nervous symptoms were again marked, and set in very early. Convalescence began on the sixteenth day, and was satisfactory. Patient was greatly emaciated.

VIII.—A female, æt. 20, had a smart attack of enteric fever, the peculiarity of which was that, while the febrile symptoms ran high, there was little or no diarrhœa. The eruption appeared on the eleventh day. The pulse ranged from 128 to 144, and the temperature from 103·6° to 104·8°. The bowels were never in the least relaxed, and at times were rather costive. There was considerable wandering at night, but none during the day. The urine was slightly albuminous. On the twenty-third day defervescence began.

On the sixth day of convalescence she was ordered fish for dinner, and to have two drachms of castor-oil, the bowels not having been moved for several days. Two days later she got chicken for dinner.

On the thirteenth day of convalescence the note was—"Had slight rigors this morning; complains of headache, debility, and loss of appetite; tongue slightly furred; pulse, 120; temperature, 102·3°. No stool for three days; to have two drachms of castor-oil, and milk diet." Another drachm of castor-oil had to be given before the bowels were moved. The febrile symptoms increased; the pulse and temperature were not so high as during the primary attack; neither was there so much wandering at night. The bowels, though not loose, were not so costive as before. The eruption reappeared on the fourth day. The patient was much enfeebled, and required a daily allowance of six ounces of wine. This second attack lasted for seventeen days. On the eighteenth (or fifty-second from commencement of illness) defervescence again set in. Patient made a slow but satisfactory convalescence. The pulse was long in regaining the natural standard.

IX.—A female, æt. 28, had a sharp attack of enteric fever. The eruption came out on the twelfth day, and was very abundant. The pulse ranged from 120 to 132, the temperature from 103·2° to 104·5°. She wandered much at night, but was generally sensible enough during the day. There was no abdominal distention; but slight tenderness, on pressure, existed on the right iliac region. The bowels were loose from the first day that she came under notice (seventh of illness). At first they were kept in check by lime-water and frequently-repeated small doses of Dover's powder. On the twentieth day about two or three ounces of blood were passed by stool. By acetate of lead in three-grain doses every three hours the diarrhœa was checked, and the hæmorrhage did not recur. The patient was considerably depressed, and required from this time eight ounces of wine per diem. On the twenty-third day defervescence began. For ten days improvement continued. On the evening of the tenth day she was ordered a drachm of castor-oil, as the bowels had not been moved for three days. On the eleventh day she was suffering from the usual early symptoms of relapse. The oil had not acted, and she got another drachm, which had the desired effect. This second attack was decidedly milder than the first. The eruption came out on the fourth day, and was very scanty, generally only three or four spots. The pulse was much the same as during the primary attack, but it had fallen very little during the interval of convalescence; the temperature ranged from 103° to 104·1°. There was wandering at night, but to a less extent. The bowels were moved twice a day as a rule, and the stools were ochrey. There was cæcal gurgling, but no tenderness. On the fourteenth day of relapse (forty-sixth of illness), she again began to convalesce, and made a good recovery. The diet administered during the primary attack had never been departed from.

X.—A female, æt. 19, had a well-marked attack of enteric fever. The eruption came out on the ninth day. The bowels were never so loose as to call for the administration of astringents, but the stools were ochrey, generally two a day. The pulse varied from 96 to 108, and the temperature from 102·8° to 104°. There was slight wandering at night. There was little variety in the symptoms from day to day.

On the twenty-second day defervescence began.

On the ninth day of convalescence it was noted—"Complains of sickness, headache, and pains in neck and back; has been shivering; tongue thinly furred; pulse, 100; temperature, 101·2°. No stool for several days. Has had no solid food, except bread. To have two teaspoonfuls of castor-oil."

All the symptoms of enteric fever came back. The eruption came out scantily on the fifth day. The pulse and temperature were both much the same as in the first attack. The bowels were not so relaxed, but the stools were ochrey. There was again slight wandering at night. These symptoms continued for thirteen days. On the fourteenth, patient again began to convalesce, and made a good recovery.

XI.—A female, æt. 17, had all the symptoms of enteric fever well marked. The eruption appeared on the eighth day. The pulse generally ranged from 120 to 132, and the temperature from

103° to 104; once it was as high as 105·3°. The bowels were never troublesome; usually one, sometimes two ochrey stools a day. There was some tenderness on pressure in right ileum. At night she wandered a good deal, but was quite rational during the day. There is a difficulty in saying exactly when defervescence commenced in this case. At the end of the third week the morning temperature fell a couple of degrees for a day or two, and the expression was also improved; but the onset of parotid swelling seemed to have the effect of keeping up all the febrile symptoms. Not till the thirty-third day, when the parotid began to discharge, was there any decided amelioration of these. After the third week the stools were well formed, and the bowels so costive that castor-oil had to be given on several occasions. On the forty-third day it is noted—"Bowels confined; to have two teaspoonfuls of castor-oil." On the evening of the forty-seventh day, the temperature was 102·6°. On the forty-eighth day the note says—"Complains of headache and feeling of sickness; did not rest well last night; tongue slightly furred; pulse, 120; temperature, 101·2°; has had no stool for four days; to have a teaspoonful of castor-oil." The febrile symptoms recurred, the pulse ranging from 120 to 132, and the temperature from 102° to 104·4°. For several days patient was much troubled with sickness. During this second attack the bowels were so confined that an enema had to be frequently administered. On the eleventh day of relapse (fifty-eighth of illness) two rose-coloured spots appeared. These were the only ones observed during the second attack. Defervescence began on the seventeenth day of relapse (sixty-fourth of illness).

Of the two other cases I have not complete notes.

XII.—A female, aged 20, was during the primary attack under the care of Dr Harvey of Aberdeen, who informed me that the case was a well-marked one of enteric fever, with eruption and general symptoms proper to that disease. Immediately after recovery she came to Dundee. Two days after her arrival she had shivering, headache, and sickness, followed by febrile symptoms. I saw her on the third day of this illness. The skin was then hot; the tongue furred in the centre, and red at the tip and edges; pulse, 104; temperature, 102·5°. The bowels were freely moved shortly before she was seen, but had been confined for some days previous to that. There was slight bronchitic wheeze in both fronts. The eruption came out on the fifth day. The symptoms were all mild. The pulse was never above 116, nor the temperature above 103·8°. The bowels were slightly relaxed from the fourth day. On the thirteenth day defervescence began.

XIII.—This case occurred in my own person. I have no detailed notes of its progress from day to day, but as I was sufficiently sensible to watch its course with the interest which one is likely to take in his own case, I can give its leading features with tolerable accuracy.

It was a well-marked one. The pulse ranged from 120 to 132; the temperature was not noted; the tongue was furred and sometimes dry in the centre; the eruption presented its usual features; there were troubled dreams, but no distinct wandering at night; the bowels were generally moved twice a day, and for a time the stools were ochrey. During the third week there was considerable tenderness in the right iliac region. These peritoneal symptoms formed the most anxious feature in the case; and to a patient conscious of the danger, a very unplesant feature it was. Symptoms of improvement first showed themselves on the twenty-third day.

During convalescence from the primary attack, the bowels were so costive that a small dose of castor-oil had to be taken more than once; and I have a distinct recollection that a dose taken shortly before the symptoms of relapse appeared got the credit of having something to do with their production. After convalescence had continued for about a fortnight (of the exact time I cannot be sure), and after I had been up on several occasions for a few hours, and had been ordered a more generous diet, symptoms of relapse showed themselves. The pulse got up, the eruption reappeared, the bowels became slightly relaxed; and for about a fortnight I was again in the same condition as during the primary attack, except that the peritoneal symptoms did not recur.

The exact day on which convalescence began I do not know. On this occasion there was no interruption.

These scanty notes suffice to show that the second attack was in every case a true relapse, as worthy of the name enteric fever as the primary seizure.

The following table shows at a glance the chief points of interest presented by these cases:—

No.	Sex.	Age.	Duration of 1st attack.	Duration of Interval.	Duration of 2d attack.	Total Duration.	Day on which eruption appeared in 1st attack.	Day on which eruption appeared in 2d attack.
1	M.	30	27 days	11 days	14 days	52 days	12th day	4th day
2	F.	32	(?) 30 ,,	14 ,,	12 ,,	56 ,,	bef. 14th ,,	6th ,,
3	M.	27	23 ,,	12 & 6 ,,*	18 & 8 ,,	67 ,,	10th ,,	4th ,,
4	M.	12	(?) 25 ,,	10 ,,	12 ,,	47 ,,	None	3d ,,
5	F.	20	21 ,,	8 ,,	14 ,,	43 ,,	10th day	None
6	F.	26	30 ,,	13 ,,	18 ,,	61 ,,	12th ,,	7th ,,
7	F.	18	24 ,,	11 ,,	15 ,,	50 ,,	9th ,,	5th ,,
8	F.	20	22 ,,	12 ,,	17 ,,	51 ,,	11th ,,	4th ,,
9	F.	28	22 ,,	10 ,,	13 ,,	45 ,,	12th ,,	4th ,,
10	F.	19	21 ,,	8 ,,	13 ,,	42 ,,	9th ,,	5th ,,
11	F.	17	Uncertain	Uncertain	16 ,,	63 ,,	8th ,,	11th ,,
12	F.	20	,,	,,	12 ,,	Uncertain	Not known	5th ,,
13	M.	26	22 days	,,	Doubtful	,,	Uncertain	Uncertain
Average		22·6	24·27	10·9	14·5	52·4	10·3	5·27

* Two relapses.

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ON OBSTRUCTIVE SUPPRESSION OF URINE.

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Suppression of urine arises under two distinct classes of circumstances. In one it arises from disease of the renal tissue, or from some disturbance in the circulation or innervation of the kidneys, and not from any impediment to the discharge of urine. These have been called cases of *ischuria renalis*, or *anuria renalis*. Cases of this class occur in the various forms of Bright's disease, in the algide stage of cholera and ague, and in severe collapse or shock from any cause.

The second class of cases includes those in which there is no primary defect in the kidneys nor in their vascular or nervous supply; but in which the suppression is due to some mechanical obstruction in the ureter or pelvis of the kidney, which impedes the outflow of urine. These may be called cases of *obstructive suppression*. In the last three years I have encountered a somewhat unusual number of cases of this class, and their study has indicated certain points of clinical and pathological interest which appear worthy of attention. Three of these cases have already been published in the weekly journals,* but as they appeared separately, and at intervals, I have thought it desirable to bring them together, and to add to them the cases I have observed since, for the purpose of more comprehensive examination.

The most common case of obstructive suppression is the impaction of a stone in the ureter of a person who has only one kidney, or, at least, only one capable of secreting urine. Sometimes one of the kidneys is congenitally absent; or it has been

* 'Lancet,' 1868, i; Ibid., 1870, i; also 'Brit. Med. Journ.,' vol. i, 1868.

permanently disabled at some preceding period of life by the lodgment of a stone in its ureter, or by some other accident or disease. The next most common case is the blocking up of the terminal portions of the ureters by the progress of a morbid growth, involving the trigone of the bladder. The less frequent cases depend on some congenital malformation of the ureters, or of the renal arteries, whereby an impediment is constituted to the outflow of urine. This may be slight at first, but in process of time it becomes progressively greater, until at length it arrests the secretion of urine. Examples of these three modes of obstruction will be found among the following cases.

A case of suppression from obstruction seldom reaches a fatal climax without some urine having been voided during its course; it may be a few ounces, or it may be a few pints. The character of this urine is distinctive, and I wish particularly to insist on this as a diagnostic mark which serves to distinguish obstructive suppression from renal suppression, or ischuria renalis. In the latter class of cases there is a certain congruity between the state of the urine and the general condition of the patient. As a rule the urine is deeply coloured and concentrated, or it bears unmistakeable marks of the disease under which the patient is labouring —*i. e.*, it contains albumen and casts. But in obstructive suppression the urine which escapes past the obstruction is markedly pale and watery, and of very low density. It may accidentally be coloured by blood, but it is defective in the proper urinary pigment, and, as a rule, is free from albumen.

This peculiarity depends on the physical conditions under which the urine in these circumstances is secreted. In order to understand the matter clearly it will be necessary to call to mind the mutual relations in health of the blood circulating in the renal arteries and the urine newly secreted from it, and flowing down the uriniferous canals. In the normal state, the limiting membrane intervening between the blood circulating in the Malpighian tufts and around the convoluted tubes on the one hand, and the urine in the uriniferous canals on the other, is subject on the side of the blood to a considerable pressure, namely, the lateral pressure within the arterial system; while on the other side there is

no counter-pressure at all so long as the escape of urine is free.

This inequality of pressure, as was first suggested by Ludwig, and afterwards experimentally proved by Hermann,* is a capital factor in the production of the urinary secretion. Hermann (operating on animals) found that when the pressure within the renal artery was lowered the flow of urine was proportionately diminished. He tested this point in two ways. In the first set of experiments he lowered the blood-pressure in the kidney by contracting (by a clamp) the calibre of the renal artery. In the second set he created a counter-pressure in the uriniferous canals by impeding the flow of urine by means of a column of mercury communicating with the ureter. By this latter method he exactly imitated the condition produced when the ureter is blocked up by a stone, or some other mechanical obstruction. Hermann found that a pressure in the ureter of 10 millimetres of mercury (0·4 inch) caused a sensible diminution in the flow of urine; this diminution went on progressing up to a pressure of 50 millimetres; and with a pressure of 60 millimetres of mercury (2·4 inches) the secretion of urine was altogether arrested. In these experiments the specific gravity and coloration of the urine are not alluded to, but it was uniformly found that the per-centage of urea progressively diminished as the pressure in the ureter increased.

Basing our deductions on the clinical facts to be presently adduced, and fortified by the results obtained experimentally by Hermann, we may assume that a mechanical obstruction in the ureter will inevitably produce the following series of events:—As soon as the obstruction is established the urine begins to accumulate above it; the accumulating urine determines an upward pressure first in the ureter, then in the pelvis of the kidney, and ultimately in the uriniferous tubes. As the urine goes on accumulating the pressure within these channels necessarily increases, until at length the pressure so created is sufficiently great in the uriniferous canals to counterpoise the pressure within the renal blood-vessels. When this point is reached the secretion of urine

* Henle and Pfeufer's 'Zeitschrift,' 1862, p. 1.

is arrested and total suppression ensues. If, again, the obstruction be not altogether complete, and there be room for some urine to escape past the obstacle, the urine so escaping will have been secreted under a high pressure within the uriniferous canals, and its constitution will be found thereby materially altered; it will be very pale, watery, devoid of its proper colouring matter, poor in urea, and of low specific gravity. It may, indeed, be tinged with blood, but this is an accidental circumstance.

Another point with regard to the urine in obstructive suppression is the irregularity of its times of emission. In nearly all the cases this is a marked peculiarity. One day there will be an emission, the next day none, or perhaps none for two or more days, and then again a return of the flow, and again an arrest. This point will be again adverted to.

The long delay of characteristic symptoms is also a striking circumstance. When even the suppression is absolute, seven or eight days elapse before the special symptoms of uræmia make their appearance, but when these do appear the end approaches rapidly, and death is not delayed beyond two or three days. Up to the rise of the proper uræmic symptoms the condition of the patient is, as a rule, wonderfully calm and free from distress. There may be more or less gastric disturbance and insomnia, and declension of the muscular strength, but the functions generally proceed tranquilly, and the intelligence is undisturbed.

The most distinctive and invariable of the special uræmic signs are muscular twitchings. I believe that these are never wanting. Contraction of the pupils is also a constant sign, but later in its development than muscular twitches. Rapidly increasing muscular weakness is also constantly witnessed, and as this invades the respiratory muscles the breathing becomes markedly slow, panting, and laborious. The tongue and palate become quite dry in the two or three last days. The cerebral functions are much less involved than might be expected. There is increasing drowsiness, with short, fitful snatches of sleep, and a little rambling delirium, but absolute coma rarely supervenes, and convulsions are quite exceptional. The intellect is more commonly preserved to the last, and in more than one instance the patient has spoken sensibly the

instant before death. Diarrhœa (unless produced artificially) is quite exceptional, so likewise is excessive vomiting. There is never any dropsical symptom. The skin is commonly moist, often sweating profusely. There is never any ammoniacal or urinous odour from the breath or skin, nor from the body after death.* The power of taking food varies: as a rule it is moderate up to an advanced stage, but complete anorexia comes on a day or two before death.

There are some other points relating to the morbid anatomy—the survivorship and the treatment which will be more conveniently noticed in the way of comment on the particular cases, or in the concluding part of the paper.

Case 1.—A man, æt. 67, who twelve years before had suffered from symptoms of renal colic, but had not passed any stone, was attacked about six weeks before his death with symptoms of left renal calculus, with frequent micturition and pains in the left loin, &c. A fortnight before his death, after a long walk, he felt a sudden access of intense pain in the left loin. This continued in great severity for four days, and was accompanied with very frequent and scanty micturition. At the end of these four days the urine became altogether suppressed, and the pain ceased a few hours after. On the third day of complete suppression I saw the patient. He had absolutely no symptoms referable to the suspended urinary function; he was calm, free from pain, also from nausea and vomiting, without desire to void urine; pulse 80; tongue clean; skin dry; he had had no sleep for two nights. He was ordered a warm bath, a saline mixture, and to have the course of the left ureter well kneaded with the aid of a liniment. Next day (fourth day of suppression) he passed a pint of pale, limpid urine; he had perspired freely and slept some hours. On palpating the renal regions the right was felt to be flat and empty, contrasting with the left which presented its natural roundness and sense of resistance. The following diagnosis was made—absence or atrophy of the right kidney and impaction of a calculus in the left ureter.

* This seems a point of distinction from *retention* of urine.

On the next day (fifth) twelve ounces of urine were voided. It was clear, almost colourless, sp. gr. 1010, not albuminous, and contained 1·92 grains of urea per ounce. There was anorexia, thirst, nausea, and occasional vomiting, a slight sense of mental confusion, but no actual delirium; pulse 80; respiration 24.

On the following day (sixth) the same symptoms continued with intense restlessness and insomnia. Sixteen ounces of colourless urine were passed, sp. gr. 1011, containing 2·08 grains of urea per ounce. The following new symptoms also showed themselves—dryness of tongue at tip, contraction of pupils, and occasional hiccough. In the evening of this day six more ounces of limpid urine were voided; sp. gr. 1011; temperature in axilla 98·6°.

On the afternoon of the next day (seventh) a great change for the worse was observed. Pulse 80, irregular; respiration 20, laboured, long-drawn, interrupted; tongue dry and brown; frequent muscular twitches all over body; patient indifferent and drowsy, but answering questions intelligently; no urine for the last eighteen hours.

Death took place thirty-six hours after the last visit—exactly nine and a half days from the commencement of the suppression. The symptoms during this last period, as observed by Mr. Mellor, with whom I saw the case, were—increasing laboriousness and slowness of the respiration, which assumed a panting character; deepening indifference, but still he answered "yes" and "no" to questions addressed to him, though slowly and unwillingly; pupils contracted to pins' points; finally complete coma. There was a doubtful convulsive seizure immediately before death.

Autopsy.—Strong rigor mortis; body well nourished and quite free from urinous or ammoniacal odour. All the organs healthy except the kidneys and ureters. The *right kidney* was wholly converted into a fibrous mass, studded with cysts, and weighed two and a half ounces. The corresponding ureter was impervious throughout, and changed to a fibrous cord, which was thickened about the middle to double its width. This thickened part was solid and fibrous like the rest. No stone existed in any part of the ureter or kidney, but it was conjectured that the thickened part of the ureter had been the seat of an obstruction, and that

the stones, or whatever object had constituted the obstruction, had been subsequently removed by absorption.

The *left kidney* was much enlarged, it weighed ten ounces, and, on section, appeared dark and intensely congested. The ureter was as thick as a goose-quill, and distended with fluid. At its lower part were found three little oxalate-of-lime calculi about the size of hemp-seeds, and weighing altogether one and a half grains. One of these was tightly impacted in the terminal part of the ureter, where it passes through the coats of the bladder; this was the cause of the obstruction. The fluid imprisoned in the ureter amounted to three drachms, and consisted of grumous bloody urine. The pelvis of the kidney was only slightly dilated, and contained about two drachms of bloody urine.

The bladder contained about six ounces of pale dilute urine; its coats were healthy.

The course of events in this man appears to have been the following:—About a month before the patient came under observation three small calculi, which had been previously lying harmlessly in one of the infundibula, were dislodged, and fell into the pelvis of the kidney. Here they sojourned some four weeks, causing pains in the left loin and frequent micturition. At the end of this period they suddenly entered the ureter, and for four days, amid great suffering, continued their descent to its lower part. Here the foremost calculus became impacted, the pain ceased suddenly, the passage of urine was blocked up, and suppression ensued. Had the opposite kidney been intact no serious consequences would have followed. The healthy kidney would have become proportionately hypertrophied and performed double duty. But the right kidney was, by an untoward coincidence, practically non-existent. It had itself, as may be conjectured, many years before, passed through a train of events similar to that which had now extinguished the activity of its fellow.

The suppression of urine in this case lasted nine days and a half. During the first three days the suppression was complete. Then followed a period of four days, during which an aggregate quantity of fifty-four ounces of urine were voided. Finally, in the last two and a half days no urine was passed, but six ounces were found in the bladder after death, making a total of sixty ounces of urine secreted in nine days and a half. This seems at first sight a not inconsiderable quantity, and causes surprise that, suppression being so incomplete, life was not longer maintained. But on closer inquiry the suppression proves to have been more complete than at first appeared. The urine discharged was exceedingly dilute, its sp. gr. ranged from 1010 to 1011, and its proportion of urea was only about two grains per ounce; this gives a total weight of urea excreted in nine and a half days of only 120 grains, which is less than one fourth of the normal amount for a single day.

Case 2.—A very stout, tall man, æt. 59, suffered four years before from symptoms of the passage of calculi from the left kidney. Two small uric-acid stones were passed after several weeks of suffering, and then the symptoms subsided.

After four years of good health the patient was seized one morning, without assignable cause, with sudden pain in the right loin and urgent desire to pass water. The pain and urgency of micturition continued until the afternoon, and small quantities of bloody urine, amounting altogether to about half a pint, were voided at short intervals during the day. The stomach was irritable throughout the day. Towards evening the flow of urine ceased entirely and the pain diminished.

I saw the patient for the first time about fifty hours after the commencement of the suppression, with Mr. Grindrod, of New Mills; and I visited him daily until his death, which took place nine days and a few hours after the arrest of the urinary flow. During this period he only voided urine once, namely, two ounces on the fourth day, and none was found in the bladder after death. This specimen of urine was quite characteristic of obstructive suppression. Its sp. gr. was 1010; it contained a little blood and a slight corresponding trace of albumen. When the blood-corpuscles had subsided the urine had a pale straw colour, and the deposit contained, besides blood-discs, a large number of epithelial cells of a transitional character, resembling those of the pelvis of the kidney.

The case, which was closely watched throughout its course, presented a typical example of death from pure anuria. Dr. Garrod was telegraphed for from London, and joined our consultation on the fifth day of suppression.

For the first six days the symptoms were marvellously slight, and yielded but faint indications that one of the capital functions of the body was in absolute abeyance. The muscular strength had indeed declined, and the sleep was bad, but the patient was calm; his tongue, skin, and pupils were natural; there was little nausea and no vomiting after the fourth day; the intellect was unclouded; there was not the least urinous or ammoniacal odour about the breath or sweat; the pulse was steady, at about 72, the respirations 24, and the temperature scarcely varied from the normal limits. There was no desire to make water, scarcely any pain or tenderness in the right loin, and he continued to take a fair amount of nourishment. On the seventh day the characteristic symptoms of suppression began to show themselves. On this day occasional slight twitchings or pluckings of the muscles were observed on the trunk and limbs, and the tongue began to be dry. The insomnia, which had been a marked symptom from the first, became very distressing; he dozed frequently for short periods, and started on falling asleep and awaking. He took nourishment fairly, and had no vomiting or thirst, and only very slight and transient nausea.

On the eighth day the patient was still calm and quite free from mental confusion or indifference when fairly awake, but when left alone he was constantly falling off into a fitful doze, and awaking with a start. The muscular twitchings were more marked than yesterday, and the muscular weakness had increased greatly; nevertheless he was up and dressed in his bedroom for an hour and a half. The pupils were natural, and he took his food pretty well, a quart of milk, some cocoa and bread and butter, and rice pudding. The skin has acted profusely from the beginning in response to warm baths. No nausea or vomiting. A peculiar panting character of the respiration was noticed to-day, which became more and more pronounced until his death. The temperature also began to fall.

On the ninth day the patient's condition changed greatly for the worse. The insomnia and restlessness were most distressing; the twitchings of the muscles very frequent and severe; the tongue and mouth were perfectly dry; the pupils were decidedly contracted, though still sluggishly responsive to light; thirst was troublesome and the appetite quite gone; the weakness was so great that he could not walk without the help of two assistants; his legs had to be lifted into the bath. There was no persistent nausea, but he vomited after a compound jalap powder. Although his intellect was clear when he was roused (he transacted some business with his lawyer) there was marked indifference when he was left undisturbed, and he lapsed at once into a dozy state, lying with his mouth open and jaw half dropped, breathing pantingly with long pause between expiration and inspiration.

On the tenth day, at one p.m., the patient died, having lived for a little more than nine whole days from the onset of the suppression, and having voided in this interval only two ounces of a very dilute urine.

The incidents of the closing scene were very distressing. The weakness increased rapidly; the night was most restless; the patient was constantly getting up to have a stool, but voided nothing except a little mucus. The thirst, dryness of the mouth and the muscular twitchings went on increasing. At six a.m. the breathing became very embarrassed, threatening suffocation. He asked to be instantly raised on the side of the bed into a sitting posture. He then belched up a large quantity of flatus, and was thereby much relieved in his breathing. After a couple of hours he lay down again, but with his head raised. The power of his legs was now quite gone; he said he could not feel them. At nine o'clock the pulse was 80, respirations 15, very laboured and interrupted. The pupils were strongly contracted. The twitchings were incessant all over the body and limbs. The breathing becoming again more embarrassed, he was lifted on the side of the bed, and finally into his arm chair. His strength failed now more and more, and the breathing became more and more difficult, and the uneasiness and distress increased, dozing and starting incessantly. He remained in his chair until one o'clock, when he began to slide off, and while

about to be assisted up again, he asked to have his hands rubbed, and suddenly fell back dead. There was no coma or convulsion throughout. He appeared to wander at times through the night, but when his attention was roused, he showed unshaken consciousness and intelligence to the end. The character of his breathing in the last two days was peculiar, and became increasingly so as death approached. The inspiration became more and more prolonged and laborious, and expiration shorter and more panting, with a lengthening pause between. The respiratory difficulty, which appeared to be the immediate cause of death, evidently arose from the diminishing power of the inspiratory muscles.

The *post-mortem* examination was confined to the abdomen. All the organs were healthy, except the kidneys and ureters. The *right kidney* was enlarged and weighed 11¾ ounces. Its surface was dotted here and there with numerous black blood-spots; but the general appearance, both on the surface and on section, was pale mottled, decidedly anæmic-looking. It contrasted strongly with the dark, almost black congested kidney found in Case 1. The pelvis and ureter were not in the least dilated. They contained about two teaspoonfuls of blood-stained urine. A small uric-acid calculus was found tightly impacted in the lower part of the ureter, just above its entrance into the bladder. It was about the size and shape of a large hemp-seed, and weighed 1½ grains.

The left kidney was found completely destroyed. It was hollowed out into a lobulated sac, about as large as the healthy kidney. On cutting it open there escaped about five ounces of an opaque white fluid, exactly resembling new milk. This singular-looking fluid retained its milky appearance, even on long standing; it was found to consist of myriads of needles of urate of soda floating in a highly albuminous serum. The sac wall consisted of a tough leathery tissue, from one to two lines in thickness, quite devoid of any recognisable renal structure. The cause of this mischief was found at the entrance into the ureter, where the channel was completely blocked up by a uric-acid stone, weighing 52 grains. The rest of the ureter was pervious and normal.

The bladder was empty and healthy. The body generally was perfectly sweet and free from any urinous or ammoniacal odour.

The pathological story of this man's case was easily read even during life, and only a few details were left to be filled in at the autopsy. The left kidney was destroyed four years before by the impaction of a calculus in its ureter. The right kidney then became hypertrophied, and performed double duty in a perfect manner until another calculus blocked up the right ureter. Then the secretion of urine was suddenly and permanently arrested, and the patient destroyed in less than ten days.

In reviewing the symptoms in this case it may be observed that insomnia and progressive failure of the muscular strength marked the entire course of the case. A certain disturbance of the stomach and slight febrile movement set in when the stone was impacted in the ureter; but these passed away after the fourth day. A fair amount of nourishment was taken up to the eighth day, after which the power of taking food almost wholly failed. The movements of the pulse, respiration, and temperature, may be seen by a glance at the following table:

	Pulse.	Respiration.	Temperature.
Third day	72	—	—
Fourth day . . .	72	24	100
Fifth day	72	24	99·7
Sixth day	72	24	99·7
Seventh day . . .	76	20	98·6
Eighth day . . .	76	22	98·2
Ninth day	76	20	97·4
Tenth day	80	15	—

The pulse remained almost stationary, but with a slight tendency to increased frequency. The respiration showed a tendency to diminished frequency, especially toward the last. The temperature manifested a steady tendency to diminution, especially as death approached. This, I believe, will be found to be the general rule in uræmia. Muscular twitches were first noticed on the seventh day. At first they were slight and infrequent, but they became more and more frequent and severe as the case approached its termination. The faculties were clear to the last gasp; there

existed, however, in the last three days a constant tendency to lapse into indifference, with fitful dozing and starting, when the patient was left undisturbed. The pupils did not show decided contraction until the ninth day, and dryness of the tongue and mouth became a marked feature on the same day.

This case and Case 1 illustrate a noteworthy point in the morbid anatomy of obstructive suppression. In both of them it is noted that the ureter above the obstruction, and the pelvis of the kidney, although moderately filled with stagnant urine, were not materially dilated or enlarged. Those examples of monstrously enlarged ureter and pelvis (sacculated kidney or hydronephrosis) which are often witnessed as the effects of obstruction in the ureter are produced by slow degrees, and must be regarded as a growth rather than a simple dilatation. Indeed, the ureter and renal pelvis appear incapable of that rapid dilatation which we are familiar with in the bladder. This consideration enables us to explain how two different results may follow one and the same course, namely, obstruction in the ureter. When the obstruction is suddenly established and is at once complete, the consequence is not enlargement and sacculation, but atrophy of the kidney and ureter. When, on the other hand, it is slowly established and incomplete, it produces hypertrophic dilatation of the ureter and pelvis, and eventually sacculation of the kidney or hydronephrosis.

Case 3.—A man, æt. 40, had suffered three months before from symptoms of renal colic on the *right* side, and voided some small calculi. He soon recovered from this attack, and went about his business in his usual health, until three weeks before his death. He then began to suffer from pain in his *left* loin, which continued for a fortnight. During this period the urine was voided in apparently the usual quantity, but his wife noted that it had entirely changed its character. Before it had been high coloured, but now it became "clear as water." At the end of the fortnight complete suppression of urine came on, and death ensued in five days.

I only saw this man once, on the day before his death, in consultation with Mr. Edwards, of Grosvenor Square. He was then in full uræmia; pupils contracted to pins' points; muscular twitchings universal over the whole body; breathing panting, slow, and interrupted; tongue and mouth quite dry. He was very restless, and almost indifferent, yet he answered questions sensibly when roused. He died next day without coma or convulsions; he spoke sensibly half an hour before his death.

Autopsy next day.—The body was quite free from urinous or ammoniacal odour, and healthy in every part except the urinary organs.

The *right kidney*, which was about the normal size, was hollowed and in process of atrophy; the cortical substance alone partially remained, and this was pale and wasted. The infundibula were moderately distended, and contained about an ounce of pale fluid, which was lost. The right ureter was plugged up at its commencement by an elongated uric-acid stone weighing twenty-two and a half grains. Another little stone, as big as a hemp-seed, lay in one of the infundibula. The ureter below the plug was normal.

The *left kidney* was much enlarged, but healthy. It had the mottled appearance of the right kidney in Case 2. Three little uric-acid calculi, like flattened mustard-seeds, lay free in the infundibula. The ureter and pelvis were moderately distended with fluid; the ureter appearing about the size of a crow-quill. On slitting it open superficial abrasions were seen along its entire track, showing the footsteps of a descending calculus. Near the bladder this calculus was found, at the termination of the ureter. It slipped into the bladder during the manipulations. It was a round uric-acid stone as large as a small pea, and weighed one and a half grains.

The bladder was empty and healthy.

Though this case was seen but once the diagnosis presented no difficulties. The course of events was evidently as follows:—Three months before the fatal attack the right ureter was plugged by a calculus, the function of the right kidney was thereby permanently extinguished, and the organ at once passed on to a state of atrophy, which was nearly complete at the time of death. The left kidney

then took up the double duty, and became proportionately hypertrophied. The calculous tendency, however, was not arrested, and about three weeks before death a small calculus passed into the left ureter. It continued to descend, amid much suffering, for about a fortnight, causing partial suppression of urine. The urine voided during this period had the special characteristic of urine secreted under pressure from below, *i. e.* it was pale and watery. At the end of the fortnight the calculus had reached the terminal portion of the ureter; there it became immovably impacted, complete suppression ensued, and death followed in five days. It must be assumed in this case that during the fortnight of partial suppression a certain degree of blood-poisoning took place from the accumulation in the blood of the effete ingredients, which should have been removed by the kidneys, so that when the suppression became complete it only required five days (instead of nine or ten) to render the blood poisoned to such a degree as to be incompatible with the maintenance of life.

Case 4.—A man, æt. 65, had been subject for some years to attacks of renal colic, and had from time to time voided uric-acid calculi. Some fourteen days before my visit symptoms of left renal colic had set in, with pain in the loin and frequent micturition. I was informed that during these fourteen days a considerable quantity of pale, clear urine had been voided, averaging altogether about two pints a day, but discharged irregularly. On some days none had been discharged, while on other days it had flowed copiously at two or three separate micturitions.

When I saw the patient he was in the last phase of uræmia; the pupils were strongly contracted; there were frequent and universal muscular twitchings; pulse 100; respirations 16, markedly panting, but consciousness was intact when the attention was roused.

The hypogastrium being protuberant and dull, a catheter was introduced by Dr. Jepson, with whom I saw the case, and two pints of urine were withdrawn. This presented the usual characteristic of obstructive suppression, it was very pale, and its sp. gr. 1006.

Death took place on the fifteenth day of suppression, which, however, had only been partial throughout. A post-mortem examination was not permitted, but it was not difficult to divine what had occurred. The right kidney had doubtless been destroyed at some previous period by the impaction of a calculus in its ureter. The left kidney, which had then become the sole organ of the urinary function, was in its turn subjected to a similar accident; a calculus entered its ureter and failed to clear the passage into the bladder, incomplete suppression ensued, and death in fifteen days.

This case is instructive in one respect, and suggestive of a caution in judging of the amount of urinary secretion. This man voided on an average about two pints of urine daily. Had this amount been of normal density and appearance, it would have indicated a degree of renal activity certainly equal to the prevention of uræmic poisoning. Patients may live for months without voiding more than fifteen or twenty ounces of urine a day, as is frequently witnessed in cases of cirrhosis of the liver and in regurgitant heart disease. But in these cases the urine is always of high density, deeply coloured, and fully charged with urinary ingredients. Here, on the contrary, the urine was pale and dilute, and the density of the specimen examined was only 1006. What amount of normal urine this represented cannot be accurately determined, but judging by the result of my analysis of the urine passed under similar circumstances in Case 1, the urea would not amount to more than about one grain to the ounce. Calculating on this basis, this man excreted only forty grains of urea per day, which is not more than one tenth of the normal amount. Another point in the case deserves notice as being more or less constantly characteristic of the mode of emission of urine in obstructive suppression; this was the irregularity of the times of discharge. Although the patient in this case discharged an average quantity of two pints a day, this was not voided with that approach to regularity which marks the normal state, but most irregularly; one day no urine at all would be voided, the next day it would be voided copiously two or three times, then again none at all for two or three days, and so forth. I have

noticed this paroxysmal character of the urine-discharge in all my cases of obstructive suppression, and I believe it to be a point of considerable diagnostic value.

The two following remarkable cases show that recovery is possible even after very protracted suppression of urine, provided the flow of urine can be re-established. The notes of the two cases were furnished to me by Dr. Clifford Allbutt, of Leeds, and Dr. Duigan, of Gainsborough, respectively. In the first case the suppression continued for nearly ten days, and in the second for nine days. In neither case were twitchings of the muscles noted, but the pupils had become contracted in Dr. Allbutt's case, and there was some mental confusion. From my own experience I should regard muscular twitchings as *the* first really undoubted and characteristic symptom of uræmic poisoning; it cannot, therefore, be said that recovery followed in either case after the full declaration of uræmic symptoms. Another apparently well-authenticated case of recovery after nearly ten days' total suppression, of obscure nature, is recorded in the tenth volume of 'Edinburgh Medical and Surgical Journal,' p. 409.

Mr. W—, a healthy vigorous man of about 56, was first seen by Mr. Wheelhouse, on Wednesday, September 11th, 1867. He complained of great lumbar pain, weight, sense of fulness, sickness, and febrile disturbance.

Monday, 16th.—Symptoms of descent of calculus along ureter commenced.

Saturday, 21st.—During this time stone apparently traced along ureter.

October 2nd.—Stone from last date till now seemed to be impacted at entrance into bladder, constant pain augmented in paroxysms till 3 a.m. this morning, when sudden and entire relief was felt, and the patient was told how to look for symptoms of stone in the bladder. At 6 a.m. he passed the last quantity of urine, about ℥ij. Up to this time the flow had been free and the fluid normal.

3rd, 9 a.m.—No urine passed. Catheter used, but no obstruction found. Bladder quite empty. 3 p.m.—Same state.

Perfect freedom from pain, no urine. No symptoms of uræmia. 10 p.m.—Consultation with Dr. Allbutt. Same state. Temperature 100°. Hot bath and fomentations ordered.

4th (Friday), 9.30 a.m. — Same state. No urine. No uræmia. Much local uneasiness and restlessness. Temperature 98°2. Fomentations, saline purgatives and diluents. Bromide of potassium with a little iodide given as a sedative, opium being inappropriate. 9 p.m.—Same state. A drop or two of urine had been coaxed out, just enough to make a stain at the bottom of a small vessel. No symptoms of poisoning. Patient quite clear and much more comfortable.

5th.—Mr. W. summoned at 5 a.m. Much pain at the old point; cramped limb of same side; not a drop of urine though frequent solicitations; firm pressure on part gives relief. Sp. Æth. Sulph. ordered every half hour. 8.30—Seen with Dr. Allbutt. Pain subsided after a few doses of ether; no urine; breath sweet; perspiration normal. On examination whole left side of belly from middle line dull; left rectus tense; dulness varies a little with position. Patient clear and intelligent; no drowsiness. Ether and bromide omitted. 3 p.m.—Same condition; pain returning; no urine; no uræmia. 9.30—Seen with Dr. Allbutt. Physical examination :—Dulness over whole of hypogastrium below a cross line drawn through the navel; dulness little affected by position. Examination per rectum showed only a tender spot behind the prostate; no bulging; catheter passes freely, and is moistened with a few drops of urine, perhaps twenty or thirty drops; upon the end of it is a little bloody mucus. Breath decidedly urinous; mind clear; no headache. Pulse weaker and quickening a little. Pulse and temperature have been normal.

6th, 9.30. — Pulse 96, better; temperature 98°2. Had passed a fair night; no urine. Dulness of belly extends a little above navel on left side, but not extending so far to the right as yesterday. Breath *not* urinous. Bowels have been kept open by salts till to-day, when no motion was reported. 9.30 p.m.—Singularly clear in head; placid sleep for five hours. Two watery stools. No urine, unless it be a very few drops passed after

repeated efforts; is cheerful, and walks about the room easily, and is well able to sit down and rise. A little cough which he has seems to shake and hurt the lower belly. Tongue coated, but food taken fairly in small quantities. Has had for instance a little partridge to-day. Pulse and temperature normal. Breath sweet. Ankles not puffy. Dulness all over hypogastrium.

7th, 9.30 a.m.—Good night. Pulse natural. Temperature 97°. No stupor or headache. Sense of a moveable tumour in lower abdomen. A few drops of urine, perhaps a teaspoonful, accumulated after repeated efforts. 10 p.m—Complains of weight at lower belly on left side, and pain there on coughing. Sickly during the day. Pulse and temperature normal. No uræmic symptoms.

8th, 9th, and 10th.—Same report, unless there be a little drowsiness and tendency to be a little "lost" at times.

11th.—This morning a little urine was passed, quantity not recorded. There is a good deal of mental oppression, especially after awaking. Aspect dull and heavy. Pupils contracted. Dulness of abdomen about the same; it is a little increased on left side, but diminished a little to the right. He has been purged today without medicine.

12th.—Has passed ℥ivss of water, and there is a little less mental obfuscation. Has had a warm bath, which relieved him in every way. Is still purged also, an action which is not prevented. Tongue loaded, appetite nil. Temperature normal.

13th.—Marked improvement; a copious flow of urine last night. The head clear; a refreshing night. Some return of appetite. Abnormal dulness much diminished.

14th and 15th.—A good deal of pain, dragging and paroxysmal; chiefly in the old place, above and to left of pubis; is irritable and restless; expression worn and anxious. There is no pain at the end of the penis. Pulse 100, weak. Temperature 100. As the water is now very abundant, we are able to give him champagne and morphia injections, which with warm water baths relieve him. Is still purged.

16th and 17th.—Pains cease. No stone is discovered. Convalescence.

21st.—May be considered well. Functions normal. Appetite good. No dulness in abdomen.

I strongly suspect that the suppression in this man was not due to the impaction of a calculus in the ureter, as seems to have been the impression of Mr. Wheelhouse and Dr. Allbutt, but to the existence of a double hydronephrosis, and that the case was similar, pathologically, to one which fell under my notice some three years ago, and which will be related presently (see Case 7). Temporary suppression of urine, extending over some days, followed by copious flow of urine, is a distinctive feature of cases of hydronephrosis; and the extensive dulness in the abdomen, which disappeared after the urine began to flow, can (the bladder being empty) scarcely be otherwise explained.

In the next case, however, the suppression was undoubtedly due to the impaction of calculi in the ureter, and ceased when the stones were voided.

Case 6. (From the notes of Dr. Duigan.)—The patient was a strong, stout, middle-aged cattle-jobber, living in the country. He had often suffered from renal colic and had frequently passed uric-acid calculi. The attack began with pain in both loins, and the patient had had complete suppression for three or four days when first seen by Dr. Duigan, in consultation with Dr. Smallman of Willingham. The pain had then completely subsided, and, except for loss of appetite and the suppression, the man presented no marked symptoms. The introduction of a catheter showed that the bladder was empty. For nine days he continued in this state, never passing any urine for all that time, and not suffering from any bad symptoms, sickness, or other indication of uræmic poisoning. At the end of this period the kidneys began to act, and he passed a quantity of clear urine of low specific gravity, containing nothing abnormal. With this urine he voided three or four uric-acid calculi, and shortly after got quite well.

In this case it is probable that one kidney had been destroyed at some former period by the impaction of a calculus in its ureter; at the same time it is not absolutely impossible, as Dr. Duigan suggests, though, I think, highly improbable, that both kidneys may have been sound, and that both ureters were obstructed by calculi at the same moment.

Case 7.—A youth of twenty had suffered since boyhood from recurrent attacks of intestinal obstruction extending over four or five days, then relieved by copious alvine evacuations. I was called to see him in one of these attacks with Mr. Jonathan Wilson and my colleague, Mr. W. Smith. There had been no stool for five days, and the urine was reported to be exceedingly scanty, amounting to no more than six or eight ounces in the twenty-four hours.

On the next day (March 1st) he passed four ounces of urine, it was discoloured with blood, and its sp. gr. was 1008. On examining the loins both were found dull on percussion, bulging and elastic, quasi-fluctuating. The intestinal obstruction still continued. The opinion was hazarded that the patient was the subject of double hydronephrosis, in other words, that the kidneys were hollowed out and distended with accumulated urine, which was unable to escape from some long-standing impediment in the ureters. The constipation, it was conjectured, was occasioned by the pressure of one or other distended kidney upon some portion of the colon adherent thereto.

On the following day three ounces of similar urine (except that it was freer from blood) were voided, but on the succeeding day no urine was passed.

At this date (March 3rd) the condition of matters was as follows:—the bowels had not acted for eight days; for the first five days six to eight ounces of urine had been voided, on the sixth day four ounces, on the seventh day three ounces, and none at all on the eighth day. On the ninth day several very copious discharges of urine took place, amounting altogether to more than eight pints. These specimens of urine had all the same character, they were pale and watery, sp. gr. ranging from 1005 to 1007, free from albumen, and only containing microscopical quantities of blood. A sensible softening of the abdomen had now taken place, and the elastic swelling on the left side was very decidedly diminished. Considerable relief followed, and the patient slept several hours, but the bowels still remained unmoved in spite of enemata of oil and gruel and repeated kneading of the abdomen.

On the tenth day great discharges of urine took place, fully eight pints altogether. In character it exactly resembled that of yesterday. At midnight the bowels yielded, and enormous quantities of semi-liquid fæces were evacuated.

On the eleventh day several copious motions took place, and it was now hoped that recovery would ensue. It was, however, noted that no urine had been voided for twelve hours and that the general condition of the patient was far from reassuring. The loins still presented the same elastic bulgings, which were perhaps even more distinct from the subsidence of the general bulk of the abdomen. The tongue was also dry and sordes were beginning to accumulate about the teeth.

On the thirteenth day the general symptoms were still more alarming. Great prostration existed, the patient was indifferent and scarcely answered questions; the pulse rose rapidly; the teeth were covered with sordes, and the urine was totally suppressed.

On the fourteenth day death took place, immediately preceded by a fit of convulsions. The patient had been in a state of typhoid coma for about twelve hours before, and no urine had been passed for sixty hours.

When the body was opened both kidneys were found hollowed out, and converted into two enormous lobulated sacs. The left was ten inches long by about seven broad, and almost filled the left half of the abdomen; the right kidney was about a quarter less. When they were cut open the pyramidal part of the kidneys were found totally absorbed, and only a thin layer of cortical substance, forming walls of the sacs, remained. The descending colon was adherent for the space of three inches to the left kidney. The bowel was contracted at this spot, and tightly stretched over the distended kidney in such a manner as to prevent the free passage of the fæces.

When the kidneys were examined the cause of the obstruction was found to be different on the two sides. On the left side there was a narrowing of the ureter at its commencement, which was so contracted that it would only admit a very small probe. Its origin

from the pelvis of the kidney, which was enormously dilated and globular, was also oblique, so that a valve-like obstruction was thereby constituted. The action of the latter impediment was clearly revealed, when the sacculated mass was held in the hands and subjected to various degrees of pressure. With moderate pressure no urine escaped by the ureter, but when the mass was strongly compressed the obliquity of the origin of the ureter was for the time effaced and the urine escaped freely. The same thing doubtless happened during life. When the distension was moderate the course of the urine was obstructed, but when the urine accumulated, until the distension became very great, the obstruction was at length overcome and the contents of the sac escaped. The narrowing at the orifice of the ureter was probably congenital, and constituted from birth a slight impediment to the free escape of urine. In the course of years it produced dilatation of the pelvis and subsequently hollowing of the kidney in consequence of the accumulation and stagnation of urine above the obstruction. As the pelvis enlarged and became distended with accumulated urine it acquired a more globular form, and the orifice of the ureter was, in consequence, carried upwards and assumed an oblique direction, so that an additional obstacle to the escape of urine was thereby created, and one which could only be overcome at intervals, when the pressure from behind became extreme.

The mechanical cause of the obstruction on the right side was due to an abnormal distribution of the renal artery. An irregular branch of the artery passed downwards toward the lower parts of the kidney, and in its course crossed in front of the ureter just below its origin. The slight but constant pressure of this branch evidently produced a certain degree of constriction of the ureter, and thus constituted a permanent hindrance to the escape of urine. In process of time this brought about, just as on the opposite side, stagnation of urine above the obstruction, dilatation of the pelvis and infundibula, and eventually sacculation of the kidney.

The cause of death in this patient was evidently not due solely to the urinary obstruction. The retention of fæces for a period of ten days must have contributed something to the blood poisoning which ultimately carried him off. The case illustrates in a most marked manner the rule which I have pointed out respecting urine secreted under pressure from below, namely, that it is conspicuously watery, and of low specific gravity.

CASE 8.—A man, æt 59, was visited by me with Dr. Herbert Renshaw, of Sale, on July 10, of this year. Six months ago he began to suffer pain in his back, loss of appetite, failure of strength, and constipated bowels. The pain in the back was of a constant and severe aching character, requiring endermic injection of morphia for its relief. The urine was pale and abundant, but discharged irregularly. It did not at any time up to my visit contain blood or albumen.

A month ago the patient had total suppression of urine for four days. This was overcome by compulsorily walking him about between two assistants. The urine returned and the pain subsided. After this, however, the discharge of urine was extremely irregular, and it was noticed that when the urine flowed freely the pain in the back was relieved, and that the pain became aggravated when the urine was for a time suppressed.

After the above-mentioned four days' suppression he recovered a good deal, and went to Southport. There he was attacked with diarrhœa, and had to return home in consequence.

At the date of my visit he was suffering severely from the pain in the back; he was very weak and the legs were slightly œdematous. He was then passing from one to two pints of a dilute urine daily; this contained a trace of albumen. I requested that all the urine which the patient voided should be collected and brought to me day by day, for the next three days. The first day he voided two pints, the second day one pint, and the third day eight ounces. For the next three days the urine was totally suppressed, and he died. The specimens of urine were all alike; they were pale and watery, the specific gravity ranged from 1009 to 1010, they were acid, and contained a trace of albumen.

The symptoms during the last three days of life were as follows, according to the statements of Dr. Renshaw and the patient's wife,

for I only saw him once myself:—Increased weakness, marked panting breathing; diarrhœa for the last two days; twitchings of the muscles; rambling delirium when left to himself, but perfect consciousness to the last when his attention was roused; no coma, no convulsions.

Autopsy.—Body quite free from urinous or ammoniacal odour. All the organs were healthy except the urinary apparatus. The source of mischief was found to be a hard scirrhous mass, as large as an orange, which half filled the pelvis. This growth involved the base of the bladder and the prostate gland. The rectum was adherent to it and constricted for the space of an inch; but I could get two fingers through the narrowest part. The seat of the scirrhous growth in the bladder was the submucous tissue. Neither the mucous nor peritoneal coats were implicated, though much puckered and folded, owing to the contraction of the thickened wall of the bladder. The whole trigone was involved, and the disease extended for a full inch above the trigone, terminating in a thick, abrupt rim or border. The walls of the bladder in the implicated region measured from half to three quarters of an inch in thickness. The fundus of the bladder was quite healthy, and the organ was capable of containing about half a pint of urine. The urethra for the length of an inch passed through the dense mass of the prostate, which was fully an inch and a half thick. The channel was quite free, a catheter had been repeatedly passed during life without any difficulty.

The terminal portions of both ureters passed for the length of an inch through the scirrhous mass; their course in this part was tortuous, and their channel compressed by the surrounding growth, but a probe could be passed through both of them, showing that neither was completely occluded. Above the bladder both ureters were dilated to the size of the little finger (the left more than the right), and distended with urine. The *left kidney* was greatly atrophied and weighed only $2\frac{1}{4}$ oz.; the interior was hollowed, without trace of pyramids, and the cortical substance was reduced to a fleshy rim of tissue of homogeneous appearance. The *right kidney* was enlarged, and weighed 7 oz.; it was hollowed, but not so completely as its fellow. The pyramids were gone, and the cortical substance was undergoing absorption. The pelvis was enlarged to the size of an egg, and distended with urine.

It was evident that the left kidney had not done any duty for some months, and that life had been sustained by the hypertrophied right kidney until this also was blocked up by the progress of the growth in the bladder.

The tumour had contracted adhesions to, and made extensions into, the adjacent parts in the pelvis. The iliac vessels passed through a dense scirrhous mass, whereby they must have been more or less compressed; this was probably the cause of the œdema of the legs.

My notes of the next two cases are exceedingly imperfect, but as each of them illustrates some point in the history of obstructive suppression, I will add them to the series.

Case 9.—This was an old lady of about sixty whom I saw with Dr. Gardiner of Ashton. She was afflicted with cancerous disease of the uterus and vagina, involving the base of the bladder and (presumably) implicating the terminal portions of the ureters. When I visited her no urine had been passed for four days, and the suppression continued without interruption for three days longer, altogether a total of seven days. After this the urine returned and flowed normally for the remaining four weeks during which she lived. During the time of suppression there was great restlessness and insomnia, with a flushed and anxious expression of countenance, but no twitchings of the muscles, and no convulsions nor coma. There was no autopsy. Seven days of suppression of urine, without the development of uræmic symptoms, and issuing in recovery so far as the suppression was concerned, is, as we have seen, not an unprecedented occurrence. It may be conjectured that in this case one ureter was permanently occluded by the morbid growth; and that during the epoch of suppression the opposite ureter had become blocked up, probably by a fungous excrescence projecting into its calibre, and that an ulcerative process at the end of seven days again cleared the passage. This is a process analogous to that which not unfrequently happens in

scirrhus of the pylorus, when the strictured state prevailing in the earlier periods is afterwards opened out by the softening and ulceration of a portion of the cancerous mass.

Case 10.—A man of about 35, greatly given to alcoholic excesses, was seen by me, with Mr. Hunstone of Strangeways, on January 15th, 1869. He had then passed no urine for four days. He was somewhat stout, and both loins were doubtfully thought to be the seat of bulging, of an elastic, quasi-fluctuating character. The previous history threw no light whatever on the nature of the case. There were no uræmic symptoms, but a great sense of tension of the abdomen. I saw this man on three successive days, and introduced a tubular needle to the depth of three inches into one of the lateral bulgings, but without reaching any collection of fluid. The notion I entertained was that a double hydronephrosis existed, and that the swellings in the loins were the sacculated kidneys distended with urine. He died two days after my last visit. No post-mortem examination was permitted. The suppression lasted nine days, and during that period only about an ounce of urine was voided. Mr. Hunstone states that this was pale. Up to the seventh day of suppression there were no twitchings of the muscles nor marked contraction of the pupils. The information respecting the final symptoms is defective. There was great restlessness and insomnia. Consciousness was maintained to the last, and the patient asked to be prayed with just before his death.

The *duration of life* in complete obstructive suppression appears to range, as a rule, from nine to eleven days, and the passage of a few ounces, or even two or three pints, of a dilute urine does not seem to extend the time of survivorship beyond a few hours. I have not discovered more than two well-authenticated cases in which the suppression was complete, or approached completeness, where the patient survived beyond the eleventh day. The first of these is recorded by Rayer ('Mal. des Reins,' t. iii, p. 490). He was a man of sixty-four years of age, who had hydronephrosis of the right kidney of many years' standing. The ureter of the left kidney was blocked up by a calculus, and suppression of urine ensued. This proved fatal in twenty-five days, and in that interval only two ounces of urine were voided. The second case is described by Mr. Paget in the second volume of the 'Transactions of the Clinical Society.' The patient was seventy-three years of age. The right kidney was atrophied and apparently incapable of secreting any normal urine. The left kidney was hypertrophied and the ureter blocked by a stone. Complete suppression ensued for thirteen days. No symptoms of uræmic poisoning appeared until the last of these thirteen days, when a slight attack of convulsions occurred. Then, on the fourteenth day, he passed an uncertain but "considerable" quantity of urine, and again six ounces on the same day; some slight convulsive movements which had been observed during the day then ceased. From this period until his death, seven days afterwards, the suppression was complete, and no urine was found in the bladder after death. So that there was total suppression for twenty-one days, only interrupted by one day's emission of urine. Muscular twitchings made their appearance on the sixteenth day. Mr. Paget attributed the extraordinary protraction of life in this case mainly to the patient's advanced age; but this view is scarcely borne out by other experience. My first patient was sixty-seven—only six years younger than Mr. Paget's case, yet he only survived nine and a half days, though he secreted sixty ounces of urine in that period.

There are, indeed, other cases on record, in the more ancient literature of medicine, in which patients are alleged to have survived many months of total suppression of urine; but it may be safely affirmed that imposition of some sort or other was practised in these cases.

Treatment.—Our notions of the treatment must vary according to the nature of the obstruction. Taking first those cases which are due to impaction of a stone in the ureter, it must appear that the use of ordinary diuretics cannot avail against a physical obstacle. There is something to be said in favour of means directed to excite the contractile power of the ureter. In my second case Dr. Garrod suggested, with this view, the use of

turpentine, but it provoked vomiting and could not be persevered with. Or remedies of an opposite class might be alternately tried with the purpose of relaxing the spasm of the ureter, such as opium, chloroform, belladonna, venesection, and warm baths. My own impression, however, is more in favour of mechanical means; and in reviewing the cases which have fallen under my notice, I cannot help thinking that something further might have been attempted in this way with a prospect of advantage. One such means, namely, kneading or shampooing the renal region and the course of the ureter, was in two of my cases followed by a so immediate, though only transient, flow of urine, that I could scarcely doubt that it was due to the means employed. But in a large number, if not the majority of cases the impaction takes place near the bladder where no direct force can be applied. Indirect means may, however, be tried. The physical condition is generally this:—Above the calculus the ureter is open and distended with stagnant urine; at the seat of the lodgment, and below it, the ureter is contracted. A displacement either upwards or downwards would be likely to be followed by relief. To provoke this succussion of the body and various changes of posture might be tried. The patient should be directed to support himself from time to time on his knees, with the upper half of the body depressed, and the sacrum might be repeatedly struck with the fist. The force of gravity would thus be brought in aid to coax the obstacle back toward the kidney. Or walking the patient between two assistants up and down stairs and about the room might be practised in the earlier periods of the case, with the object of facilitating the descent of the calculus into the bladder. Means of this class should be persevered in to the end, for experience is warrant that hopes may be entertained, even almost to the last, that the obstruction may be yet overcome.

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PLATE

Illustrating Dr. W. Roberts's paper on Disease of the Lymphatics of the Abdomen.

W. Searson, ad nat. lith. J. Clarke, imp. Manr

A CASE OF DISEASE

OF THE

LYMPHATICS OF THE ABDOMINAL INTEGUMENTS,

WITH

OCCASIONAL DISCHARGE OF LARGE QUANTITIES OF A CHYLOUS FLUID.

By WILLIAM ROBERTS, M.D., F.R.C.P.,
PHYSICIAN TO THE MANCHESTER ROYAL INFIRMARY.

The disease described in the following pages is so rare in these temperate climes that it must, for the present, be regarded more as a pathological curiosity than a matter of practical interest. But it not unfrequently comes to pass that exceptional freaks of nature throw great light on some of its more ordinary processes, supplying an important link in some well-known chain of sequences. So it happens with regard to some rare and obscure forms of animal and vegetable life; although insignificant in themselves they come to assume great scientific importance when they are found to fill what would otherwise be an inexplicable gap in a series, or serve to connect, in a rational manner, two approximating groups of animals or plants. It is, therefore, the duty of those who chance to encounter one of these *lusus naturæ* to observe it and place it on record, in the expectation that some future generaliser may find in it something which may assist him in framing useful practical deductions.

The case here recorded is probably closely related to another rare—but not nearly so rare—a disease, namely, chylous urine. And it is something to get any light on the pathology of that disorder. The opportunity furnished by a *post-mortem* examination in the following case has enabled me to propose a new and, I hope, more rational theory of that strange disease than any hitherto offered.

W. Robinson, aged 45, was admitted into the Manchester Royal Infirmary, September 21st, 1868. He was born in Accrington, and never resided out of Lancashire. A clogger by trade, he worked occasionally in the mill, and enjoyed good health, with the exception of occasional indigestion, until two and a half years ago. His family history is good, and neither in that nor in the antecedents of his own life, could any circumstance be found bearing directly on the genesis of his present complaint. He is a widower and never had children.

Two and a half years ago he began to suffer from a succession of large abscesses; one appeared on the buttock, another on the right breast, a third in the left groin, and a fourth in the right iliac region, two inches from the middle line, and about midway between the horizontal level of the umbilicus and the pubes. The two last formed, opened, and refilled several times before they finally closed. These abscesses continued to trouble him for a period of six months, and brought him to a low state of health. He suffered from severe shooting pains which seemed to extend from one abscess to another; he lost his appetite and grew weak and thin, and had to go to Southport to recruit his health.

After all the abscesses had healed up he noticed a scab to remain over the site of the one situated in the right iliac region, and one night, some twenty months before admission into hospital, he picked this off, and immediately a pale watery fluid, exactly like gum-water began to exude. This escaped so quickly that in a quarter of an hour his clothes were soaked; the discharge continued during the night and the succeeding day, in gradually diminishing quantity, and then ceased. He calculated that from two to three pints of fluid escaped on this occasion. And now he noticed a number of pale transparent vesicles, no larger than pins' heads scattered in the right iliac region over and around the site of the old abscess. When he first observed them they were ten or a dozen in number, but in a few weeks they began to spread and multiply until, in a few months, they dotted the surface of the

lower part of the abdomen on both sides of the middle line, almost as low as the pubes on the one hand, and as high as the umbilicus on the other. Some of them, also, began to discharge a pale watery fluid. By and bye the vesicles and the discharge from them began to assume a thick, milky appearance, and gradually they assumed the condition in which they were found on admission.

State on admission.—The patient has dark hair and blue eyes; he is somewhat under medium stature, and rather spare. He is not confined to bed, and usually goes about with a thick towel round the abdomen to keep himself dry. P. 108, R. 24, Temp. 97·4° in axilla. Tongue nearly clean; appetite variable, bowels habitually costive.

When the abdomen is uncovered the lower part is seen to be studded with numerous vesicles filled with a milk-white fluid. These are arranged partly in irregular groups and in part singly. Some of the groups contain three or four, others eight or twelve, vesicles, closely aggregated together. Some of the vesicles are so small that they are only just visible to the naked eye, others are as large as peas, and between these extremes are others of every intermediate size. Most of them are hemispherical, but some are oblong or irregular, as if two or more had coalesced. In the smaller ones the vesicular membrane appears quite transparent, without a trace of organization, their opaque white contents shining through them like drops of rich milk; but a few of the largest ones are distinctly marked by meandering lines of delicate blood-vessels giving them a faint rose colour.

The seat of this eruption is the hypogastric region from the umbilicus to the pubes (see Plate). It extends considerably more to the right than to the left of the middle line. The eruption is thickest near the centre of the hypogastrium, and the vesicles are generally smaller and more sparse towards the confines, but this is not uniformly so; a cluster of large vesicles exists close to the umbilicus, and another still more considerable cluster is placed near the upper and rightward limit. The skin over the affected area is thick and soft and of a dull red colour. When pressed with the fingers it yields an almost spongy impression, but it does not pit. The integument is manifestly hypertrophied, and this gives to the lower part of the belly a protuberant appearance. This dull-red tumid area is somewhat more extensive than the limits of the vesicles, and fades at the circumference into the healthy skin about an inch beyond the furthest vesicle in all directions.

The skin around the larger vesicles, and groups of vesicles, is raised into soft nipple-like elevations, and has a more decidedly spongy feel than elsewhere. Slight pressure causes no pain, but the whole arc is more or less tender on deep pressure.

The total number of vesicles is about two hundred and fifty. It may be said that in their normal state they do not discharge externally. But very frequently one, two, or three vesicles are ruptured and discharge freely a fluid resembling milk.

The patient remained in the Infirmary from September, 1868, until his death in May, 1869. He continued in a stationary condition of health until about the end of March, when symptoms of pulmonary consumption set in, and increased rapidly, finally carrying him off on May 22nd. During this period he was carefully watched, not only by myself but also by Mr. Cullingworth, our able physician's assistant, and by my clinical clerk, Mr. W. A. Patchett, from whose careful diary this history has been chiefly drawn up.

In their normal state the vesicles were, as I have said, closed, and the immense majority remained throughout in this condition; but some dozen or so of the largest vesicles were, at one time or other, in a ruptured state, and discharged immense quantities of a chylous or lymphous fluid. The cause of rupture appeared to be some slight movement or violence. Sometimes the act of turning in bed sufficed to set the discharge in motion. It rarely happened that more than two or three vesicles were discharging at the same time. The quantity of this discharge, and its occurrence and arrest, were most irregular. Sometimes several pints would be discharged in a day and night, and sometimes only sufficient to moisten the cloths with which the patient girded himself. The patient was sometimes continuously wet for three and four weeks; at other times the flow would continue only a few hours or a few days. The

intervals of complete dryness were similarly uncertain, and varied from a day or a few days to two or three weeks.

The character of the discharge also varied: sometimes it was like thick milk, sometimes like skimmed milk, and sometimes perfectly pale like gum-water. Whether white or pale it was always spontaneously coagulable, and white or yellowish clots collected about the seat of discharge. The colour of the unruptured vesicles varied in correspondence with that of the discharge, from milk-white, or opalescence, to pale straw. The degree of milkiness at any particular moment was always the same in all the separate vesicles, showing that the cause of variation was not a local one particular to any vesicle, but something affecting the eruption generally, and depending presumably on the state of the blood.

The vesicles varied not only in colour but also in fulness and turgidity; and it was noticed that the whiter they were the more distended they appeared, and that when they were pale they were also more flaccid.

Two circumstances affected, though somewhat irregularly, the whiteness and fulness of the vesicles, namely, the general state of the patient's health, and the digestion and assimilation of food. On the days when the patient was out of sorts or feverish, the vesicles were paler and more flaccid, but when the appetite and sleep returned the vesicles became milky and turgid. As his health finally declined, the milky condition became less marked, and in the last week of life the vesicles became permanently pale and flaccid.

The effect of food was found to be tolerably constant in kind though not uniform in degree. The vesicles were paler in the morning before breakfast, after the prolonged fast of the night. At this period they were often quite lymphous. Soon after breakfast they began to grow fuller and whiter, and, as a rule, the milkiness increased through the day, attaining its maximum some seven or eight hours after dinner. Of course the appearance of the discharge, if there were any, followed the same rule.

The vesicles seemed to be situated in the substance of the cutis, and their surface-wall was evidently composed of something besides epithelium. In the larger vesicles their base was raised, and consisted of soft cutaneous tissue; and capillary vessels could be seen travelling over their transparent summits. When a vesicle was gently pressed with the tip of the finger it was immediately emptied, its fluid contents escaping into the deeper parts. After the pressure was withdrawn the vesicle slowly filled again. There was no direct communication between neighbouring vesicles, and when one was ruptured and discharging the vesicles around it still appeared full and turgid. It was noticed, however, that when the discharge had been very free for some hours, all the vesicles appearea flaccid. Even when a whole cluster was compressed the neighbouring vesicles did not appear more distended. The idea conveyed by the study of the effects of pressure on different vesicles and groups of vesicles was, that each vesicle communicated with a more deeply situated reservoir of anastomosing channels. When a vesicle was pricked the flow from it immediately began, and it continued at a steady rate for hours together. On one occasion the rate of flow from a punctured vesicle was tested, and found to be equivalent to eight ounces per hour!

The characters of the discharge, whether it was milky or opalescent, were always essentially the same. After standing a few minutes it set into a tremulous jelly. In a few hours there was a separation into clot and serum. It coagulated with heat and with nitric acid, but not with acetic acid. When shaken with an equal bulk of ether the white appearance was removed and the fluip became transparent and yellowish like blood-serum. These reactions prove that it contained fibrin, albumen, and fat, and that it differed essentially from true milk in not containing casein. The reaction was always alkaline. The varying degree of milkiness was, of course, due to the varying quantity of fatty matter. Under the microscope myriads of fine fat molecules were seen sometimes mixed with larger oil globules; in addition to these pale corpuscles, identical in structure with the white corpuscles of the blood or chyle, were always present, but not in large numbers. No other organic forms were ever seen except the transparent fibrillæ of coagulated fibrin.

The fluid is thus seen to be similar in character to chyle when milky, and to lymph or liquor sanguinis when pale. It is also identical with the admixture which takes place in cases of chylous urine. A case of chylous urine happened to be in the hospital at the very time the present case was under observation, and neither chemically (excepting proper urinary ingredients) nor microscopically could any distinction be made between them. Still more significant of this alliance was the fact to be noted presently, that on two separate days this man did actually pass chylous urine.

The condition of the *urine* was carefully noted during the progress of the case. It was generally found to be remarkably scanty in quantity and of high specific gravity. When the discharge was abundant the quantity of urine ranged from 13 to 18 oz. in the twenty-four hours. When the eruption was dry the urine was somewhat more abundant and varied from 18 to 25 ozs.—on one occasion it reached 34 ozs. and on another 40 ozs., which was the largest flow chronicled during his long sojourn in the Infirmary. The sp. gr. varied from 1025 to 1032; it frequently deposited lithates, but did not contain either albumen or sugar. The scantiness of the urine was partly due to the voluntary abstention of the patient from drink. He believed that drinking always increased the flow of the discharge: and he endured constant and severe thirst in order to check this loss.

On December 2nd the urine was voided milky on two occasions. It presented all the ordinary characters of chylous urine. Again, on January 15th, the patient passed three ounces of chylous urine, and on the following day fat was found in the urine with the microscope, though not in sufficient quantity to produce a milkiness of the secretion. During these two days the eruption was dry. With these exceptions the urine continued of normal composition throughout, and free from albumen.

A large number of observations on the *temperature* of the body were made. Until the end of March the temperature never transcended the normal limits. When the discharge was running freely it fell to 97·6° and 97°. When the eruption was dry it ranged from 98° to 98·6°; but when, towards the end of March, tuberculous symptoms began to show themselves, the temperature rose to 99, 100, and even 102 degrees.

The patient's weight was 120 lbs. on admission in October; he steadily grew heavier until the end of December, when he weighed 128 lbs. From this date until the middle of March, his weight remained stationary at about 126 lbs.; then it began to decline. On April 27th he weighed only 112 lbs., after which he was not weighed.

The only general symptoms referrible to the disease on the abdomen, and the discharge, were attacks of chilliness and shivering, with a sense of great weakness. These occurred repeatedly when the discharge was copious and long continued. He also complained occasionally of aching pains in the abdomen and of indifferent sleep; but as a rule, he was in a state of fairly comfortable health until the tuberculous symptoms broke out.

The eruption withered slowly as the pulmonary disease advanced, the vesicles became persistently pale and flaccid; the discharge became watery and scanty, and finally ceased some five days before death. The state of the eruption the day before death is thus described by Mr. Patchett. "The vesicles have lost their character of vesicles altogether, they seem converted into small furfuraceous scales of different colours, some being of a reddish-yellow, others of a raspberry colour; the small vesicles scattered over each flank look exactly like flea-bites."

The urine was reduced to six and eight ounces per day in the last week, and the symptoms assumed the so-called typhoid character—low muttering delirium, indifference, picking at the bed-clothes, and finally coma. Death occurred on May 22nd.

Autopsy twenty-one hours after death.—Both lungs were studded with grey granulations intermixed with larger masses of grey and yellow tubercle, some of which were softened. Two small vomicæ were found in the left apex, and one in the right. Tuberculous ulcers were also found in the small and large intestines. The bronchial and mesenteric glands were enlarged. The liver weighed sixty-four ounces, and the spleen nine ounces; both organs were healthy. The kidneys and bladder were healthy. The integument of the hypogastrium was much thickened and spongy, contrasting strongly with the emaciated integument over

the thorax. The lining membrane of the bladder was minutely examined, and appeared smooth, glistening, and healthy throughout. No enlargement or unnatural condition of the thoracic duct or of the lymphatic vessels or glands could be detected. A considerable piece of the abdominal wall, embracing a portion involved by the disease and a portion extending beyond into the healthy skin, was cut out for further examination.

Examination of the skin in the diseased area.—On making a vertical section through the skin and subjacent parts it was at once perceived that the disease involved essentially the cutis vera and the subcutaneous tissue. The tendinous, muscular, and peritoneal strata were in every respect perfectly normal. The skin was immensely thickened and formed, with the subcutaneous tissue, to which it was structurally united, a thick pad or layer of tissue varying from half an inch to an inch thick. When fresh, the cut surface had a pale rose and somewhat fleshy or glandular appearance. This tissue was traversed by short channels or lacunæ, varying from the width of a crow-quill to that of a hair. By making numerous thin sections vertically and horizontally and examining them with a lens and the microscope, these lacunæ could be seen to communicate freely with each other by small smooth orifices. The vesicles evidently constituted the surface boundaries of the more superficial lacunæ. The lining membrane of the lacunæ and of the vesicles was smooth and glistening; and, when gently scraped with a knife it yielded a small quantity of a whitish debris, which, under the microscope, resolved itself into spheroidal and nucleated cells resembling those which were found in the discharge during life.

Here are evidently the elements of a glandular structure—a membrane lined with spherical nucleated cells. But the analogy is rather with the ductless follicles of Peyer's patches and still more with the ganglia of the lymphatic chains than with glands engaged in the regular work of secretion and possessing excretory ducts. The new structure had no connection with the normal glands of the skin. The funnel-shaped orifices of the sweat-glands could be seen opening independently on the surface in the hollows between the vesicles, and the hair follicles presented their normal appearance.

The chief interest of the case lies in the light which it throws on the pathology of chylous urine. It can scarcely be doubted that the case was generically identical with that curious disorder. The absolute similarity of the discharge with the fibro-albuminous and fatty elements added to the urine in chylous urine, the sudden appearance and cessation of the discharge, the capricious terms of the duration of the discharge in the two disorders, and the actual occurrence of chylous urine on two occasions in the case of Robinson, scarcely leaves any room for doubt on this point. Had the disease in this case, instead of occupying the subcutaneous tissue of the abdomen, been developed in the submucous tissue of any part of the urinary passages, it is evident that the conditions for the production of an ordinary case of chylous urine would have existed. It is even almost certain that some small part of the urinary membrane—probably that of the front of the bladder—was actually invaded by the disease which affected the abdomen, but no anatomical traces of such extension could be detected at the autopsy, owing probably to the fact that in the last few weeks of life the morbid process had retrograded, and had consequently left no appreciable marks on the surface of the bladder.

In the way of *treatment*, the means tried were the internal administration of styptics of various sorts, especially of tannic acid, which was pushed to large doses. Locally, compression was attempted by means of long wide strips of adhesive plaster, but without any good effect. The discharge moistened and loosened the plaster, and the soft yielding nature of the abdominal wall rendered impossible any effective compression by bandages or belts. Attempts were also made to varnish the surface with a solution of india-rubber in benzole, and with collodion, but every device proved unavailing.

The rarity of this disorder is so great, that only one other exactly similar case, so far as I know, has been recorded as having occurred in Great Britain. This is the case published by Dr. A. B. Buchanan, of Glasgow, in the 46th vol. of the 'Medico-Chirurgical Transactions.' In this case, a woman, 46 years of age, had an excoriated patch of skin, about the size of the palm of the hand, on the inner and posterior aspect of the left thigh, which discharged at

times a large quantity of milky fluid, possessing exactly the same chemical characters as that in the case of Robinson—it coagulated spontaneously, it contained fibrin, fat, and albumen—but no casein. The discharge flowed partly from the excoriated surface and partly from broken vesicles in the neighbourhood of this patch. The central patch was congested of a deep red colour when the patient was standing; while when she was lying prone the colour was paler, though still indicating a great amount of local hyperæmia. The patch, moreover, stood out irregularly in relief, exactly like the raised map of a mountainous district. This area was thickly covered with vesicles, from the size of a pin's head upwards, some of them being even as large as those met with in herpes zoster. The same were also visible in some numbers on the surrounding skin, even where it was no longer infiltrated or congested: they were, however, both smaller and more sparsely disseminated the further they lay from the centre. The white contents shone clearly through the thin epidermic pellicle that confined them, imparting to the vesicles the appearance of pearly drops of fluid. Some vesicles were entire and perfectly dry on the surface, but from the excoriations resulting from their rupture, even when they were simply opened with the point of a lancet, a thin constant stream of milky fluid oozed forth and ran down the leg. This discharge was considerably affected by the position of the patient. It continued to flow for a long time from an excoriated point, even when she was lying on her face; but it was both more copious and more persistent when she was erect and moving about. About an hour after she retired to rest it commonly ceased to run, so that the leg in the morning was quite dry. The flux recommenced about an hour after the patient rose in the morning, and increased in profusion as the day wore on. In the after part of the day her garments were usually drenched, and on placing a basin underneath the thigh to receive the fluid that ran from it, somewhat over five ounces were collected in an hour. The discharge sometimes ceased for days and even two or three weeks in dry weather, but in moist weather the intervals of dryness were always very brief. The affected thigh was considerably swollen, and the superficial veins of the thigh and leg were varicose. Neither the inguinal nor any other lymphatic glands within reach were enlarged. The nature of the food of the patient was not found to affect the quantity nor the quality of the discharge. The urine was throughout normal in composition, but it was less in quantity when the discharge was profuse. The patient, who was a married woman with six healthy children, stated that twenty-one years ago, two or three months after her second confinement, she had a shivering fit, and shortly afterwards she noticed "a lump" on the back of her left thigh. The swelling was attended with no uneasiness, but it did not subside until the period of the third pregnancy, when it went away or at least diminished for several months: again reappearing, however, after another shivering fit about a fortnight after delivery. Every year, for the last twenty years, she has also suffered from at least one attack of inflammation (phlebitis?) in the affected limb. About fifteen years ago, between her third and fourth pregnancies, a few vesicles made their appearance after an inflammation of this kind somewhere near the centre of the "lump." The surface at this point was itchy, and a brownish fluid exuded from the vesicles on scratching, which continued to be secreted at remote intervals, and in small quantities, for about a year. Another inflammation having supervened, the vesicular area extended, the discharge became more frequent and profuse, and began to assume a whitish appearance. From this period the affected area gradually increased, and the discharge more abundant and milky, until for the last six years it assumed almost the appearance presented when the case came under notice.

The catamenia were regular, and the discharge was in no wise affected by the menstrual periods. The general health continued fair, but considerable exhaustion followed when the discharge was abundant and long continued. The case was still in progress when reported. The only treatment that seemed to restrain the discharge was the pressure of an elastic stocking reaching to the top of the thigh.

Dr. Buchanan, in his paper, cites at length three other cases occurring long ago in France and Germany, of evidently identical nature. In two of them, one a man and the other a woman, the seat of the disease was in the thigh; in the third, a woman, the dis-

charge exuded from the skin of the right side of the abdomen beneath the ribs.

A still more similar case to that described in this paper is recorded by Dr. Fetzer,* of Stuttgart. A girl of sixteen, who had not menstruated, but otherwise healthy, noticed a few warty projections on the abdomen, to which, however, she paid no attention. Six months after, she showed these to Dr. Fetzer, who then found a band of brownish streaks, three finger-breadths wide, extending across the left half of the abdomen. This band commenced an inch below the navel, and ran upwards and outwards between the false ribs and the crest of the ilium, fading gradually towards the spine. On this band, in the front of the abdomen, there were about eighteen warty elevations of the skin, varying from the size of the male to that of the female nipple. These were soft and painless to the touch, and on pressure could be made to sink into the skin, rising again as soon as the pressure was removed. About a year after they were first observed two of the elevations gave way after a walk, and discharged about a quarter of a pound of a milky fluid, which coagulated spontaneously and had all the character of chyle. Three days later the discharge returned and continued for three days without interruption. On the next day Dr. Fetzer cut off one of the elevations with a pair of scissors. It was found to consist of thin but perfect cutaneous tissue. Into the opening thus made a probe could be introduced for the space of an inch upwards and to the left. The flow of discharge through this opening could only be stopped by the application of nitrate of silver, but not by compression. The patient felt faint after the discharge. In this case the discharge was milky during fasting as well as after eating. It was also noticed that the clot reddened perceptibly on exposure to the air, a circumstance that was also noticed in one of Dr. Carter's cases, to be presently described. Fetzer regarded the elevations as varicose bulgings from a bundle of subjacent lymphatics. Both the microscopic and chemical examination of the fluid confirmed the opinion that it was identical with chyle.

* 'Archiv für Physiol. Heilk.,' vol. viii, p. 128.

Dr. Carter, of Bombay,* has recorded two cases, of which the following short abstracts may be read with interest:

1. This was a Parsee youth in whom the inguinal glands were greatly enlarged, soft and doughy to the touch, but not painful. On the cutaneous surface of the thigh, a few inches below Poupart's ligament, was a small, hardly perceptible pimple, from which there occasionally issued a milky fluid, and sometimes so copiously, that in the course of the day a pint has been collected. Pressure just above the spot caused the flow to cease. The discharge commenced six months before; it lasted two or three days and then ceased, pressure having been applied; it reappeared after an interval of a month, and again stopped after a few days. The discharge reappeared a third time, when the patient came under the observation of Dr. Carter. Before the discharge came on the glands in the groin became tumid and rather painful. The fluid collected from this man's thigh presented in perfection the characters of chyle. It coagulated in about five minutes, it separated a few hours afterwards into clot and serum, the latter being milky in appearance. At a later period the whole became again fluid. Under the microscope blood-corpuscles and chyle-corpuscles were found. In this man the urine never became chylous.

2. This was an adult Hindoo who became a hospital out-patient under Dr. Carter's care on August 23rd, 1859, on account of an affection of the scrotum. The skin of this part was corrugated in a peculiar way, thickened, and studded with numerous small tubercles, which were soft to the touch, and when punctured freely discharged a chylous fluid. The inguinal glands on both sides were much enlarged, soft and doughy to the touch, and they diminished in size under pressure.

The scrotum began to enlarge four months before, and after a time the peculiar corrugation of the skin appeared. The milky discharge occurred occasionally, and spontaneously, and intermitted. It did not issue from one spot, but from several. *When it ceased, and also sometimes during its continuance the urine became chylous.* The tumefaction of the inguinal glands seemed to alternate with the appearance of chyle in the urine. The parts

* 'Med.-Chir. Trans.,' vol. xlv, p. 189.

also became tumefied two or three hours after a full meal, and then subsided.

The urine, when chylous, coagulated spontaneously, and the coagulum reddened sensibly when exposed to the air. This reddening was, however, more decided in the coagulum from the scrotal discharge, which changed in a few minutes to blood-red. Under the microscope the urine was found to contain, besides blood-corpuscles, nucleated granular cells, exactly resembling chyle-corpuscles.*

Pathology.—It may be confidently assumed that the disease on the abdomen in my case, on the thigh in Dr. Buchanan's case, and that of Fetzer, and on the scrotum in Dr. Carter's case, was pathologically identical with that which causes chylous urine. The reasons for this conclusion have already been enumerated. It may also be regarded as proved that chylous urine is neither a disease of the blood nor of the kidneys. The blood has been repeatedly examined in these cases, and no peculiarity has been found in it; and it would be quite impossible that the albumen and fibrine found in chylous urine could come from the kidneys without causing casts of the uriniferous tubes to appear in the urine, and no such casts have ever been seen in chylous urine.†

Looking to the absolute identity of the discharge in these cases with chyle and lymph, it is very difficult to avoid the impression that the structures which produce this discharge are anatomically related to the lacteal and lymphatic tissues; and among the theories framed to explain chylous urine, the most reasonable hitherto put forward is that which supposes the existence of a leak or fault in

* Two other apparently similar cases, affecting the scrotum, are reported from Canton in the 'Edin. Med. Journ.' for July, 1860, but the notes are so defective that the nature of the disease cannot be identified with any certainty.

Demarquay ('Mém. de la Soc. de Chirurgie,' t. iii, p. 139) describes a case in a youth of seventeen, born in Brazil, which much resembled that of Dr. Buchanan. There was a patch of elevated skin on the inner and lower part of the thigh, covered with little elevations, one of which occasionally discharged chyle. Another vesicular patch was situated on the front of the thigh. The whole limb was thicker than its fellow. The disease was supposed to be due to varicose lymphatics.

† See the author's article on "Chylous Urine" in his work on 'Urinary and Renal Disorders,' p. 268.

some part of the lymphatic or lacteal system, which permits the regurgitation of chyle and lymph on the surface of the urinary passages. Dr. Carter—basing his views mainly on the study of his own cases—applies this theory in the following manner:—"We may suppose that distension of the delicate lymphatics and lacteals in the lumbar region is at length followed by exudation of their contents at one or more points; or rupture taking place, a fistulous orifice remains, which gives free exit to the chyle or lymph at times of recurring distension; or an abnormal reservoir (receptaculum) may be formed, which periodically discharges its contents into the pelvis of the kidney, ureter, or bladder. The cases before related evince that such a condition of the lymphatic vessels, accompanied with enlargement and increased functions of the corresponding glands, does occur, that the flow of chyle may be reversed or regurgitation may occur, and with this state the urine may be chylous."

A somewhat different view was suggested to me by the examination of the skin of the abdomen in the case of Robinson. When the preparation was fresh the thick, soft layer, into which the skin and subcutaneous tissue were converted, had very much the pale flesh-colour and general appearance of lymphatic gland tissue; and the short communicating lacunæ traversing it in all directions suggested a structure not dissimilar to an immense exaggeration of the lymphatic plexus. I found it impossible to resist the idea that this was really the true pathological solution of the case, and that a similar solution applied to cases of chylous urine.

It is well known that the skin with the subcutaneous tissue, and the mucous membranes with the submucous tissue, are exceedingly rich in lymphatics, which form a close network of communicating channels in these situations. It is further known that the cells lining the lymphatic channels, especially those of the lymphatic glands, perform a glandular function, and impress important changes on the lymph passing through those channels.

Now let it be supposed that at some spot the lymphatic network becomes immensely hypertrophied; that its channels become varicose (as it were); that the contained cells assume by degrees the property and function of the cells lining the lacteal ducts and

lacteal glands; that the more superficial of these varicose enlargements project above the surface of the skin or of the urinary mucous membrane, as the case may be; and, lastly, let some of these superficial enlargements become ruptured and discharge their contents externally or into the urinary passages, and the conditions are presented for the production of chylous urine or of such a case as that of Robinson or of those related by Dr. Buchanan and Dr. Carter.

It is always satisfactory in studying any rare disease to be able to refer it by analogy to some pre-existing well-known category, and the view just presented of the pathology of chylous urine and the allied disorder on the skin finds its exact analogy in those hypertrophies of the blood-vessels which constitute venous nævi, erectile tumours, and aneurisms by anastomosis,—all of which are exaggerations or hypertrophies of the normal arterial or venous plexuses.

It rarely or never happens that any tissue suffers morbid hypertrophy without some degree of modification of its normal structure and the hypertrophied lymphatic tissue which I have suggested as the true cause of chylous urine and the allied condition in the skin is undoubtedly modified by the morbid impulses which generate it. Not only is the anatomical structure considerably altered from the normal type of lymphatic tissue, but the function of the cells also suffers a modification. The cells which, in the normal state, elaborate lymph, in the morbid state come to produce chyle, or a fluid intermediate between lymph and chyle. These modifications are, however, strictly within the limits which we generally find in other morbid hypertrophies.

It may be remarked that, both in my case and in that of Dr. Buchanan, as well as in one of Dr. Carter's, the discharge and the contents of the vesicles were *pale* in the earlier periods of the disease and that they only gradually, and at a later stage, assumed a *milky* character. This is significant. It appears to indicate that the deviation from the normal type commenced in the parietal structures, and that afterwards the functions of the cells became modified.

It is no objection to the theory here proposed that the contents of the vesicles and the discharge from them varied greatly in its degree of milkiness. Every secretion varies in character more or less at different parts of the day, as exercise or rest, food or fasting, &c., vary the condition of its elaboration. I will only instance the urine, with which I may claim a more intimate knowledge. The urine is immensely influenced both in its composition and quantity by the various events of the day, and the influence of food is particularly great on it. The composition of the discharge in Robinson's case was always essentially the same, though it varied in appearance greatly according to the smaller or larger proportion of fat in it, but it always contained the same characteristic elements, namely, albumen, fibrine, and fat.

Closely allied to the above cases are certain forms of varicose disease of the lymphatics of the penis and elsewhere, and also certain forms of elephantiasis of the leg and scrotum. Carswell* figures a case of a young man with enormous varicose dilatation of the thoracic duct and the lumbar and pelvic lymphatics, together with those of the groins, where they formed two tumours, one on each side, which had been mistaken for hernial tumours. These latter had existed from boyhood.

In the 2nd vol. of the 'Transactions of the Clinical Society' three cases of what may be called lymphatic elephantiasis of the leg are described, in each of which a chylous fluid was at times discharged from vesicles or excoriated surfaces on some part of the enlarged and indurated extremity.

* 'Pathol. Anat.,' art. "Hypertrophy," pl. iv, fig. 4.

EFFECTS OF THE

DROUGHT OF 1870

ON SOME OF

THE EXPERIMENTAL CROPS AT ROTHAMSTED.

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FROM THE
JOURNAL OF THE ROYAL AGRICULTURAL SOCIETY OF ENGLAND.
VOL. VII.—S. S. PART I.

THE

DROUGHT OF 1870,

AND THE

EXPERIMENTAL CROPS AT ROTHAMSTED.

The rainfall of Great Britain is usually sufficient for the growth of a considerable variety of crops, in fairly abundant quantity. Indeed, so far at least as the growth of corn is concerned, our fears are of injury from an excess rather than from a deficiency of rain. It is only occasionally, and generally at long intervals, that a season of great drought occurs; and then it is that we forcibly realise how essential for luxuriant vegetation is an abundant supply of water.

Throughout the Midland, Southern, and Eastern portions of England, the year 1870, just past, has been characterised by a season of drought, commencing with the period when vegetation usually becomes active, and extending, with little intermission, to the time when its activity has upon the whole greatly diminished, and in the case of some crops entirely ceased. To find a parallel we must go back to 1844, or more than a quarter of a century. The summer of 1868 was, it is true, one of great drought; and, being hotter than that of 1870, it is not improbable that there was at some periods of it a greater deficiency of moisture in the soil than in the latter year. In fact, those who travelled through the Southern and Midland counties of England in July, 1868, will not soon forget the almost entire absence of green in the meadows, and the intense heat of the atmosphere, resembling more what we read of in tropical countries than the usual experience of our own summers. Although both the drought and heat were more extreme during the months of May, June, and July in 1868 than in 1870, the deficiency of rain commenced a month earlier and extended later last year; and hence, not only the first crops of grass and hay, but also the second growth, suffered much more in the season just past than in 1868.

It is only when crops are grown under precisely similar circumstances, as to manure and other conditions, for many years in succession, that we can obtain satisfactory data for studying the influence of variation of season on the amount and character of the produce. At Rothamsted, as is known to most of the readers of this Journal, numerous experiments on the growth of various crops, each grown year after year on the same land, with different descriptions of manure, the same description being applied year after year to the same plot, have been carried on without change for many years; in some cases reaching back as far as the drought of 1844, above referred to. Taking advantage of the results so obtained, it is proposed, in the present paper, to consider briefly :—

1. The probable amount of water exhaled during growth by some of our most important crops.

2. The source whence the required supply of water is obtained.

3. The difference of the effects of the drought of 1870 on the different experimental crops.

Amount of Water given off by Plants during Growth.

A series of experiments was commenced in 1849, and was continued for ten years, to determine the amount of water given off by plants during their growth, in relation to the amount of the various constituents they assimilated. Of agricultural plants, wheat, barley, and mixed grasses, as representatives of the Graminaceous family; beans, peas, and clover, of the Leguminous family; and swedes, white turnips, mangolds, potatoes, and artichokes, as root-crops, were thus experimented upon. Similar experiments were also made on the exhalation by evergreen and deciduous trees, six of each being selected.

The plan of experimenting was as follows:—Cylindrical vessels, first of glass and afterwards of zinc, 14 inches in depth, 9 inches in diameter, and holding about 40 lbs. of soil, were employed. Soil from the plot in the experimental wheat-field which had grown 10 successive crops without manure was selected. The general rule was to make three experiments with each description of plant; one with the above soil without further addition; one with the same soil with purely mineral manure added; and the third with the same soil and both mineral manure and ammonia-salts in addition. In the cases of wheat and barley, plants from three seeds, and of beans, peas, and clover, one plant only, were planted in each vessel. A glass plate, having a hole in the centre about three-quarters of an inch in diameter for the plants to grow through, and another smaller one, closed at pleasure by a cork, for the supply of water, were then firmly cemented upon the top of each vessel. One vessel, supplied with soil and fitted with a glass cover like the rest, was, however, always left without a plant, in order to ascertain the probable amount of evaporation from the surface of the soil itself, through the centre orifice, independently of growth; though, in the experiments with plants, the hole was always partially closed, by laying small pieces of glass over it as far as the stems would allow. Of course in experimenting with root-crops the holes in the glass covers were larger, but they were kept closed around the plants as far as possible, in the manner just described.

The vessel with its contents, weighing more than 40 lbs., was weighed from time to time, generally every ten days during active growth, by means of a delicate balance made for the purpose; which, though carrying so heavy a weight, was capable of indicating a change of a few grains. The plants were of course supplied with water as it was needed. The earlier results, both with agricultural plants and trees, are published in the 'Journal of the Horticultural Society of London,' and to the reports there given we must refer the reader for the details of the inquiry as far as they are yet recorded.*

Referring here only to the results obtained with some of the agricultural plants, it will be sufficient for our present purpose to summarise them as follows:—

1. The amount of water given off by the plants during growth was found to bear relation to the quantity of the total dry matter, or the total non-nitrogenous substance, fixed or assimilated; and within somewhat narrow limits the same relation was observed in the case of both graminaceous and leguminous corn-crops.

2. In relation to a given quantity of water exhaled, twice or three times as much nitrogenous substance is fixed by a leguminous, as by a graminaceous corn-crop.

3. In the growth and ripening of either graminaceous or leguminous corn-crops, probably on the average from 250 to 300 parts of water are given off for 1 part of total dry substance fixed or assimilated.

Before considering the application of this estimate to any special cases, it may be well to give an illustration of its bearing in general terms. Several plots in the experimental wheat-field give an average of about 3 tons of total produce (corn and straw) per acre per annum; and if we assume one-sixth of this to be water, we have remaining $2\frac{1}{2}$ tons of dry substance ripened by the end of July, or the middle of August, each year; and if we further assume that 300 parts of water may be exhaled for 1 part of dry substance fixed, we have $300 \times 2{\cdot}5 = 750$ tons of water evaporated per acre by the growth of such a crop.

Owing to the difficulty of eliminating surface evaporation other than through the growing herbage, in experiments on the exhalation from a sod of mixed grasses, we cannot so safely adopt a figure to represent the probable average amount of water given off for 1 part of dry substance fixed in their case

* 'Experimental investigation into the amount of water given off by plants during their growth, especially in relation to the fixation and source of their various constituents.'—('Jour. Hort. Soc. Lond.,' vol. v. part i. 1850.)

'Report upon some experiments undertaken at the suggestion of Professor Lindley, to ascertain the comparative evaporating properties of Evergreen and Deciduous Trees.'—('Jour. Hort. Soc. Lond.' vol. vi. parts iii. and iv. 1851.)

as in that of their ripened allies, wheat and barley. We will assume, however, for the purpose of illustration, that in the growth of hay, as in that of the grain-crops, about 300 parts of water will be exhaled for 1 part of dry substance assimilated; and since one of the experimental plots of meadow land at Rothamsted has given an average, over fifteen years, of 3 tons of hay, or about 2½ tons of dry substance per acre per annum, its growth would again represent an exhalation of about 750 tons of water per acre per annum—but extending in this case not later than to the middle or end of June.

We will now adduce some special cases illustrating the amount of water exhaled by different crops, and their dependence on the rainfall of the period of active growth, or on the supplies of moisture previously accumulated within the soil.

Results relating to the Growth of the Hay-crop.

The following Table (I.) shows the amount of hay obtained per acre each year for fifteen years in succession (1856-1870):—

1. Without manure.
2. With mixed mineral manure and 400 lbs. ammonia-salts per acre per annum.
3. With mixed mineral manure and 550 lbs. nitrate of soda per acre per annum (thirteen years only, 1858-1870).

The Table also shows, side by side with the records of produce, the amount of rain, in inches, which fell at Rothamsted each year

Table I.

Years.	Hay per Acre.				Rain at Rothamsted.			
	Without Manure.	Mineral Manure and Ammonia-salts.	Mineral Manure and Nitrate of Soda.	Mean.	April.	May.	June.	Total.
	Cwts.	Cwts.	Cwts.	Cwts.	Inches.	Inches.	Inches.	Inches.
1856	22¼	56¾	..	39½	2·61	4·70	1·91	9·22
1857	25½	57¾	..	41⅝	2·16	1·10	2·21	5·47
1858	22	64	50¾	45½	2·58	2·55	0·96	6·09
1859	22¾	55¾	54¼	44	2·70	2·09	2·72	7·51
1860	24¾	50¾	49¼	41½	1·94	4·30	6·26	12·50
1861	25¾	56¾	52¾	44⅝	1·28	1·04	2·98	5·30
1862	27½	57¼	51	45¼	2·84	2·91	3·41	9·16
1863	20¼	53¾	58¼	44¼	0·96	1·01	4·60	6·57
1864	24	50¼	60¾	45	1·25	1·88	1·79	4·92
1865	11½	34½	47¼	31¼	0·47	3·05	0·68	4·20
1866	23¾	41½	58¾	41¼	1·95	1·24	4·51	7·70
1867	29¾	48	64¼	47¼	2·82	3·35	1·06	7·23
1868	17½	59¼	69	48½	2·19	0·73	0·37	3·29
1869	38	68¾	76¼	61	2·13	3·23	1·07	6·43
1870	5¾	29½	56¼	30½	0·46	1·35	0·98	2·79
Average	22¾	52¾	57¾	43½	1·89	2·30	2·37	6·56

during the months of April, May, and June, which may be considered as including the period of active growth of the hay-crop.

Although there is much to be learnt from the results brought together in the foregoing Table, much more information than is there given would be required—as to the difference in the character of the herbage produced under the different conditions, the distribution of the rain, the degree and range of temperature, and the mutual adaptations of moisture, heat, and stage of growth of the plants—to enable us to account for all the fluctuations in the amounts of gross produce which the records show.

It is seen at a glance that the fluctuations from year to year in the amounts of produce without manure, though doubtless greatly dependent on the quantity and distribution of the rain falling during the period of active growth, by no means correspond with the fluctuations in the total amount of rain during the three months. Thus, the average fall for the three months is 6·56 inches, and the average produce of hay without manure is 22¾ cwts. But we have, with almost exactly the same total amount of rain during the same period in 1863 (6·57 inches), only 20¼ cwts. of hay; whereas, with even rather less (6·43 inches), in 1869, we have the heaviest produce obtained in any one of the series of 15 years, namely, 38 cwts. The fact is that, coincidently with the small produce of 1863, less than one-third of the total rainfall of the three months occurred during the first two months of the period; whilst, coincidently with the very heavy produce in 1869, there was considerably more than the average fall of rain in both April and May, and less than half the average fall in June; the result being that more than five-sixths of the total fell during the first two of the three months, when its influence upon the growth would be the greatest. Again, the heaviest total fall within the growing period was in 1860, when there was nearly double the average amount, whilst the produce only exceeded the average by less than 2 cwts. of hay; the facts being, that about half the total amount fell in June, that is, not until the last month of growth; and that the temperature was very unusually low almost throughout the period of active vegetation.

The lowest amounts of produce were—17½ cwts. in 1868, 11½ cwts. in 1865, and only 5¾ cwts. in 1870. This last, the lowest amount in the series, is coincident with the smallest amount of total rain over the three months throughout the fifteen years, namely 2·79 inches. With only 3·29 inches in the three months of 1868, there was a produce of 17½ cwts., but with 4·2 inches in 1865, there was only 11½ cwts. But whilst, in the latter year, there was in April only about one-fourth the average fall, and very high temperature, there was during the same month in 1868 more than the average fall, and about the average temperature.

Turning to the columns of produce obtained by the two artificial manures, it is seen that, whilst in the earlier years the mineral manure and ammonia-salts gave more hay than the mineral manure and nitrate of soda, in the later years the mineral manure and nitrate yielded considerably more than the mineral manure and ammonia-salts. It is obvious, therefore, that the fluctuations in the produce are dependent on other conditions than the variations in external or climatic circumstances alone. It will come within the special province of our subject to explain this further presently; but, in passing, we may here remark that the character of the mixed herbage in regard to the distribution of plants, and the prevalence of individual species, was very widely different in the two cases; and the dependence of the amount of produce on external supplies of moisture will, of course, be greatly measured by the degree of root range, and the consequent command of the moisture within the soil itself, of the particular species favoured.

These few observations will be sufficient to indicate some of the points of interest which the study of the subject in detail is calculated to elucidate, and to show the complexity of the conditions upon which the final result—the weight of hay—depends.

We will now turn to the more special object of the present communication.

The following are the amounts of hay obtained per acre in 1870, on each of the three plots already referred to, and also the average amounts over 15 years without manure, and with mineral manure and ammonia-salts, and over 13 years with mineral manure and nitrate of soda.

Table II.

	Hay per Acre.		
	1870.	Average 15 (or 13) Years, 1856-70.	Deficiency in 1870.
	Cwts.	Cwts.	Cwts.
Without manure	5¾	22¾	17
Mineral manure and ammonia-salts ..	29½	52¼	22¾
Mineral manure and nitrate of soda ..	56¼	57½	1¼

Thus, under the influence of the extraordinary drought of 1870, there was a variation in the amount of produce on closely adjoining plots, from only 5¾ cwts. of hay without manure, to

29½ cwts. with mineral manure and ammonia-salts, and to 56¼ cwts. with mineral manure and nitrate of soda. Indeed, without manure there was not only less produce than in any preceding year of the fifteen, but only about one-fourth the average amount. With mineral manure and ammonia-salts there was again considerably lower produce than in any other of the fifteen years with the same manure, and a deficiency of nearly 23 cwts. compared with the average. Notwithstanding this, we have the remarkable result of 2 tons 16 cwts. of hay produced by mineral manure and nitrate of soda, or only about 1¼ cwt. less than the average amount by that manure; about 2½ tons more than without manure, and 1½ ton more than by the mixture of mineral manure and an amount of ammonia-salts containing about the same quantity of nitrogen as the nitrate.

On the assumption that probably about 300 parts of water pass through the plants for one part of dry substance fixed, about 700 tons of water must have been exhaled by the herbage during the growth of the 56 cwts. of hay. But, reckoning an inch of rain to represent a fall of 101 tons per acre, the 2·79 inches which fell in 1870 during April, May, and June, the period of active vegetation, could only supply 282 tons of this, provided (which would not be the case) none of it was lost by drainage, and none of it passed off by evaporation otherwise than through the plants themselves. On the same assumptions, the amount which fell would be about 160 tons less than sufficient for the requirements of the crop grown by mineral manure and ammonia-salts, but more than three times as much as would be required by the growth of the unmanured produce.

So striking was the difference in the effect of the drought on two plots side by side, the one manured with mineral manure and a given quantity of nitrogen in the form of ammonia-salts, and the other with the same mineral manure and the same quantity of nitrogen, but the latter in the form of nitrate of soda instead of ammonia-salts, that it was decided, on the removal of the crop, to determine the quantities of water existing in the soil of the three plots to a depth somewhat greater than the lowest to which roots could be traced; and also to observe the difference in the development and distribution of the roots, if any, on the different plots. Accordingly, on July 25 and 26, 1870, samples of soil were taken from the three plots to the depth of 54 inches in each case, roots having been traced on one of them to within a few inches of that depth.

The plan of collecting and preparing samples of soil for analysis will be understood from the following description of the process in the present instance: A square yard, comprising a fair proportion of the species contributing to the bulk of the herbage, having been carefully selected on each plot, a case or frame, open at the top and bottom, made of strong sheet-iron, 6 inches square by 9 inches deep (but which may be of any desired size), was driven into the ground in the centre of the square, level with the surface. The enclosed soil was then dug out exactly to the depth of the case. The soil around the case, to the extent of the square yard selected, was then removed to the level of the bottom of it; it was again driven down, and its contents carefully taken out; and so on, the process was repeated, until the desired depth was attained. The determination of the water in the samples being the special object of the experiments in question, the exact weight of the soil was taken immediately on removal, so that any loss of moisture by evaporation during preservation, or preparation for analysis, might be duly taken account of. The whole was then broken up, the stones sifted out, separating first those which did not pass a 1-inch sieve, next a $\frac{1}{2}$-inch, and finally a $\frac{1}{4}$-inch sieve being used. The mould, or soil, passing the $\frac{1}{4}$-inch sieve was weighed, a proportional part of it finely powdered for analysis and re-weighed. In the soils so prepared, the loss of moisture, at different temperatures, has been, and the nitrogen and some other constituents will be determined.

The following Table shows the percentage of moisture, as determined by the loss when dried at 212° Fahr., inclusive of that by evaporation during preparation for analysis, in the soil from each of the three plots of the experimental meadow-land, at each depth to which the samples were taken:—

TABLE III.—MOISTURE in the Soil from Plots of Permanent Meadow Land differently Manured. Samples collected July 25–6, 1870.

Depth of Sample.	PERCENTAGES OF MOISTURE (Soils dried at 212° Fahr.).		
	Plot 3. Without Manure.	Plot 9. Mineral Manure and Ammonia-salts.	Plot 14. Mineral Manure and Nitrate of Soda.
First 9 inches	10·83	13·00	12·16
Second 9 inches	13·34	10·18	11·80
Third 9 inches	19·23	16·46	15·65
Fourth 9 inches	22·71	18·96	16·30
Fifth 9 inches	24·28	20·54	17·18
Sixth 9 inches	25·07	21·34	18·06
Mean	19·24	16·75	15·19

The results recorded in this Table are of great interest and significance; and they supply important data towards the explanation of the extraordinary difference in the amount of produce obtained on the different plots. It should be premised, however,

that between the removal of the crops and the date of sampling the soils, in all nearly an inch of rain had fallen, perhaps affecting somewhat the actual percentages, but the relative amounts probably but little.

The first point to remark is, that the first 9 inches of soil of both the heavily manured, and more or less heavily cropped, plots contained a higher percentage of moisture than that of the unmanured and lightly cropped plot. But from that point downwards to a depth of 54 inches, and doubtless further still, the manured and more heavily cropped soils contained much less moisture than the unmanured; and the most heavily cropped soil, that of Plot 14, manured with mineral manure and nitrate of soda, contained considerably less than that of Plot 9, manured with mineral manure and ammonia-salts. And whilst at a depth of from 45 to 54 inches the unmanured soil contained 25 per cent. of moisture, that receiving mineral manure and ammonia-salts contained only 21·34 per cent.; and that receiving mineral manure and nitrate of soda only 18 per cent., or scarcely $\frac{3}{4}$ths as much as the unmanured soil at the same depth. To sum up the results, there is an average amount of moisture down to the depth of 54 inches, of 19$\frac{1}{4}$ per cent. on the plot without manure, of only 16$\frac{3}{4}$ per cent. on the plot manured with mineral manure and ammonia-salts, and of scarcely 15$\frac{1}{4}$ per cent. on that manured with mineral manure and nitrate of soda, or only about $\frac{3}{4}$ths as much on the latter as on the unmanured plot.

The subsoil of this meadow land is a reddish yellow clay, interspersed with grey veins, and the specific gravity increases by about one-half from the surface down to the greatest depth taken. For our present purpose it will be a sufficiently near approximation to the truth to assume that down to the depth of 54 inches, the soil (exclusive of stones) weighed an *average* of 1,000,000 lbs. per acre for every 3 inches of depth, or an aggregate of 18,000,000 lbs. per acre to the depth of 54 inches. Adopting this estimate, and the percentages of moisture given in Table III., it results that down to the depth of 54 inches, or 4 feet 6 inches, the unmanured soil retained 1546, the soil of Plot 9, 1346, and that of Plot 14, 1221 tons of water. That is to say, to the depth of 4 feet 6 inches, the soil of Plot 9, manured with mineral manure and ammonia-salts, contained 200 tons, and that of Plot 14, manured with mineral manure and nitrate of soda, 325 tons less water per acre than that of the unmanured soil to the same depth; whilst, from the great difference in the percentage at the lowest depths taken in the three cases, there can be no doubt that the difference extended considerably deeper still.

Here, then, we have evidence of the source whence the manured crops derived the water required for their growth, over and above that supplied by the rain actually falling during the period of active vegetation. But the questions obviously arise—if the unmanured subsoil retained so much more water, why did the crop suffer from the drought so very much more than the manured crops? and why did the crop manured with mineral manure and ammonia-salts suffer so much more than that manured with mineral manure and nitrate of soda, and not avail itself so fully as did the latter of the stores of moisture within the soil? To gain some information on the points here suggested, careful examination was made of the distribution of species on the square yard of the plot selected, of the section of the soil and subsoil, and of the distribution of roots within them.

It should be stated that 53 species in all are found on the continuously unmanured plot; this great complexity of herbage being maintained in consequence of the little encouragement to luxuriance of any. On the other hand, by the application of mineral manure and ammonia-salts on Plot 9, and of mineral manure and nitrate of soda on Plot 14, for many years in succession, and the consequent great encouragement and predominance of certain individual species, the total number discernible has become reduced to 30 on each of these plots. And whils the herbage on the unmanured plot comprises 17 graminaceous, 4 leguminous, and 32 miscellaneous or weedy species, that of Plot 9 includes only 15 graminaceous, 2 leguminous, and 13 miscellaneous species, and that of Plot 14 only 14 graminaceous, 3 leguminous, and 13 miscellaneous species.

But such, again, is the difference in the character of the two nitrogenous manures—ammonia-salts and nitrate of soda—in regard to their reactions upon the soil, and the consequent degree of rapidity and range of distribution of them or their products of decomposition within it, that they respectively encourage the development of species of widely different underground, as well as above-ground habit of growth. Thus, the dominant plants were very different on the two manured plots. Under the influence of the annual application of mineral manure and ammonia-salts, *Dactylis glomerata* (rough cock's-foot), *Agrostis vulgaris* (common bent-grass), *Festuca ovina* (sheep's-fescue), and *Poa pratensis* (common meadow-grass), among graminaceous plants, and *Rumex acetosa* (sorrel-dock), among the miscellaneous herbage, prevailed somewhat in the order of enumeration; whilst under the influence of mineral manure and nitrate of soda *Bromus mollis* (soft brome-grass), had become so prominent as to constitute probably about one-half the crop; *Poa trivialis* (rough meadow-grass)was also very prominent, *Holcus lanatus* (woolly soft-grass),

Festuca ovina (sheep's-fescue), *Lolium perenne* (rye-grass), *Dactylis glomerata* (rough cock's-foot), *Avena flavescens* (yellow oat-grass), and among weeds *Anthriscus sylvestris* (wild beaked-parsley), coming next in order of prevalence. And, whilst the plants most encouraged by the ammonia-salts have a tufty habit of growth above ground, and a tendency to luxuriate within a limited range beneath the surface, some of those most favoured by the nitrate of soda, and especially under its influence, are very different in character, not growing in tufts, but producing comparatively uniformly dense herbage, with many stems, comparatively few root-leaves, and roots having a characteristically downward tendency, those of the *Bromus mollis* especially (which contributed such a large proportion of the whole crop) being strong and wiry, and descending far into the subsoil.

The sectional examinations, indeed, showed great differences in the character of the turf, in the prevalence and character of development of the roots within and below it, and in the character of the soil and subsoil, as the following brief abstract of the observations made will show. It should be first stated, however, that whilst on the square yard selected as characteristic of the unmanured plot, there were found 9 graminaceous, 4 leguminous, and 11 miscellaneous—in all 24 species; on that of Plot 9, having mineral manure and ammonia-salts, there were only 6 graminaceous, no leguminous, and only 3 miscellaneous species; and on that of Plot 14, receiving mineral manure and nitrate of soda, again only 6 graminaceous, only 1 leguminous, and 2 miscellaneous species.

Owing to the great complexity of the herbage on the unmanured plot, including a comparatively large number of leguminous, and miscellaneous or weedy species, some fleshy roots were observed at a considerable depth. The turf consisted of a complex network of fine roots and fibrils, which were much less in size and strength than in the case of either of the manured plots. These fine roots seemed to have more or less complete possession of the soil to a depth of about 6 inches, and some of them then showed a downward tendency; becoming, however, much fewer, and even in the second and third 9 inches extremely fine; and at a depth of about 40 inches they were as fine as a fibre of silk or a spider's web. It was concluded, though not with great certainty, that the roots found at the greatest depth were those of *Agrostis vulgaris* and *Bromus mollis*. The sample of the first 9 inches of the unmanured soil possessed the character of mould not much less than that of the manured plots; the second 9 inches, too, was very much altered from the character of the clay subsoil; but below this point very slight difference was observable; though, of the four lower samples, the uppermost, that is, the third from the surface, perhaps showed slightly the least, and the lowest, or sixth, the brightest red tinge.

The turf of Plot 9, manured with mineral manure and ammonia-salts, consisted of a dense, almost peat-like mass, of decomposing roots, radicle leaves, and stubble, thickly penetrated with strong roots and fibrils, the whole being as much matted as on the unmanured soil, showing, however, less complexity, but greater strength of roots. The horizontal subterraneous stems of the *Agrostis vulgaris* greatly predominated, emitting many fibrils, and sending out many descending fibrous roots. *Poa pratensis* also developed a large amount of strong root, and a profusion of fibrils. Roots penetrated to about the same depth as on Plot 3, but in larger quantity, and of larger size; being, however, in the fifth 9 inches, both very few in number and very fine. As already said, the samples of the first 9 inches of the soil of the three plots differed comparatively little from one another in the degree of their change by the action of vegetation; but, if anything, that of this Plot 9 was the darkest, indicating so far more of mould-like character. The second 9 inches of this plot was decidedly more changed than that of the unmanured, or of even Plot 14. The third and fourth 9 inches were, compared with the unmanured, slightly darker, or less bright in colour, showing still some change. The fifth and sixth were little, if at all, distinguishable in colour from the raw, reddish-yellow clay of the unmanured plot at corresponding depths.

The turf of Plot 14, manured with mineral manure and nitrate of soda, had not the peaty appearance of that of Plot 9; the prevailing plant, *Bromus mollis*, which made up about half the crop, possessing comparatively few radicle leaves; whilst, especially under the influence of this manure, *Poa trivialis*, *Holcus lanatus*, and *Lolium perenne*, have a tendency to assume the same character of development above ground. The *Bromus mollis*, too, was found in a most striking degree to send down strong wiry roots into the subsoil, leaving only its fibrils, and the roots of less prominent or smaller species, to feed near the surface. The second 3 inches of soil also held together, being full of fibre. At the extremity of the fibrils of the *Bromus mollis* small tubercles, much like those which occur on the roots of some leguminous plants, were observed down to a depth of perhaps 12 or 14 inches. The roots of this grass extended, however, to a depth of nearly 4 feet, still maintaining their wiry character. The difference in the character of the samples of soil, and especially of the subsoil, of this compared with those of either of the other plots, was very striking. The first 9 inches differed little from that of the unmanured plot. The second was, however, more altered

than that of the unmanured plot at the corresponding depth. The third, fourth, fifth, and sixth 9 inches were very strikingly different in appearance from the corresponding layers of either of the other two plots; the clay, instead of being of a comparatively uniform reddish yellow colour, was very much mottled or veined, showing a mixture of yellow, grey, red, and brown, with the yellow and grey predominating. So much was this the case that when the samples were powdered they were of a yellowish grey colour, instead of reddish yellow; and the lighter or less yellow the greater the depth of the sample, that of the sixth 9 inches being the lightest of all.

There was, perhaps, more of natural grey vein in the subsoil of this than in that of the other plots, but the difference in colour and texture was too great to be so accounted for. Upon the whole the lower layers were softer and more soapy than in the case of either Plot 3 or Plot 9; though, as Table III. at page 10 shows, they contained a considerably less percentage of moisture. Indeed, the subsoil of this plot had much more the appearance of disintegration from some cause than that of either of the others; it was consequently much more easily worked, and especially more so than that of the unmanured plot, which was very tough and hard.

To sum up these distinctions: it is seen that not only did different plants become dominant according to the different condition of the plot as to manure, but those which prevailed on the unmanured land, though numerous, had much finer and much less vigorous roots; the raw clay of the subsoil was much less changed; and it had yielded up very much less moisture to the growing crop. On the plot manured with mineral manure and ammonia-salts free-growing grasses predominated; but chiefly those whose underground habit of growth was such as rendered them dependent for their food and moisture in great measure on that which is to be found in the upper layers of the soil. Still, owing to the increased vigour of growth under the influence of the manure, it is seen that moisture was obtained, either directly by the roots of the plants, or by capillary action induced by the pumping out of the upper layers, from the extreme depths to which the samples were taken; and, from the great difference in the percentage of moisture at that depth compared with that of the unmanured plot, there is no doubt that the action extended deeper still. On Plot 14, on the other hand, where nitrate of soda was applied, the plant which contributed about half the produce had roots of a very characteristically downward tendency. We find the soil, to the depths examined, pumped drier still; and, coincidently, the drought has comparatively little affected the amount of the crop.

Intimately connected with the greater change in the subsoil of the plot manured with nitrate of soda than in that manured with ammonia-salts, with the greater predominance and luxuriance of the deeper-feeding herbage, and with the consequent little evil effects from the drought where the nitrate was employed, is doubtless the fact that the ammonia of the ammonia-salts is much more readily absorbed and retained by the soil than is the nitric acid of the nitrate. The latter, consequently, becomes, under the influence of rain, more rapidly distributed and washed into the subsoil, whither the roots follow it. As this filtration, into and through the subsoil, of a solution of the nitrate, or of its products of decomposition within the soil, has been proceeding for thirteen years in succession, there is little cause for surprise that the subsoil should have become much more changed than where the ammonia-salts had been used. It seems intelligible, too, that those plants of the herbage, whose habit of growth is characterised by a comparatively large development of descending roots, aided as they would be when once they had asserted their predominance by more and more self-sowing each succeeding year, should get such complete possession of the lower layers of the soil, with their stores of food and moisture. On this point it may be remarked, that the *Bromus mollis*, which so strikingly predominated on the nitrated plot, and whose roots, though only a biennial, had obtained more complete possession of the subsoil than those of any other plant, is one of the earliest of the grasses, and has, in point of fact, generally seeded to a greater or less extent before the crop has been cut.

It may be here mentioned in passing, that, wherever, in the course of the experiments at Rothamsted, nitrate of soda is employed year after year on the same plot of arable land, the difference in the appearance and texture of the soil is very great, and is discernible at a considerable distance. The soil apparently retains very much more moisture, becomes more agglutinated, and so sticky compared with that of adjoining plots under equal conditions of weather, as to be with difficulty worked at the same time, and never brought to the same tilth without the expenditure of extra labour upon it. It may be judged, indeed, that during the wet season the nitrated soil, and its more disintegrated subsoil, would acquire more moisture, or at least more available moisture, than the soil and raw clayey subsoil of the other plots.

We have, then, in the properties of the nitrate of soda and its effects upon the soil and subsoil, in the influence of these in determining the character of the prevailing herbage, and in the comparative independence of external sources of moisture which a deep root range gives to the plants encouraged, an explanation

of the fact that, notwithstanding the unusual drought of 1870, which almost suspended the growth of the unmanured herbage, and much diminished that manured with mineral manure and ammonia-salts, the plants which had gradually asserted possession over others on the plot continuously manured with mineral manure and nitrate of soda, should have yielded, under the same circumstances of scarcity of rain, an all but average crop.

Before leaving the subject of the influence of the drought of 1870 on the hay-crop, it may be added that a portion of the park adjoining the experimental plots was liberally manured with London stable-dung, but no benefit whatever was apparent, and the crop was so light as to be scarcely worth mowing.

The evidence at command in regard to the effects of the drought on other of the experimental crops, is not of the same, or in some respects of so direct a kind, as that relating to the mixed herbage, and to the soils, of the experimental plots of grass land. Nevertheless, some facts of interest may be recorded illustrating the influence of the moisture stored up within the soil on the growth of both wheat and barley.

Results relating to the Growth of Wheat.

The following Table (IV.) shows the amounts of grain, and the amounts of total produce (corn and straw together), obtained in the experimental wheat-field for 19 years in succession, 1852-1870 inclusive :—

1. On Plot 3, continuously unmanured.
2. On Plot 2, receiving 14 tons farmyard manure per acre per annum.
3. On Plot 7, receiving, annually, mixed mineral manure, and 400 lbs. ammonia-salts per acre.
4. On Plot 9A, receiving, annually, the same mixed mineral manure as plot 7, and 550 lbs. nitrate of soda per acre.

The Table also shows, side by side with the amounts of produce, the fall of rain each year during the months of April, May, June, and July, which may be said to include the period of active vegetation and accumulation of substance. It should be further explained, that, in order that the different amounts of grain from year to year may be more strictly comparable one with another, and to avoid the necessity of recording and considering the weight per bushel in each case, the total weight of dressed corn has been divided by 61, and the Table shows, therefore, not the actual number of measured bushels in each case, but the number of bushels of an assumed uniform weight of 61 lbs.

TABLE IV.—Produce of Wheat by different Manures, and fall of Rain during the 4 Months of active growth each Year, for 19 Years, 1852—1870.

YEARS.	DRESSED CORN. (In Bushels of 61 lbs.)					TOTAL PRODUCE. (Corn and Straw.)					RAIN AT ROTHAMSTED.				
	PLOT 3. Without Manure.	PLOT 2. Farmyard Manure.	PLOT 7 ab. Mineral Manure and Ammonia-salts.	PLOT 9a. Mineral Manure and Nitrate Soda.	MEAN.	PLOT 3. Without Manure.	PLOT 2. Farmyard Manure.	PLOT 5ab. Mineral Manure and Ammonia-salts.	PLOT 9a. Mineral Manure and Nitrate Soda.	MEAN.	April.	May.	June.	July.	TOTAL.
	Bushels.	Bushels.	Bushels.	Bushels.	Bushels.	lbs.	lbs.	lbs.	lbs.	lbs.	Inches.	Inches.	Inches.	Inches.	Inches.
1852	12¾	26¾	24¼	(¹)	21½	2457	5173	5440	(¹)	4357	0·52	1·84	4·70	2·28	9·34
1853	4¾	16	20¾	(¹)	13½	1772	4492	5101	(¹)	3788	3·00	1·73	3·47	4·49	12·69
1854	20¾	42¼	46¼	(¹)	36¾	3496	7155	8497	(¹)	6373	0·49	4·38	0·77	0·86	6·50
1855	16⅛	35⅛	32¾	28¾	28¼	2859	6082	6146	5878	5241	0·41	2·32	1·65	6·97	11·35
1856	12¾	34¾	34¾	30¾	28½	2450	6594	6757	5894	5424	2·61	4·70	1·91	1·48	10·70
1857	19	40⅞	44¾	43¼	36¼	2813	5910	6628	6634	5496	2·16	1·10	2·21	1·61	7·08
1858	17¾	39¾	39⅞	37¼	33⅞	2811	6349	6519	6701	5595	2·58	2·55	0·96	3·19	9·28
1859	15⅞	33⅞	31⅞	26⅞	27	3226	7073	6833	7076	6052	2·70	2·09	2·72	3·02	10·53
1860	11¾	29¾	24¾	27¼	23½	2197	5304	4675	6635	4703	1·94	4·30	6·26	1·99	14·49
1861	10¾	34¼	33⅞	31¼	27¼	1990	5303	5751	6607	4913	1·28	1·04	2·98	3·19	8·49
1862	15¾	38¾	34¾	42¾	32⅞	2709	6642	6143	8738	6058	2·84	2·91	3·41	1·80	10·96
1863	17⅞	45⅛	55	56¾	43⅞	2727	7165	9358	9888	7284	0·96	1·01	4·60	0·70	7·27
1864	16⅞	41	47¾	52¾	39¾	2428	6488	7970	9315	6551	1·25	1·88	1·79	0·89	5·81
1865	13⅛	37⅞	40¾	44¼	33¼	1861	5484	6249	7563	5289	0·47	3·05	0·68	2·93	7·13
1866	12¾	33	29¾	32⅛	26¾	2046	6128	5775	7377	5314	1·95	1·24	4·51	3·01	10·71
1867	8¾	27⅞	22¾	28¾	21¼	1505	4891	4179	6773	4337	2·82	3·35	1·06	4·10	11·33
1868	16¾	42¼	39¾	47¼	36¾	2027	6794	6317	8150	5821	2·19	0·73	0·37	0·37	3·66
1869	13¾	35¾	26⅞	36⅛	28	2198	6193	4972	7298	5165	2·13	3·23	1·07	0·97	7·40
1870	15¾	38	42	46⅞	35½	2002	5092	5836	6851	4946	0·46	1·35	0·98	1·12	3·91
Averages	14¼	35⅝	35¼	38⅞ (¹)	30¾	2398	6016	6267	7336(¹)	5406	1·72	2·36	2·43	2·37	8·88

(¹) In 1852, 1853, and 1854, there was no mineral manure employed on plot 9a, and the amounts of nitrate used were less than the quantity mentioned in the text. Hence the produce is not given for those years; and the average produce by the mineral manure and nitrate is taken over 16 years only.

The evidence afforded by the results in the foregoing Table is confessedly quite inadequate to show what are the climatic conditions favourable or otherwise to the growth of wheat. It is, however, quite sufficient for our present purpose, which is to illustrate the comparative independence of the crop on the mere amount of rain falling during the period of active vegetation. It will suffice to call attention to a few of the more extreme examples.

The four years of largest total fall of rain over the four months in question were, 1853, 1855, 1860, and 1867, and three of them were also the seasons of smallest average crop, both of corn and total produce, whilst the fourth (1855) was a season of generally less than the average produce. On the other hand, the three years of highest produce, both corn and total produce, were 1854, 1863, and 1864, and all three were seasons of less than the average fall of rain during the four months of active growth. Lastly, the two seasons of lowest fall of rain during April, May, June, and July were 1868 and 1870; and both gave, with each of the four conditions as to manure, more than the average produce of corn over the nineteen years; and in 1868, though not in 1870, there was even more than the average of total produce also, under each of the three manured conditions. But although there was, in both these years of great deficiency of rain during the growing period, more than the average produce of corn without manure, there was, in both, less than the average amount of both straw and total produce.

As in the case of the hay crop, so again with the wheat, it is seen that, whilst during the earlier years the mineral manure and ammonia-salts gave more produce, both corn and total produce, than the mineral manure and nitrate of soda, during the later years the nitrate has given more, and sometimes considerably more, of straw especially, than the mineral manure and ammonia-salts. The questions arise, how far may this be due: to the more rapid and more extended distribution of the nitrate of soda, or its products of decomposition, within the soil and subsoil? to the mutual reactions of the manure and the soil? to the greater power of retention of moisture acquired by the latter, as the result of such reaction? and to more active root development in the spring under these conditions?

Unfortunately, no comparative determinations of moisture in the soils of these two plots, or of root development, have been made, so as to obtain direct evidence in regard to the questions here suggested. Due weight should, however, be given to the fact that, whilst the ammonia-salts are sown in the autumn, before the seed, the nitrate is applied as a top-dressing in March. It is known that nitrate of soda, or its nitric acid in combination with some other base, distributes more rapidly, and, under equal circumstances as to rain, is more liable to be washed into the subsoil or the drains, than is the ammonia of the ammonia-salts. Hence it is not applied until the commencement of active growth, when the plant is able rapidly to avail itself of it. It is also known that a portion of the ammonia of the ammonia-salts itself becomes converted into nitric acid, and then is subject, in like manner, to loss by drainage; but to what degree a saturated condition of the soil during winter may cause serious loss, in this way, of the ammonia applied as ammonia-salts in the autumn, is a question not yet sufficiently investigated, and to which we shall make some further reference before concluding.

Although, as has been said, there is no evidence at command in regard to wheat, in reference to the questions above raised, so direct as that referring to the meadow land, yet the results now to be adduced nevertheless supply interesting and important data in respect to the variation in the amount of moisture within the soil at different depths, as affected by season, by manure, and by the growth of the crop.

Such were the drought and heat of May, June, and July, 1868, that it is hardly possible to suppose conditions more calculated to induce extreme dryness of soil than those preceding the harvest of that year. Accordingly, towards the end of July, just before the crop was ripe, samples of soil were taken from three plots of the experimental wheat-field, with the special view of determining the amount of moisture retained at different depths. The plots selected were:—

Plot 3. Without manure, since 1839.

Plot 2. With 14 tons farmyard manure per acre per annum.

Plot 8*a*. With mixed mineral manure, and 600 lbs. ammonia-salts per acre per annum.

The mode of collecting the samples was that already described, excepting that the iron frames employed were only 3 inches deep, instead of 9; the object being to determine the amounts of moisture at each 3 inches of depth, down to a total depth of 36 inches, or rather below the pipe-drains.

The subsoil of the farm consists of a tolerably tenacious reddish-yellow clay, resting upon chalk, and the corn crops seldom suffer from a scarcity of rain. At the time the samples were taken, the wheat had suffered but little from the drought, as the results already quoted show. But barley and oats were exceedingly light crops, and a bean crop in an adjoining field was quite dried up and dead for want of moisture.

For comparison with these samples taken at a time of extreme dryness, others were collected from the same plots in January, 1869, after much rain during the preceding ten days; the drains were running, and it was supposed that the ground was quite saturated. It was, indeed, so wet that it was necessary to lay down boards for the men to stand upon whilst working.

Table V., overleaf, shows the percentages of moisture in the different samples of soil; bringing together—first, the results for the three plots during the drought; second, those for the three plots when the land was saturated; and lastly, the same results arranged for the convenient comparison of the percentages in the dry state and the wet state, and showing the difference between the two, for each plot separately.

It will be obvious that the amount of water at the different depths in July, 1868, after about three months of great deficiency of rain, and the growth of a crop then approaching ripeness, must, in the main, be dependent on the supplies accumulated during the previous winter and early spring. But it is affected, to a greater or less depth from the surface: by any difference of texture and power of absorption, the result of previous cultivation, manuring, and cropping; by the influence of the pipe-drains, which are at a depth of about 30 inches; also, by the shade of the crop on the one hand, lessening evaporation from the soil itself, and on the other, by the requirements of the growing crop increasing, according to its amount, the exhalation through the plants themselves, and the consequent pumping out of the stores within the soil.

The soil of Plot 3, which had received no manure and produced little root (tending to disintegrate the soil and increase its absorptive surface), which had comparatively little shade from the growing plants, preventing surface evaporation, and whose crop would exhale comparatively little, is seen to retain a somewhat less percentage of water than either of the others within 3 inches of the surface, but more than either within the next 9 inches. In it, as in the others, the percentage of moisture increased gradually from that point downwards, until obviously affected by the action of the pipe drains.

The soil of Plot 2, which had then been manured with 14 tons of dung per acre per annum for twenty-five years in succession, notwithstanding the greater requirements of the crop, retained rather more moisture than the unmanured soil within 3 inches of the surface; a result partly due, perhaps, but not wholly, to more shade. But, from that point downwards, doubtless influenced by the requirements of the crop, the dunged soil retained less at every stage (excepting the lowest) than the unmanured.

The soil of Plot 8, manured annually with mineral manure and ammonia-salts, and yielding pretty uniformly a heavier crop

TABLE V.—PERCENTAGES of MOISTURE, in SUMMER and in WINTER, in the SOIL at different depths, of PLOTS in the EXPERIMENTAL WHEAT-FIELD differently manured.

Nos. of Samples; each 9 Inches deep.	Collected July, 1868.				Collected January 6-7, 1869.				Plot 3. Without Manure.			Plot 2. Farmyard Manure.			Plot 8a. Mineral Manure and Ammonia-salts.		
	Plot 3. Without Manure.	Plot 2. Farmyard Manure.	Plot 8a. Mineral Manure and Ammonia-salts.	Mean.	Plot 3. Without Manure.	Plot 2. Farmyard Manure.	Plot 8a. Mineral Manure and Ammonia-salts.	Mean.	Collected July, 1868.	Collected Jan. 6, 1869.	Difference.	Collected July, 1868.	Collected Jan. 7, 1869.	Difference.	Collected July, 1868.	Collected Jan. 6, 1869.	Difference.
1	4·05	4·48	4·31	4·28	21·43	39·67	26·53	29·21	4·05	21·43	17·38	4·48	39·67	35·19	4·31	26·53	22·22
2	7·20	7·01	6·07(¹)	6·76	24·54	35·62	22·93	27·70	7·20	24·54	17·34	7·01	35·62	28·61	6·07(¹)	22·93	16·86
3	8·91	7·38	6·66	7.65	24·35	28·85	20·62	24·61	8·91	24·35	15·44	7·38	28·85	21·47	6·66	20·62	13·96
4	10·65	8·14	8·45	9·08	21·41	23·95	24·07	23·14	10·65	21·41	10·76	8·14	23·95	15·81	8·45	24·07	15·62
5	11·24	9·98	12·44	11·22	22·07	20·59	24·84	22·50	11·24	22·07	10·83	9.98	20·59	10·61	12·44	24·84	12·40
6	13·20	12·26	14·34	13·27	21·48	21·07	24·79	22·45	13·20	21·48	8·28	12·26	21·07	8·81	14·34	24·79	10·45
7	14·03	12·51	15·20	13·91	21·82	26·96	23·69	24·16	14·03	21·82	7·79	12·51	26·96	14·45	15·20	23·69	8·49
8	15·09	12·91	16·86	14·95	23·59	24·87	28·98	25·81	15·09	23·59	8·50	12·91	24·87	11·96	16·86	28·98	12·12
9	16·84	13·78	17·98	16·20	24·74	25·75	27·01	25·83	16·84	24·74	7·90	13·78	25·75	11·97	17·98	27·01	9·03
10	18·03	13·45	18·53	16·67	25·71	25·34	28·59	26·55	18·03	25·71	7·68	13·45	25·34	11·89	18·53	28·59	10·06
11	14·64	14·49	17·67	15·60	23·97	25·18	28·93	26·03	14·64	23·97	9·33	14·49	25·18	10·69	17·67	28·93	11·26
12	15·44	16·11	16·85	16·13	22·94	22·75	27·40	24·36	15·44	22·94	7·50	16·11	22.75	6·64	16·85	27·40	10·55
Mean	12·44	11·04	12·95	12·14	23·17	26·71	25·70	25·12	12·44	23·17	10·73	11·04	26·71	15·67	12·95	25·70	12·75

(¹) There was an error in the determination in this case; and the figure given is calculated on the assumption that the amount of the moisture in the second 3 inches would probably bear about the same relation to that in the first and third 3 inches as the average in the cases of Plots 3 and 2.

than the dung, shows less moisture within the first 9 inches, and but little more within the next, or fourth 3 inches, than that of the dunged plot; also a total to that depth considerably less than the unmanured soil. From that point, however, there is a gradually increasing amount down to the range of the drains; notably more than in the dunged soil, and even more than in the unmanured, whose crop could only have withdrawn from it about one-third as much.

Supposing the three plots to have possessed exactly the same character of soil and subsoil, and to have contained the same amount of moisture to a given depth at the time of the commencement of active growth, we could well understand that, when the growth was nearly completed, the subsoil of the dunged plot, growing more than three times the crop, should contain less moisture than the unmanured subsoil. But, on the same suppositions, it would be difficult to account for the subsoil of Plot 8*a*, which grew even a larger crop than the dung, retaining not only more than the subsoil of the dunged plot, but more also than that of the unmanured plot. The differences between plot and plot as to percentage of moisture are, it is true, in some cases not great. But there is too much regularity and consistency in the results to admit of the supposition that the differences are due to errors arising from the unavoidable difficulties incident to the collection, weighing, and preparing the samples for drying, without some error of experiment affecting the estimation of the amount of water. The results relating to the soils and subsoils when supposed to be in a state of saturation will show, indeed, that the active growth of the crops probably did not commence with equal soil-supplies of moisture in the three cases.

The unmanured soil, when saturated, contained, to the depth examined, not much less than one-fourth its weight of water, and nearly twice as much as in the dry condition. The range of variation in the percentage was much less than in the dry soil; but, on the other hand, the order and degree of increase or decrease is much less regular in the wet soils. The top 3 inches contained rather less water than the second and third; otherwise, there would seem to be, at the time of saturation, more water near the surface, then a decreasing amount, and then a gradually increasing quantity, until the range of the drains is reached.

The dunged soil, with its vast accumulation of organic matter, and doubtless greater degree of disintegration, porosity, and power of absorption within some distance from the surface, is seen to hold about one and a half times as much water within the first 6 inches as the unmanured soil, or even as that manured with mixed mineral manure and ammonia-salts. The third 3 inches, also, contains more than either; and the fourth more than the unmanured, and about as much as the artificially manured soil. The quantity continues to diminish to the fifth 3 inches, and then increases to about the level of the drains. To the total depth examined, the dunged soil contained more than a quarter of its weight of water, about 3½ per cent. more than the unmanured, and about 1 per cent. more than the artificially manured soil.

The soil receiving mineral manure and ammonia-salts also retained more water within what may be called the staple than immediately below it. It then again increased in percentage of moisture, more or less regularly, until within the direct influence of the drains. It is to be observed, too, that, whether owing to a greater retentive power of the natural clay at that point, or more probably to the accumulation, and the action, of the constituents of the manures, or of their products of decomposition, rendering the clay more hygroscopic, the lower layers of the soil of this plot retained considerably more water when saturated than did the corresponding layers of either of the other plots. The amount of water to the total depth was about 2½ per cent. more than in the unmanured soil, but not so much as in the dunged soil.

As might be expected, there are greater irregularities of increase or decrease indicated in the percentages of water at the different depths, among the results relating to the saturated, than among those relating to the dry soils. This may be due in part to accidental differences of permeability of the soil, and consequently to variation in the freedom of access of the percolating water, at the different points; but it is, doubtless, partly due to unavoidable error in the collection, weighing, and after-manipulation, of soil in so wet a condition.

Disregarding the irregularities, however, and interpreting the obvious direction of increase or decrease of moisture at the different depths, it is pretty clear that, down to a certain depth from the surface—which varied in the different plots according to the varying power of retention of the staple and immediately subjacent layers—the increased percentage of moisture was due to the comparatively recent rains. There was then reached the layers partially drained since the preceding rains, from which point downwards the percentage increased, until again reduced by the action of the pipe-drains.

Further, it is obvious that, by evaporation from the surface, and the consequent withdrawal by capillary action of water from below upwards on the one hand, and by the gradual descent, aided by the natural drainage of the chalk and the artificial

drainage of the pipes, on the other, what may be called the normal supply of water within the soil would, doubtless, at the commencement of active growth, be considerably less than that indicated by the percentages in the saturated soils. There is also good reason to suppose that, owing to the action of the manures, or their products of decomposition, within the soil and subsoil, the manured plots would retain more than the unmanured; and further, that whilst the effects of the dung would be chiefly to increase the retention by the upper layers, those of the artificial manures would be more characteristically to increase the amount retained by the lower layers.

This brings us to a comparison of the amount of water in each plot in the two conditions of unusual dryness and of saturation or abnormal wetness, as shown in the right-hand half of the Table V.

Referring first to the unmanured soil, there is seen to be a difference of more than 17 per cent. of moisture between the wet and dry conditions of the staple, or uppermost 6 inches of soil. The difference then diminishes, more rapidly at first, until, in the lower layers, it ranges from under 8 to about 9 per cent. There is an average of about 10¾ per cent. more water in the wet than in the dry soil to the total depth examined.

The difference between the saturated and the dry conditions of the various layers of the dunged soil is much more striking still: amounting to over 35 per cent. within the first 3 inches, to nearly 29 per cent. in the second 3 inches, to more than 21 per cent. in the third 3 inches, and to nearly 16 per cent. within the next, or fourth, 3 inches. It then lessens considerably, again increases, and again diminishes to within the range of the drain-pipes. The result is that, within the uppermost 12 inches of soil, there is an increase of about 25 per cent. of moisture in the wet as compared with the dry condition; or, taking the total depth of 36 inches, there is an increase of over 15½ per cent.

The artificially manured soil also shows, almost throughout, greater difference in the amount of water retained in the two states than the unmanured, but less than the dunged soil. In the lower layers there are, as in the case of the dunged plot, some irregularities not satisfactorily explained. The final result, to the total depth of 36 inches, is an average of nearly 13 per cent. more water in the wet than in the dry condition.

It will be useful to compare the actual amounts of water per acre, in the different soils to the total depths examined, which the percentage results represent. Reckoning, as before, the soil in the dry state to weigh, exclusive of stones, an average of 1,000,000 lbs. per acre for each 3 inches of depth, we have 12,000,000 lbs. for the weight of the dry soil to the depth of

36 inches; and allowing one-eighth more for the wet soil, we have 13,500,000 lbs. per acre for its weight to the depth of 36 inches. Adopting these figures, and the average percentage of moisture in the soil of each plot, we have the following amounts of water per acre on the respective plots in the two conditions:—

TABLE VI.

	July, 1868. Dry.	January, 1869. Saturated.	Difference.
Tons of Water, per Acre, to a depth of 36 inches.			
Plot 3.—Unmanured	666	1396	730
Plot 2.—With Farmyard Manure	591	1610	1019
Plot 8a.—With Mineral Manure and Ammonia-salts	694	1549	855
Tons of Water, per Acre, over (or under) Plot 3.			
Plot 2.—With Farmyard Manure	−75	214	289
Plot 8a.—With Mineral Manure and Ammonia-salts	28	153	125

Thus we have on the unmanured plot 730, on the dunged plot 1019, and on the artificially manured plot 855 tons, more water per acre, to the depth of 36 inches, when the soils were saturated than when in the dry condition. As already said, the soils would not retain such an amount of moisture at the time of the commencement of active vegetation. But, by way of illustration, it may be stated that if they retained even two-thirds of the indicated difference prior to the commencement of the drought, and the commencement of active growth in 1868, the amount would be considerably more than would be required by the unmanured crop, and would supply a large proportion of that required by the manured crops, on the supposition that about 300 parts of water would be exhaled by the plants for 1 part of dry substance fixed by them. The soil-resources of moisture available to the growing crop would, however, doubtless extend beyond the depth to which the examinations refer. Then again, the amount of rain which actually fell during the period of active growth, though comparatively small, would, nevertheless, be not immaterial considered in relation to the balance of the requirements of the crops.

A very remarkable point connected with these results is, however, the difference in the amount of water retained per acre to a given depth by the soils of the different plots when saturated.

The unmanured soil and subsoil, comparatively little disturbed and disintegrated by the permeation and the decomposition of roots, and not at all by the action of manures, would offer less surface and absorb less water, and they are seen to retain less than those of either of the manured plots. The soil and subsoil of the artificially manured plot would be affected by the permeation not only of more roots, but of the solution of the manures or of some of their products of decomposition,—by the latter especially in the lower layers. But it is the dunged plot, with its vast accumulation of organic matter near the surface, and its finely divided and dissolved products of decomposition permeating to a greater or less depth beyond, and, doubtless, a considerable development of root, that is seen to possess the greatest power of retention of moisture, especially near the surface.

Taking the figures relating to the saturated soils as they stand, the artificially manured plot retained 153 tons, and the dunged plot 214 tons more water per acre, to the depth examined, than the unmanured—amounts which represent, respectively, about 1½, and more than 2 inches of rain. Or, if we take the difference between the amounts retained in the dry and the wet conditions, the dunged soil shows a still greater excess of absorption when saturated, both compared with the unmanured, and with the artificially manured soils. Further, the details show that the dunged soil, when saturated, retained, within 12 inches of the surface, an excess of water which would be equivalent to about 1½ inch of rain more than that held to the same depth on either of the other plots.

In connection with this interesting fact, it may be mentioned, that whilst the pipe-drains from every one of the other plots in the experimental wheat-field run *freely*, perhaps on the average four or five times annually, the drain from the dunged plot seldom runs at all more than once a year: indeed, it has not with certainty been known to run, though closely watched, since about this time last year. At first it was thought that there must be some stoppage, or some fault in the levels. Accordingly, the soil was opened in various places, but was found to be far from saturated down to the range of the drains. It was then concluded that the result was due to the greater power of absorption and retention of moisture by the dunged soil near the surface; and even supposing the figures above given should exaggerate the difference actually occurring, there would still be a wide margin remaining, sufficient to account for the fact of no water reaching the drains excepting under the influence of an unusually large and continued rainfall. Such a fact as the one here recorded is obviously of great interest and significance. Whether the porosity of a clay soil be increased by the application of manure, by mechanical means, or by a combination of the two, its power to absorb and retain water, without being wet, and in an available state, will be proportionately increased, and the necessity for artificial drainage, at any rate on some soils, would be greatly obviated.

From the results adduced, it may safely be concluded, as already intimated, that the three plots would retain different amounts of water, due to the previous winter rains, at the time of the commencement of active vegetation in the spring. And although the actual amounts of excess indicated by the figures in Table VI. may not be true measures of the increased retention by the manured as compared with the unmanured soil, and although the excess at any one time may not be sufficient to meet the increased requirements of the manured crop, it must be supposed that the soils of higher retentive power would retain proportionally more of every heavy shower falling from time to time during growth; and hence may be accounted for the differences, not at first sight adequately explained, in the amounts of water retained by the different soils at the period when they had supported, and nearly carried to completion, such widely different amounts of crop.

Have we not, also, in the fact that the soil and subsoil, to a considerable depth, may frequently during the winter be saturated with water, a probable explanation, of part at least, of the less effect of a given amount of nitrogen applied in the autumn in the form of ammonia-salts, than of an equal amount supplied in the spring as nitrate of soda? For although the ammonia of the ammonia-salts is in great part absorbed by the upper layers of the soil, it is well established that a portion of the nitrogen supplied as manure in the form of ammonia becomes converted into nitric acid, and reaches the drains in the form of a nitrate; and it may be assumed that this action would, other things being equal, be the greater the greater the amount of water passing through the soil. Professor Voelcker, who has analysed many of the drainage waters collected at different times from the several plots in the experimental wheat-field at Rothamsted, has, moreover, found a greater amount of nitric acid in them the greater the amount of ammonia-salts applied as manure.

Another reason which may in part explain the frequent less effect of a given amount of nitrogen applied as ammonia-salts than of an equal amount applied as nitrate of soda, even when both are sown at the same time in the spring, may be that, as the nitric acid of the nitrate distributes more rapidly under the influence of rain than does the ammonia of the ammonia-salts,

so may the development of root be the more encouraged under the influence of the nitrate; and so, proportionately, will the plant gain greater possession of the soil, and consequently be able to avail itself of a wider range of both food and moisture within a given time. Further, from the results which have been recorded on the point in the foregoing pages, it would seem that when the nitrate is applied year after year on the same plot for many years in succession, the action on the soil and subsoil of its solution, or of that of the products of its decomposition, tends to increased disintegration, and to increased power of retention of moisture, and thus, again, to encourage a greater extension of root.

Results relating to the Growth of Barley.

Our next and last illustrations have reference to the growth of barley. This crop has been grown at Rothamsted for nineteen years in succession on the same land, without manure, with farm-yard manure, and with numerous artificial mixtures each year. The fluctuations in the amount of produce dependent on season, manure, and the continued growth of the crop, being greater than in the case of wheat, it would occupy too much space to follow up the same line of illustration as that adopted in regard to that crop; and it is the less necessary or desirable to do so, as we hope to report the whole of the results after the twentieth crop in succession has been harvested.

Referring to the influence of the variation of rainfall from year to year, it will suffice to say here that extremely low produce of barley was obtained with both a great excess and a great deficiency of rain during the months of active vegetation. The bad result with excess of rain was coincident with unusually low, or unusually high temperatures; and that with deficiency of rain with high temperatures. On the other hand, the highest amounts of produce were obtained with only moderate amounts of rain during the growing period, provided there were a favourable distribution of it, and a favourable adaptation of temperature. And whilst an excess of rain, during the growing months, is adverse to the favourable growth of both wheat and barley, a great deficiency of rain during that period is found to be, as would be anticipated, more adverse to the spring-sown barley than to the winter-sown wheat.

In the experiments on barley, equivalent amounts of nitrogen, as ammonia-salts and nitrate of soda respectively, have not been employed in conjunction with mineral manures from the commencement; but where they have been employed, each separately, without such admixture, a similar result is observed as with both hay and wheat. That is to say, higher amounts of both corn and total produce have been obtained from the use of a given amount of nitrogen applied as nitrate of soda, than from that of an equal amount applied as ammonia-salts—both manures being in the case of barley sown in the spring.

In 1868 experiments were commenced in which nitrate of soda was used in conjunction with mineral manures, and below are given the results obtained in 1868, 1869, and 1870, with mixed mineral manure and 200 lbs. of ammonia-salts per acre per annum, compared with those of the same mixed mineral manure and 275 lbs. of nitrate of soda, which is estimated to contain about the same quantity of nitrogen as the ammonia-salts. As in the case of wheat, not the actual number of bushels measured, but the bushels of dressed corn calculated at an assumed uniform weight per bushel are given. For barley, 52 lbs. per bushel is taken.

TABLE VII.—Showing the effects on the Barley Crop of a given amount of Nitrogen as Ammonia-salts, compared with an equal amount as Nitrate of Soda.

	Dressed Corn. (In bushels of 52 lbs.)		Straw.		Total Produce. (Corn and Straw.)	
	Mineral Manure and Ammonia-salts.	Mineral Manure and Nitrate Soda.	Mineral Manure and Ammonia-salts.	Mineral Manure and Nitrate Soda.	Mineral Manure and Ammonia-salts.	Mineral Manure and Nitrate Soda.
	Bushels.	Bushels.	lbs.	lbs.	lbs.	lbs.
1868 ..	37	49	2333	2808	4311	5454
1869 ..	54⅜	58⅜	3853	4265	6701	7194
1870 ..	41⅞	48⅞	2090	2050	4287	4621
Mean	44⅜	50⅞	2759	3061	5100	5756

Here, then, we have again a similar result. There is, too, proportionately a greater increase with the nitrate, especially of corn, in the two drier and hotter seasons of 1868 and 1870—years, in fact, of summer drought.

The following Table shows the produce of barley without manure, with farmyard manure, and with mixed mineral manure and 200 lbs. ammonia-salts per acre, in 1868, and in 1870, the two recent years of summer drought; and also, under the same conditions as to manure, the average produce over the nineteen years of the experiment. As before, the number of bushels of dressed corn, reckoned at an uniform weight of 52 lbs. per bushel, is given. And, side by side with these records of produce, is given the amounts of rain at Rothamsted, in April, May, June, and July, each year, those being the months of active growth of the barley crop.

TABLE VIII.

	Dressed Corn. (In bushels of 52 lbs.)				Total Produce. (Corn and Straw).				Rainfall at Rothamsted.				
	Without Manure.	Farmyard Manure.	Mineral Manure and Ammonia-salts.	Mean.	Without Manure.	Farmyard Manure.	Mineral Manure and Ammonia-salts.	Mean.	April.	May.	June.	July.	Total.
	Bush.	Bush.	Bush.	Bush.	lbs.	lbs.	lbs.	lbs.	Ins.	Ins.	Ins.	Ins.	Ins.
1868	11¼	47¼	37¼	32	1902	5281	4311	3831	2·19	0·73	0·37	0·37	3·66
1870	13¾	52¼	41⅞	35¾	1489	4949	4287	3575	0·46	1·35	0·98	1·12	3·91
Average, 19 Years, 1852–1870 ..	20	50¼	48⅜	39¾	2453	5856	5786	4698	1·79	2·36	2·43	2·30	8·88

As there has been a decline in the produce without manure during the second as compared with the first half of the period over which the experiments have extended, the difference indicated between the unmanured produce in the years of drought and that over the nineteen years will exaggerate the deficiency due to the deficient rainfall alone during the four growing months of the two years in question. On the other hand, the produce by farmyard manure has considerably increased during the latter half of the period, and hence the deficiency in the years of drought which the figures show for that manure is less than is due to the characters of the seasons alone. With the artificial manure the produce was, however, very much more nearly equal during the first and second halves of the total period, and the indicated deficiency in the years of drought probably more nearly represents that really due to the characters of the seasons in its case. With this manure there was a deficiency compared with the average, of 11 bushels of corn in 1868, and of 6⅜ bushels in 1870; or, of total produce, of 1475 lbs. in 1868, and of 1499 lbs. in 1870. There was not far from an equal total amount of rain during the four months in the two seasons; but whilst there was more than an average fall in April, 1868, and only about one-fourth the average fall in April, 1870, there was a greater deficiency in May, June, and July, 1868, than in the same months in 1870. The result was a greater deficiency of corn, but a less deficiency of straw, in 1868 than in 1870.

We are enabled to adduce more direct experimental evidence showing the extent to which the barley-plant can avail itself of the stores of moisture within the soil, than that which was at command relating to wheat.

Before considering the results themselves, to which reference is here made, it will be well to describe briefly the circumstances under which they were obtained. With a view to the determination of what proportion of the rainfall passes to given depths in the subsoil, under different conditions of season, manuring, and cropping, a series of experiments has been commenced, for the cutting off, and the collection, of the drainage-water from the land at different depths—an essential condition being that neither soil nor subsoil should be disturbed. Leaving out of view for the present the questions of the influence of different manures, or of the growth of different crops, early in 1870 three plots of uncropped land, each of one-thousandth of an acre area, were selected, with a view of determining the amount of water passing below the depths of 20, 40, and 60 inches, respectively. The plan of operating was, to cut a sufficiently wide trench for men to work in, down one side of the plot, to a considerably greater depth than that at which the drainage was to be cut off. The plot was then carefully undermined and shored up at the depth decided upon, until a cast-iron plate, rather more than the length of the plot, 8 inches wide, and having small holes for the water to drain through, could be got in and fixed underneath. The plot was then further undermined, until another plate could be put in; and so on, until the whole was supported at the proper depth, without disturbance, by a perforated iron flooring, which finally was itself supported on three sides by brickwork, and on the fourth and across the middle by iron girders. The three as yet undisturbed sides of the plot were then trenched round; a 4½-inch brick and cement wall was built round the plot, resting on the projecting rim of the iron flooring below, and finished level with the surface above. The trench outside the wall was then filled in again. Thus, the exact area required was cut off from the surrounding soil by brickwork at the sides, and below, at the depth required, by a perforated iron flooring.

The field in which these *drain-gauges* were made, had grown wheat in 1869, and was sown with barley in March, 1870, and the drill by mistake was allowed to sow two rows of seed on the plots along one side of them. As the excavations proceeded, barley-roots were observed to have extended to a depth of between 4 and 5 feet, and the clayey subsoil appeared to be much more disintegrated, and much drier, where the roots had penetrated than where they had not. Accordingly, it was decided to make careful notes on the sections under the two conditions, and also to take samples of soil and subsoil to a depth below that at which roots

were traced, with a view to the determination of the amounts of moisture at the different depths in the two cases. Portions of the barley-ground and the fallow-ground, closely adjoining the drain-gauge plots, but undisturbed by the excavations in connection with them, were selected, and from each six samples, 6 × 6 inches superficies by 9 inches deep, that is, in all to a depth of 54 inches, were taken.

The following Table shows the percentages of moisture in the different samples, including that lost during their preparation, as well as that afterwards expelled at a temperature of 212° Fahr. :—

TABLE IX.—Percentages of Moisture in Uncropped and in Cropped Land, at different depths.

Samples collected June 27th and 28th, 1870.

Depth of Sample.	Fallow Land.	Barley Land.	Difference.
First 9 inches	20·36	11·91	8·45
Second 9	29·53	19·32	10·21
Third 9	34·84	22·83	12·01
Fourth 9	34·32	25·09	9·23
Fifth 9	31·31	26·98	4·33
Sixth 9	33·55	26·38	7·17
Mean	30·65	22·09	8·56

Before commenting on these results, it should be stated that, ten days previous to the collection of the samples, about two-thirds of an inch of rain had fallen, and only three days before the collection about one-tenth of an inch; and hence, perhaps, may in part be accounted for the somewhat high percentage of moisture in both soils near the surface at that period of a season which was upon the whole one of unusual drought. Further, for a few days during the interval since the heavier rainfall, some soil, thrown out from the excavations near, had laid upon the spot whence the samples from the uncropped land were taken, and hence, again, may be accounted for part of the excess near the surface in the uncropped as compared with the cropped land.

The difference between the amounts of water retained at the depths examined by the uncropped and the cropped ground, at points only a few feet apart, is very striking; and that it should be greater in the upper portions of the subsoil, which had probably contributed more to the exigencies of the growing crop than the lower layers, is what would be expected. The percentage of water in the subsoil even of the cropped land was very high—indeed nearly as high at corresponding depths as in that in the experimental wheat-field in January, 1869, when it was supposed to be in a state of saturation; whilst the amount in the subsoil of the uncropped land was not only considerably higher than in that of the cropped land, but considerably higher also than in that of the saturated wheat soil. We shall recur presently to the difference in the percentage of moisture in the soils and subsoils of the different fields which have been referred to, but must first direct attention to the more special application of the results now under consideration.

The following Table shows the number of tons of water per acre retained to the total depth of 54 inches, or $4\frac{1}{2}$ feet, by the uncropped and the cropped land, and the difference between the two. The upper line gives the amounts calculated according to the actual weights of the measured samples of soil (exclusive of stones), and the lower line the amounts, assuming that (exclusive of stones), the dry or barley soil would weigh 18, and the wet, uncropped or fallow soil $19\frac{1}{2}$ million lbs., to the depth of 54 inches:—

TABLE X.—Tons of Water per Acre to the depth of 54 inches, in Fallow Land, and in Land Cropped with Barley.

Samples collected June 27th and 28th, 1870.

	WATER PER ACRE.		
	Fallow Land.	Barley Land.	Difference.
	Tons.	Tons.	Tons.
According to experimentally determined weights of soil	2875	1951	924
According to assumed average weights of soil	2668	1775	893
Mean	2772	1863	909

On whichever basis the calculation is made, the indication is that there were about 900 tons less water per acre in the soil and subsoil, to the depth of 4 feet 6 inches, where the barley had grown than where the land was fallow. It may be that part of the excess in the uncropped land was due to the shelter from surface evaporation since the last preceding heavy rain, by the laying of soil upon it for a few days, as above referred to. But even supposing a liberal deduction on this account, the evidence would still point to the conclusion that there had been a higher rate of exhalation by the growing crop than 300 parts of water for every 1 part of dry substance fixed; for it may

safely be assumed that the dry matter of the crop at the time of the experiment would be under rather than over 2 tons per acre, which, at the rate of 300 parts to 1, would only account for an exhalation of 600 tons of water per acre. Further, since there was such a great difference in the percentage of moisture in the two cases at the lowest depth taken, it is only reasonable to conclude that the difference extended lower still.

To conclude, in reference to these particular experiments, it is clear that we have in the facts adduced sufficient evidence, and a striking illustration, of the enormous extent to which, in a time of drought, our crops may rely upon the supplies of moisture previously stored up within the soil. At the same time it cannot fail to be recognised how dependent must be the result upon the character of the soil and the subsoil with which the farmer may have to deal.

SUMMARY, AND GENERAL OBSERVATIONS.

Leaving detail, it will be of interest to summarise the results illustrating the difference of effect of the drought of the past year on the different crops, and also to bring together those relating to the amount of water retained by the soils and subsoils of the different fields, under the various conditions as to season, manuring, and cropping.

It has been already said that although the summers of both 1868 and 1870 were seasons of drought, yet, chiefly owing to the facts that the deficiency of rain commenced later, and the temperatures ruled higher in 1868, there was in reality considerable difference in the characters of the periods of growth of the two seasons, and in their consequent effects upon the different crops. To save space, however, we will confine attention here to the effects on the different crops of the more continued drought of 1870.

Table XI. shows the average annual produce obtained, under selected conditions as to manure, of hay, of wheat, and of barley; also the produce of each in 1870, and the deficiency compared with the average. In the case of the hay, the average is taken over 15 years, and in that of wheat and barley over 19 years. For simplicity of comparison, the produce is, for all three crops, given in lbs.; and the figures relating to wheat and barley represent the total produce, corn and straw together—which, of course, more clearly indicates the total amount of vegetable growth, compared with that of the hay, than the records of corn and straw separately would do.

TABLE XI.—Produce of Hay, Wheat, and Barley in 1870 compared with the average.

	Hay; 15 Years.	Total Produce, Corn and Straw.	
		Wheat; 19 Years.	Barley; 19 Years.
Without Manure.			
	lbs.	lbs.	lbs.
Average produce per acre per annum	2391	2398	2453
Produce in 1870	644	2002	1489
Deficiency in 1870	1747	396	964
With Farmyard Manure.			
Average produce per acre per annum	4604*	6016	5856
Produce in 1870	1556	5092	4949
Deficiency in 1870	3048	924	907
With Mixed Mineral Manure and Ammonia-salts.			
Average produce per acre per annum	5794	6267	5786
Produce in 1870	3306	5836	4287
Deficiency in 1870	2488	431	1499

It is remarkable that, notwithstanding the great fluctuation in the amounts of produce of each of the three crops from year to year according to season, and also the difference in the degree in which each will vary from the average in one and the same season, still, when the average is taken over a considerable number of years, hay, wheat, and barley, are seen to yield *without manure* almost identically the same average weight of produce per acre per annum. On this point it should be mentioned that the second crop of grass is never removed from the land, being either consumed on it by sheep having no other food, or mown and left to rot as manure. The deficiency without manure, due to the drought of 1870, is seen to be 1747 lbs. of hay, 964 lbs. of barley (corn and straw), and only 396 lbs. of total produce of wheat. Thus, the deficiency was much the greatest in the hay; there being a reduction in its case by nearly three-fourths, in that

* For the hay crop, farmyard manure was only applied in the first 8 years; but the average produce is taken over the 15 years.

of the barley by scarcely two-fifths, and in that of the wheat by only about one-sixth, compared with average amounts.

For the hay-crop, farmyard manure was only applied during the first 8 years of the 15; but as the average produce was as great over the succeeding 6 years without the manure, as over the first 8 years with it, and as there was a heavier crop in 1869 than in any of the preceding 13 years, the deficiency in 1870 compared with the average, may be taken as at any rate mainly due to the drought, and but little to the cessation of the manuring. The figures as they stand show, as without manure, again, a much greater deficiency than in either wheat or barley; the crop amounting in fact to only one-third the average. Of total produce of wheat and barley, there is, with farmyard manure, again nearly the same average amount over 19 years in the two cases. The deficiency in 1870 compared with the average is also very nearly the same with the autumn-sown wheat and the spring-sown barley; amounting in each case to scarcely one-sixth. In the wheat the reduction is actually much greater, but in proportion to the average, only about the same as without manure; but in the barley it is actually less, though in proportion to the average very much less, than without manure. The greater power of retention of water which a dunged soil has been shown to possess in its upper layers, has doubtless much to do with the result.

With the artificial mixture, in the case of the hay and the wheat supplying 400 lbs., but in that of the barley only 200 lbs. of ammonia-salts per acre per annum, there is not the same uniformity in the average annual produce of the three crops; the wheat giving nearly 500 lbs. more gross produce than the hay with the same amount of ammonia applied, and the barley about the same as the hay, with only half the supply of ammonia-salts. The deficiency in 1870 amounts, in the hay to more than two-fifths, in the barley to rather more than one-fourth, and in the wheat to little more than one-fifteenth, compared with the average.

Thus, then, with a drought extending over the months of April, May, June, and July, the mixed herbage of permanent meadow land suffered, under the different conditions of manure in question, very much more than either wheat or barley; and the spring-sown barley suffered, both without manure and with the artificial manure, very much more than the autumn-sown wheat. With the farmyard manure, however, the barley would appear to have been as little adversely affected by the deficiency of rain during the period of actual growth as the wheat. We need not here again refer to the special conditions already explained, under which the hay crop was as little, or less, affected by the drought than the other crops.

The difference between the conditions of growth of the chiefly perennial (or biennial) plants composing the complex mixed herbage of permanent meadow land, and those of an annual, like wheat or barley, sown at a stated period of the year in arable land, and having a fixed, and in the case of barley only a limited time for distributing its underground feeders, and so availing itself of the resources of nutriment and moisture within the soil, are obviously very great.

The perennial, or biennial, character of most of the plants composing the mixed herbage, would seem at first sight to give the grass a great advantage over the corn crops. But observation shows, that although the immediately superficial layers of the soil may be more thoroughly penetrated by the roots of the perennial grasses than by those of either wheat or barley, yet it is only a very few of the former, encouraged to great predominance only under special conditions, that seem to get anything like the same possession of the lower layers of the soil as the two corn crops. Careful examination has also shown, and it is probably generally assumed, that the winter-sown wheat secures possession by its underground feeders of a more extended range and greater bulk of soil, and consequently is better able to avail itself of the supplies of food and moisture existing below a certain limited depth from the surface, than the spring-sown barley. The wheat-plant, indeed, has the advantage of making root, more or less according to season and manure, throughout the winter months, during periods of which, at any rate, the soil will be saturated with moisture; and in the case of moderately retentive and well drained soils, it will be able to establish its independence of rain falling during the period of active above-ground growth, very much more than will a spring-sown crop like barley.

But there are other points of distinction between the growth of the corn and the hay crops. Thus, most of the grasses, which comprise the greater proportion of the latter, flower earlier than the wheat or the barley; and the mixed herbage is cut by, or before, the end of June, when very little, if any of it, has arrived at the degree of ripeness in which the corn crops are cut. These, on the other hand, are not only allowed fully to ripen, but direct experiments made at Rothamsted upon wheat have shown that a very large proportion, probably about half, of the total dry vegetable substance, or of the total carbon of the crop, is fixed in it under the influence of the greater power of the sun's rays after the time at which the hay crop is usually cut.

These facts are obviously an element in the explanation of another fact, to a certain extent commonly recognised, and which a careful comparison of the results of the field experiments at

Rothamsted, with the records of the conditions of heat and moisture under which the crops have been grown, brings clearly to view—namely, that, as compared with the hay crop, the corn crops are not only less dependent on the amounts of rain falling during the period of active vegetation, but more on a relatively high degree of temperature during that period. This is more strikingly the case when wheat is grown by means of readily soluble mineral and nitrogenous manures, than when it is grown without manure, or with farmyard manure. Without manure the produce is comparatively more dependent on the amount of certain constituents brought down by the rain, or rendered available by its means from the stores of the soil itself; and it would seem that where farmyard manure is employed, a considerable amount of rain is required during the early growing period to aid its decomposition, and so to set free, distribute, and render available, its fertilising constituents. In the case of the artificial manures, on the other hand, some of the most active fertilising constituents are supplied in a much more soluble form, and require a less amount and continuity of rain for their solution and distribution throughout the pores of the soil within a given range.

It is seen, then, that several reasons concur to render corn crops less dependent on the fluctuations in the amount of rain falling during the period of active vegetation and accumulation of substance than is the hay crop growing under otherwise parallel conditions as to soil and manure. It is quite intelligible, too, that the autumn-sown wheat, with its much longer time for the formation and distribution of root, and its tendency to develop proportionally more in the lower and proportionally less in the upper layers of the soil, than the spring-sown barley, should be less adversely affected than the latter by a deficiency of rain during the period of active above-ground growth.

Table XII. brings together at one view the percentage amounts of water retained by the soils and subsoils of the different fields, under the various conditions as to season, cropping, &c. The results so summarised relate to samples collected as under:—

1. From the experimental wheat field, just before harvest, 1868; mean of three plots differently manured.

2. From the experimental wheat field, in January, 1869, when the land was supposed to be saturated; mean of the same three plots differently manured.

3. From uncropped land, near the end of June, 1870.

4. From land cropped with barley, closely adjoining the uncropped land; samples collected at the same date, end of June, 1870.

5. From permanent meadow land, in July, 1870, after the removal of the crop; mean of three plots differently manured.

TABLE XII.—Summary of Percentages of Moisture in Soils and Subsoils from different Fields, and under different conditions as to Season, Cropping, &c.

Depths of Samples.	EXPERIMENTAL WHEAT FIELD.		BARN FIELD. Samples collected, June 27th and 28th, 1870.		PERMANENT MEADOW LAND.
	Samples collected, July, 1868; Mean of Plots 3, 2, and 8a.	Samples collected, Jan. 6th and 7th, 1869; Mean of Plots 3, 2, and 8a.	Uncropped Land.	Land Growing Barley.	Samples collected, July 25th and 26th, 1870; Mean of Plots 3, 9, and 14.
First 9 ins.	6·23	27·17	20·36	11·91	11·99
Second 9 ,,	11·19	22·70	29·53	19·32	11·77
Third 9 ,,	15·02	25·27	34·84	22·83	17·11
Fourth 9 ,,	16·13	25·65	34·32	25·09	19·32
Mean 36 ,,	12·14	25·19	29·76	19·79	15·05
Fifth 9 ,,	..	..	31·31	26·98	20·67
Sixth 9 ,,	..	..	33·55	26·38	21·49
Mean 54 ,,	..	..	30·65	22·09	17·06

The special application of the detailed results having been already fully considered, attention must be confined here to the more general indications only of the foregoing summary.

In the first place, it should be observed that all three fields have a subsoil of reddish yellow clay, resting upon chalk, at a varying depth, but of not many feet from the surface. All, therefore, have good natural drainage; and it is very seldom that any water collects in the furrows, and then only for a very few hours. The experimental wheat field is, however, pipe-drained at a depth of about 30 inches, and at a distance of about 25 feet from drain to drain.

It is of interest to observe that there is no wide difference in the amount of water retained at corresponding depths in the experimental wheat-field in July 1868, when the crop was nearly at maturity, and in the permanent meadow land in July 1870, after the removal of the hay crop. The percentages are, however, rather lower in the drained land; which, at the time, had probably supported a higher average amount of produce also.

Towards the end of June 1870, the undrained arable land, which then carried a crop of growing barley, representing perhaps from $1\frac{1}{2}$ to 2 tons of dry substance fixed, retained only about the same amount of water near the surface as the meadow land in July 1868; but, lower down, it held considerably more than either the drained wheat land in July 1868, or the undrained meadow land in July 1870.

It is remarkable that the uncropped and undrained land, though retaining much less water within 9 inches from the surface, from that point downwards retained, in June 1870, considerably more at every stage than the drained wheat soil in January 1869, when the drains were running, and the land was supposed to be saturated. From this comparison, it is obvious that no safe conclusion can be drawn from the percentage of water in the subsoil of the uncropped but undrained land, as to the probable amount retained by the subsoil of the drained land at the commencement of active vegetation in the spring. The amount retained in the subsoil of the uncropped and undrained land is indeed enormous; but the comparison of it with that in the adjoining cropped land shows clearly enough that it was readily available for the purposes of vegetation. In reference to this latter point, the fact of the good natural drainage by the chalk must not be overlooked.

There is, upon the whole, general consistency in the results brought together in Table XII. It may, perhaps, safely be concluded that, notwithstanding the natural drainage by the chalk, the pipe-drains had contributed to reduce the percentage of moisture retained by the subsoil of the experimental wheat field, to the depth examined; but that they had, at the same time, rendered the clay more permeable by roots, and the water that was retained more readily available. The evidence is, at any rate, very striking as to the degree in which, in a time of drought, our crops are enabled to rely upon the water previously accumulated within the subsoil—provided the latter be of sufficient depth, of sufficient retentive power, and at the same time sufficiently permeable.

Before concluding, it will be well to call attention to a very important bearing of some of the results adduced. Assuming, as we may be allowed to do for the sake of illustration, that a good crop of hay, wheat, or barley, will probably exhale not less, and perhaps more, than 700 tons of water per acre during growth, we still have only about 7 inches of rain, out of an average annual fall of say 25 inches, thus directly disposed of by the growing crop; and, taking the amount retained by the soil itself as practically a constant quantity from year to year, there remains to be disposed of by evaporation from the surface, and by passage into the drains or otherwise beyond the reach of the roots of the crop, an average of about 18 inches of rain annually, equivalent to more than 1800 tons of water per acre.

How much of this large quantity of water passes off by evaporation from the surface of the soil itself, inducing by capillary action the withdrawal of water, carrying with it, it may be, essential plant-food, from the lower to the upper layers of the soil?—or, how much passes downwards, carrying in solution any manurial matters in excess of the quantity which can be absorbed and retained within the pores of the soil and the upper layers of the subsoil?

These questions cannot be so satisfactorily answered in regard even to any particular soil, or season, as is desirable; and could they be so, the answers would vary greatly with variations of soil and season. As already stated, direct experiments are now in progress at Rothamsted with the view of acquiring useful data on this subject. With regard to the results hitherto obtained, it may be remarked that, from September 1st to December 31st, 1870, that is, commencing after the unusual drought of the preceding summer, it was found that, out of a rainfall of about 10·5 inches within the same period, about 50 per cent. had passed below a depth of 20 inches, about 40 per cent. below 40 inches, and about 20 per cent. below 60 inches from the surface. Calculation further showed that, even supposing there were some accumulation during August, still, a very large proportion of that which did not so pass, would be required to bring the previously very dry soil to the point of saturation—judging this requirement from the results which have been already given bearing upon the point. That is to say, as would be expected, a comparatively small proportion of the rainfall was evaporated at that season of the year. Much more would, of course, so disappear taking the whole year round; the quantity varying considerably with the characters of the soil and the season.

Towards the end of the last century, Dr. Dalton * devised an apparatus for the determination of the proportion of the rainfall which passed off from the soil by drainage, and by evaporation, respectively. It consisted of a cylinder, 10 inches in diameter, 3 feet deep, open at the top, and closed at the bottom; but having one small exit tube near the top, and another near the bottom, for the escape of water into bottles placed to receive it. The vessel was filled with earth, and sunk into the ground level with the surface, one side being left exposed for access to the bottles. He continued the experiment for three years, 1796-7-8, and found the drainage to average, over that period, 25 per cent., and the evaporation to be, therefore, equal to 75 per cent. of the rainfall. This was exclusive of any evaporation of dew, but inclusive of that resulting from vegetation, as the surface of the soil became, after the first year, covered with grass; a circumstance which, however, Dr. Dalton considered immaterial.

For eight years, 1836-1843, Mr. Dickinson, of Abbott's Hill, King's Langley, Herts,† experimented with a modification of

* Mem. Lit. Phil. Soc. of Manchester, vol. v., part 2.

† 'Journal of the Royal Agricultural Society,' vol. v.

Dalton's apparatus. The cylinder he employed was 12 inches in diameter, and 3 feet deep, but provided at that depth with a perforated bottom, and a receptacle beneath for the collection of the water; and there was an arrangement of tubes for the escape, and measurement, of the drainage water. Grass was grown on the surface of the soil in the cylinder. The drainage would doubtless be more free in the experiments of Mr. Dickinson than in those of Dr. Dalton; and the results, over 8 years, showed, with a less rainfall, a larger actual amount of drainage; the latter representing 42½ per cent., and the evaporation, therefore, only 57½ per cent. of the rainfall. This amount included, of course, the exhalation due to vegetable growth.

From results obtained by gauging the flow of water from pipe-drains, it has been concluded that a still larger proportion of the rainfall passes off by evaporation than that indicated by the experiments of either Mr. Dickinson or Dr. Dalton. But results obtained by deducting the amount passing through drains from the total rainfall may be judged to be quite untrustworthy, from the fact that, before the pipe-drains in the experimental wheat field had passed any water at all in the autumn of last year, the *drain-gauges* already referred to had indicated that, of the rain which had then fallen since the 1st of September, nearly 25 per cent. had passed below 20 inches, nearly 10 per cent. below 40 inches, and nearly 4 per cent. below 60 inches from the surface. It is clear, therefore, that the amount of water passing through artificial drains may be no measure whatever of the total quantity passing below the reach of the roots of growing crops.*

In the admitted defect of satisfactory evidence from which may be deduced the probable average amount of evaporation from the surface of the soil independently of vegetation, we will assume, by way of illustration, that, taking the average of many

* It was not until after this Paper went to press for the *Journal*, that we recollected the experiments of Maurice, Gasparin, and Risler, relating to this subject, and therefore make brief reference to them here. M. Maurice, experimenting at Geneva, over two years, 1796 and 1797, sought to measure the evaporation from the soil by means of a cylindrical iron vessel filled with earth, the changes in the weight of which he determined daily during that period. His results indicated an amount of evaporation corresponding to about 61 per cent. of the rainfall, which latter averaged over the two years about 26 inches per annum. ('Bibl. Britan. de Genève: Sciences et Arts,' t. i.) M. Gasparin, experimenting in a somewhat similar way at Orange, in the south of France, found the evaporation so determined to amount, in the two years 1821 and 1822, to about 80 per cent. of a rainfall of about 28 inches per annum. ('Cours d'Agriculture,' t. ii. p. 116.) M. Risler, again, at Calèves, near Nyon, Switzerland, by gauging drains 1·2 metre (about 4 feet) deep, in a very compact and impervious subsoil, estimated the evaporation over the two years, 1867 and 1868, to amount to about 70 per cent. of a rainfall which averaged over the period about 41 inches per annum. The land was cropped, as usual, during the period of the experiment; so that the amount of evaporation indicated includes that due to vegetable growth. ('Archives des Sciences de la Bibliothèque Universelle,' Sept., 1869.)

soils and seasons, three-fourths of a total rainfall of 25 inches will pass off by the combined action of evaporation from the surface of the soil itself, and of the exhalation due to the growth of a good crop of hay or corn. On this supposition there would still remain more than 6 inches of rain, equivalent to more than 600 tons of water per acre, annually passing downwards, and carrying with it more or less of fertilising matters.

Fortunately, some of the most important mineral constituents of soils and manures are, in the case of the heavier soils at any rate, almost wholly retained by them within the range of the roots of our crops. Nitrogen, whether supplied in the form of ammonia-salts or nitrates is, however, much less completely so retained; being, in whichever state supplied, carried off in greater or less quantity in the drainage water, chiefly in the form of nitrates. According to results obtained independently by Professor Frankland and Professor Voelcker, on the analysis of drainage water from the experimental wheat-field at Rothamsted, that collected during the winter, from land manured in the autumn by an amount of ammonia-salts supplying 82 lbs. of nitrogen per acre, may contain from 2·5 to 3 parts, or even more, of nitrogen, as nitrates and nitrites, per 100,000 parts of water. Assuming that only 2·5 parts of nitrogen were so carried beyond the reach of roots for every 100,000 parts of water passing downwards, there would still be, for every inch of rain so passing, a loss per acre of between 5 and 6 lbs. of nitrogen, supplied in manure at a cost of not much less than 1*s.* per lb.

The above estimate of quantity must be understood to be adopted only provisionally, and by way of illustration. It is, however, a sufficiently near approximation to what must happen in the case of many soils and seasons at any rate, to show the very great importance of further investigating the reactions of various descriptions of nitrogenous manure on different descriptions of soil, and of determining the best modes, and the best periods of the year, for the application of such manurial matters, so as to reduce the loss by drainage to a minimum. This subject is now receiving attention at Rothamsted.

Rothamsted, January, 1871.

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ROTHAMST

MAY,

AND PREVIOUS CROP

(16 Years, 1856-

...ing and Manuring.		Crop, &c., Present Season, 1870-71.	Acres.	Name of Field.
1864.				
Wheat, 2 cwts. Guano.	...o.	Barley, 3 cwts. superphosphate, 2½ cwts. Nitrate Soda.	20	Barn.
		Barley,		

ROTHAMSTED FARM.

MAY, 1871.

SUMMARY STATEMENT OF THE PRESENT AND PREVIOUS CROPPING, &c., OF THE ARABLE LAND NOT UNDER EXPERIMENT.

(16 Years, 1856-71, inclusive.)

Name of Field.	Acres.	Previous Cropping and Manuring. 1856.	1857.	1858.	1859.	1860.	1861.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	Crop, &c., Present Season, 1870-71.	Acres.	Name of Field.
Barn	20	Turnips, Artificial.	Wheat, Artificial.	Oats, Artificial.	Red Clover (perm.), Unmanured.	Wheat, after Sheep-Folding.	Swedes, Dung & Artificial.	Oats, 2 cwts. Guano.	Red Clover (perm.), Unmanured.	Wheat, 2 cwts. Guano.	Wheat, 2 cwts. Guano, 1 cwt. Nitrate Soda, 1½ cwt. Corn Manure.	Oats, 2 cwts. Guano, 1 cwt. Sulph. Ammonia.	Oats, 2 cwts. Guano, 1 cwt. Sulph. Ammonia.	Mangolds, Dung & 3 cwts. Guano. Swedes, 2 cwts. Guano, and 2½ cwts. superphos.	Wheat, Unmanured—after Mangolds carted off, Swedes ploughed up, and Fallow.	Barley, 2 cwts. Guano.	Barley, 3 cwts. superphosphate, 2½ cwts. Nitrate Soda.	20	Barn.
Thirty Acres	30	Oats, Artificial.	Red Clover (perm.), Unmanured.	Wheat, after Sheep-Folding.	Oats, Artificial.	Swedes, Dung & Artificial.	Oats, after Sheep-Folding.	Red Clover (perm.), Unmanured.	Wheat, Sheep-Folded, and 2 cwts. Guano.	Oats, 2 cwts. Guano, 1 cwt. Corn Manure.	Oats, 1 cwt. Guano, 3 cwts. Corn Manure.	Tares and Swedes, Dung and Artificial.	Oats, after Sheep-Folding.	Clover.	Wheat, 2 cwts. Guano.	Oats, 2 cwts. Guano.	Barley, 2 cwts. superphosphate, 2 cwts. Nitrate Soda.	30	Thirty Acres.
Upper Harpenden	14	Turnips, Artificial.	Barley, after Sheep-Folding.	Beans, Dung.	Wheat, Artificial.	Barley, Artificial.	Swedes, Dung & Artificial.	Oats, after Sheep-Folding.	Red Clover, Unmanured.	Wheat, 1½ cwt. Guano, 1½ cwt. Corn Manure.	Oats, 1 cwt. Guano, 2 cwts. Corn Manure.	Oats, 2 cwts. Guano, 1 cwt. Sulph. Ammonia.	Tares, Dung. Swedes, Artificial.	Wheat, ⅚ths. 2½ cwts. Guano, ⅙th. Sheep-folded.	Oats, 2 cwts. Guano, 1 cwt. dried Blood, ½ cwt. Sulph. Ammonia.	Swedes, Dung and superphosphate.	Wheat, 2 cwts. Guano.	14	Upper Harpenden.
Harpenden	22	Red Clover (perm.), Unmanured.	Wheat, Artificial.	Oats, Artificial.	Swedes, Dung & Artificial.	Oats, after Sheep-Folding.	Red Clover (perm.), Unmanured.	Wheat, 2 cwts. Guano.	Oats, 3 cwts. Guano.	Mangolds and Turnips, Dung and Artificial.	Wheat, Sheep-Folded.	Red Clover, (perm.), Unmanured.	Wheat, 2½ cwts. Guano.	Oats, ⅔rds {2 cwts. Guano, & 1 cwt. Nitr. Soda. ⅓rd {1 cwt. Nitr. Soda and Sheep-folded.	Swedes, Dung and various Artificial Manures.	Wheat, 3 cwts. Guano.	Oats, 3 cwts. Guano, 1 cwt. Nitrate Soda. Tares, Dung.	22	Harpenden.
Little Hoos	9	Oats, Artificial.	Turnips, Artificial.	Wheat, after Sheep-Folding.	Oats, Artificial.	Mangolds, Dung & Artificial.	Oats, Unmanured.	Barley, 2 cwts. Guano, 1 cwt. superphos.	Barley, 3 cwts. Guano, 1 cwt. superphos.	Red Clover.	Wheat, 1½ cwt. Guano, 1 cwt. Nitrate Soda, 1 cwt. Corn Manure.	Mangolds, Dung and Artificial.	Wheat, Unmanured.	Oats, 2 cwts. Guano, 1 cwt. Nitrate of Soda.	Barley, 1 cwt. dried Blood, ½ cwt. Sulph. Ammonia, 1 cwt. superphosphate.	Barley, 2½ cwts. Guano.	Barley, 3 cwts. superphosphate, 2½ cwts. Nitrate Soda.	9	Little Hoos.
Fosters' ..	18	Wheat, Artificial.	Barley, Artificial.	Swedes, Artificial.	Barley, after Sheep-Folding.	Red Clover (perm.), Unmanured.	Wheat, Artificial.	Oats, 3 cwts. Guano.	Barley, 5½ cwts. Artificial Manure.	Swedes, Dung and Artificial.	Oats, 1 cwt. Guano, 1 cwt. Corn Manure.	Red Clover, Unmanured.	Wheat, 2 cwts. Guano, ½ cwt. Corn Manure.	Oats, 2 cwts. Guano, 1 cwt. Nitrate of Soda.	Barley, 1 cwt. dried Blood, ½ cwt. Sulph. Ammonia, 1 cwt. superphosphate.	Oats, 2 cwts. Guano, 3 cwts. Blood Manure.	Roots, Tares, and Rape, Dung and Artificial.	18	Fosters'.
Knott Wood	30	Oats, Artificial.	Swedes, Dung & Artificial.	Barley, after Sheep-Folding.	Red Clover, (perm.), Unmanured.	Wheat, Artificial.	Oats, Artificial.	Swedes, Dung and Artificial.	Oats, Sheep-Folded.	Red Clover, (perm.)	Wheat, Sheep-Folded, 1 cwt. Guano.	Oats, 2 cwts. Guano, 1 cwt. Sulph. Ammonia.	Oats, 2 cwts. Guano, 1 cwt. Sulph. Ammonia.	Swedes, 2 cwts. Guano, 2½ cwts. superphosphate and Dung.	Wheat, 3 cwts. Guano (one-half), Unmanured (one-half), after Swedes ploughed up and Fallowed.	Oats, 2 cwts. Guano.	Oats, 3 cwts. Guano, 1 cwt. Nitrate Soda.	30	Knott Wood
Little Knott Wood ..	14	Wheat, Artificial.	Oats, Artificial.	Swedes, Dung & Artificial.	Oats, after Sheep-Folding.	Red Clover (perm.), Unmanured.	Wheat, after Sheep-Folding.	Oats, 3 cwts. Guano.	Swedes, Dung and Artificial.	Wheat, Unmanured.	Red Clover, (perm.), Unmanured.	Red Clover, (perm.), Sheep-Folded.	Wheat, 1 cwt. Guano, ½ cwt. Corn Manure.	Oats, 2 cwts. Guano, 1 cwt. Nitrate Soda.	Mangolds, 12 tons Dung, 2 cwts. Guano.	Wheat, 3 cwts. Guano.	Oats, 3 cwts. Guano, 1 cwt. Nitrate Soda.	14	Little Knott Wood.
Sawpit ..	14	Red Clover (perm.), Unmanured.	Wheat, Artificial.	Oats, Artificial.	Mangolds, Dung & Artificial.	Oats, Unmanured.	White Clover, Unmanured.	Wheat, 2 cwts. Guano.	Tares and Oats, Sheep-Folded, and 2 cwts. Guano.	Barley, 1½ cwt. Guano, ½ cwt. superphos., 1 cwt. Corn Manure.	Mangolds and Turnips, Dung and Artificial.	Wheat, Unmanured.	Red Clover, Unmanured.	Wheat, 1 cwt. Guano, 1 cwt. Wheat Manure.	Wheat, 3 cwts. Guano.	Mangolds, Dung and 3 cwts. Guano.	Wheat, 3 cwts. Guano.	14	Sawpit.
Rick-yard	8	Oats, Artificial.	Mangolds, Dung & Artificial.	Wheat, Unmanured.	Oats, Artificial.	Tares, Dung.	Oats, Unmanured.	Mangolds, Dung and Artificial.	Wheat, Unmanured.	Wheat, Sheep-Folded, and 3 cwts. Guano.	Barley, 2 cwts. Guano, 1½ cwt. Corn Manure.	Red Clover, Sheep-Folded.	Wheat, Guano.	Barley, 2 cwts. Wheat Manure.	Tares, Dung.	Barley, 1 cwt. Guano.	Mangolds, Dung and 4 cwts. Cotton Cake.	8	Rick-yard.
Six Acres ..	6	Barley, after Sheep-Folding.	Trefoil, Unmanured.	Wheat, after Sheep-Folding.	Barley, Artificial.	Beans, Dung.	Wheat, Unmanured.	Oats, 3 cwts. Guano.	Mangolds, Dung and Artificial.	Wheat, Unmanured.	Red Clover, Unmanured.	Wheat, 2 cwts. Guano, 2 cwts. Corn Manure.	Oats, 3 cwts. Guano.	Beans, Dung.	Wheat, 2 cwts. Guano, 1 cwt. Nitrate of Soda.	Barley, 2½ cwts. Guano.	Barley, 3 cwts. superphosphate, 2½ cwts. Nitrate Soda.	6	Six Acres.
Clay-Croft	5	Oats, Artificial.	Beans, Dung.	Wheat, Artificial.	Oats, Artificial.	Red Clover (perm.), Unmanured.	Wheat, Artificial.	Beans, Dung and Fallow.	Wheat, Dung.	Wheat, 2 cwts. Guano, 2 cwts. Corn Manure.	Oats, 2 cwts. Guano, 2 cwts. Corn Manure.	Oats, 2 cwts. Guano, 1 cwt. Sulph. Ammonia.	Beans, Dung.	Wheat, 2 cwts. Guano.	Oats, 2 cwts. Guano, 1 cwt. dried Blood, ½ cwt. Sulph. Ammonia.	Turnips, Dung and 3 cwts. superphosphate.	Wheat, Unmanured.	5	Clay-Croft.
Apple Tree	18	Swedes, Dung & Artificial.	Oats, after Sheep-Folding.	Red Clover (perm.) Unmanured.	Wheat, Artificial.	Oats, Artificial.	Mangolds, Dung & Artificial.	Wheat, Unmanured.	Laid down in Grass Seeds.	Grass.	Grass.	Grass.	Grass.	Grass, 1 cwt. Guano, 1 cwt. Nitrate Soda.	Grass, (Mangolds fed off by Sheep).	Grass, Dung and Clay.	Grass, 1 cwt. Nitrate Soda.	18	Apple Tree.
Ten Acres ..	10	Barley, Artificial.	Tares, Dung.	Oats, Unmanured.	Tares, Dung.	Oats, Artificial.	Red Clover, (perm.) Unmanured.	Wheat, after Sheep-Folding.	Oats, 3 cwts. Guano.	Oats, 2 cwts. Guano, 1 cwt. Dried Blood.	Tares, Dung.	Turnips, Artificial.	Wheat, Guano.	Red Clover.	Wheat, 2 cwts. Guano.	Oats, 3 cwts. Guano.	Mangolds, Dung and 4 cwts. Cotton Cake.	10	Ten Acres.
Park Field	10	Wheat, Artificial.	Red Clover, Unmanured.	Wheat, after Sheep-Folding.	Wheat, Artificial.	Oats, Artificial.	Red Clover, (perm.), Unmanured.	Wheat, 3 cwts. Guano.	Oats, 3 cwts. Guano.	Tares, Dung.	Barley, Sheep-Folded.	Barley, 1 cwt. Guano, 1½ cwt. Corn Manure.	Swedes.	Wheat, 1 cwt. Guano.	Oats, 2 cwts. Guano, 1 cwt. dried Blood, ½ cwt. Sulph. Ammonia.	Tares, One-half, Dung; one-half, sheep folded.	Barley, 3 cwts. superphosphate, 2½ cwts. Nitrate Soda.	10	Park Field.
Agdell ..	9	Barley, Artificial.	Tares, Dung.	Oats, Unmanured.	Barley, Artificial.	(Garden-ground.)	Oats, Unmanured.	Tares, Dung.	Barley, Sheep-Folded.	Barley, 1½ cwt. Guano, ½ cwt. superphos., 1 cwt. Corn Manure.	Red Clover, Unmanured.	Wheat, 1½ cwt. Guano, 1½ cwt. Corn Manure.	Oats, 2 cwts. Guano.	Tares, Dung.	Barley, Unmanured.	Barley, 1½ cwt. Guano, 1½ cwt. superphosphate.	Mangolds, Dung and 4 cwts. Cotton Cake.	9	Agdell.
Long Hoos	25								Fallow.	Swedes, Dung and Artificial.	Barley, 1 cwt. Guano, 1 cwt. Corn Manure.	Barley, 1½ cwt. Guano, 1 cwt. Corn Manure.	Mangolds and Swedes, 15 tons Dung, 3 cwts. Guano.	Wheat, 1 cwt. Guano.	Oats, 2 cwts. Guano, 1 cwt. dried Blood, ½ cwt. Sulph. Ammonia.	Sainfoin, Unmanured.	Sainfoin, Unmanured.	25	Long Hoos.
Sawyers' ..	25								Swedes and Fallow, Artificial.	Barley, 1 cwt. Guano, 1 cwt. Corn Manure.	Swedes, Dung and Artificial.	Wheat and Barley, Sheep-Folded.	Red Clover, Unmanured.	Wheat, 3 cwts. Guano.	Fallow.	Wheat, 4 cwts. Guano.	Wheat, 4 cwts. Guano, 1 cwt. Nitrate Soda.	25	Sawyers'.
Barn Field	34								Swedes, Dung and Artificial.	Oats, 1½ cwt. Guano, 1½ cwt. Corn Manure.	Red Clover (perm.), Sheep-Folded.	Wheat, 1½ cwt. Guano, 1½ cwt. Corn Manure.	Barley, 1 cwt. Blood Manure, 1 cwt. superphosphate, 1 cwt. Sulph. Ammonia.	Fallow.	Wheat, 3 cwts. Guano.	Sainfoin, Unmanured.	Sainfoin, Unmanured.	34	Barn Field.

Ueber die

antipyretische Wirkung

von

Chinin und Alkohol.

Von

C. Binz,
a. o. Professor in Bonn.

(Separatabdruck aus Virchow's Archiv für pathologische Anatomie und Physiologie und für klinische Medicin. Einundfünfzigster Band.)

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I.

Humorale Beziehungen des Chinin. — Prüfung desselben in dem durch Rückenmarkstrennung erzeugten Fieber. — Ueber die Resorption des Chinin beim fiebernden Menschen.

Zur Erörterung der Frage, von welchen Systemen oder einzelnen Theilen des thierischen Organismus aus die temperaturerniedrigende Wirkung gewisser Arzneistoffe sich geltend mache, unternahm ich in Gemeinschaft mit Herrn C. Bouvier die folgende Versuchsreihe [1]).

Von den zahlreichen Antipyreticis lag am nächsten das Chinin. Es ist nur wenig giftig, bietet die sicherste therapeutische Wirkung dar, und ist am leichtesten zu handhaben. Ich habe früher zu zeigen gesucht, dass ihm ein directer Einfluss auf die thierischen Säfte zukommt, und ging dabei von den Beziehungen aus, die ihm ausserhalb des Kreislaufes gegenüber Fermentkörpern und organischen Stoffen eigen sind.

Das chlorwasserstoffsaure Chinin, ebenso wie jedes andere in einem gewissen Grad ohne Säurezusatz lösliche Chininsalz, verhin-

[1]) Die Herren Dr. Kemmerich und Cand. med. Gützloe unterstützten uns mehrfach durch ihre Beihülfe.

dert mit einer für neutrale Verbindungen sonst unübertroffenen Energie Zerlegungsvorgänge der verschiedensten Art [1]). Besonders einige wichtige Oxydationsprozesse werden durch seinen Einfluss schon bei Anwesenheit kleiner Quantitäten deutlichst herabgesetzt: die gewöhnliche Fäulniss proteinhaltiger Gewebe und Flüssigkeiten sowie die Ozonbildung in ebensolchen flüssigen Gemengen.

Auch am lebenden Thier zeigt sich die nehmliche Einwirkung. Die in ihm durch Injection fauliger Stoffe erregte Septicämie ist ein Fäulnissvorgang in modificirter Form. Das beweisen allein die schon im Leben beginnenden Zersetzungssymptome und die deutlich ausgesprochene Tendenz zum putriden Zerfall vieler Gewebe unmittelbar nach dem Tode. Das Chinin ist unter Umständen befähigt, die ganze Reihe der Erscheinungen entweder niederzuhalten oder doch auf ein geringeres Maass zurückzuführen. Es wirkt hier als inneres Antisepticum, so wie sonst als äusseres. Und dem analog gestalten sich seine Beziehungen zur Ozonbildung durch organische Substanz. A. Schmidt hat eine Methode angegeben, den erregten Sauerstoff im frisch entleerten Blut nachzuweisen. In Verbindung mit anderen Thatsachen und Betrachtungen hat sie wohl allgemein zu der Ansicht geführt, auch im kreisenden Blut befinde sich der Sauerstoff zum Theil in jenem wirksameren Zustand [2]). Es gelingt nun mit schon kleinen Mengen Chinin sehr leicht, die Ozonbildung in vege-

[1]) In dem Sitzungsbericht der Kaiserlichen Akademie Bd. 59. S. 817 findet sich eine vorläufige Notiz „über den Ursprung und die Vermehrung der Bacterien" von A. Polotebnow, die mit folgenden Worten schliesst: „Auch concentrirte Chininlösungen, weit entfernt die Entwickelung der Vibrionen aus den Sporen (von Penicillium glaucum) zu beeinträchtigen, wie gewöhnlich angenommen wird, begünstigen sogar dieselben in hohem Grade." Ein näheres Urtheil über diesen Theil der P.'schen Versuche ist noch nicht möglich. Was ich selbst über die Behinderung der Fäulniss und verwandter Prozesse durch Chinin früher veröffentlicht habe, darf ich in allen wesentlichen Punkten nochmals als zutreffend erklären.

[2]) Bd. XLVI. S. 148 dieses Archivs wurde bereits hervorgehoben, dass es mir beim Verhindern der Guajacbläuung weniger auf die Hemmung eines Auftretens von Ozon im Sinne Schönbein's als auf die eines deutlichen und raschen Oxydationsvorganges ankomme. Letzterer lag in den betreffenden Versuchen unzweifelhaft vor. Es bleibt sich demnach gleich, ob man dabei an „Ozon" oder an Sauerstoff im Status nascens oder an sonstwie abgetrennte und darum activere O-Atome denkt. In diesem Sinn wird auch gegen den abwechselnden Gebrauch beider Ausdrücke nichts einzuwenden sein.

tabilischen Auszügen zu vermindern. Die Reaction am Thierblut zeigt das gleiche Verhalten, und zwar nicht nur wenn das Chinin dem frisch gelassenen Blut zugesetzt wird, sondern auch wenn es zuvor dem Kreislauf einverleibt worden war.

Nach Klebs ist Eiter von jeder Beschaffenheit ozonerzeugend [1]). Für die Behinderung dieser Eigenschaft gilt das Nehmliche wie für Pflanzenauszüge und entleertes Thierblut. Man bringe etwa 1 Gramm guten Eiter in ein Proberöhrchen, füge 10 Theile Wasser hinzu, schüttele und versetze mit einer geringen Menge frischer Guajactinctur, so wird man sofort die Bestätigung der erwähnten Angabe gewahren. Um die Chinineinwirkung zu sehen, versetze man eine gleichwerthige Eitermenge statt mit Wasser allein noch mit etwa 10 Tropfen der einprocentigen Lösung eines neutralen Chininsalzes und lasse es zugleich mit dem ersten Präparat an der Luft einige Augenblicke stehen. Der Zusatz des Guajacharzes ruft eine deutlich verminderte, nur schmutzig grüne Reaction hervor, die wenn selbst vielleicht im Anfang der normalen Reaction ähnlich oder gleich, jedenfalls viel eher erblasst und präcipitirt als die der Controle. Zur Erleichterung des Anstellens dieses Versuches sei noch erwähnt, dass auch das purulente Nasalsecret des gewöhnlichen Schnupfens sich zur Guajactinctur wie parenchymatöser Eiter verhält.

Nach den Untersuchungen von G. Harley [2]), von denen zu erwähnen blieb, dass jenes von mir citirte Experiment nur eins von mehreren ist, die mit dem nehmlichen Erfolg angestellt wurden, verringern schon sehr kleine Quantitäten Chinin die Fähigkeit gelassenen Blutes, aus der Luft Sauerstoff aufzunehmen und Kohlensäure zu bilden. Bei Gelegenheit der Prüfung des Dihydroxylchinin in seinem physiologischen Verhalten hat G. Kerner auch diese für die Auffassung von der Chininwirkung wichtige Angabe in allem Wesentlichen bestätigt gefunden [3]).

Hiermit übereinstimmende Resultate hat Zuntz erhalten. Die von ihm früher beschriebene energische Säurebildung in frischem Blut, welche sich unter dem Einfluss der Luft und der Mitwirkung der rothen Blutkörperchen vollzieht, wird durch den Zusatz schon

[1]) Centralbl. f. d. medic. Wissenschaften 1868. S. 406 u. 417.

[2]) Vgl. dieses Archiv Bd. XLVI. S. 156.

[3]) Pflüger's Archiv für Physiologie Bd. 3. S. 126.

minimaler Quantitäten eines neutralen Chininsalzes messbar eingeschränkt. Das Nähere hierüber wird demnächst zur Veröffentlichung gelangen.

Für den lebenden Organismus gelten keine anderen chemischen Gesetze als für die von ihm abgetrennten Theile. Was hier antiseptisch und oxydationsverhindernd wirkt, muss, wenn keine Zerlegung oder Bindung der wirkenden Substanz erfolgt, auch dort im Allgemeinen sich ebenso verhalten. Es erscheint diese Annahme um so mehr berechtigt, als sie mit den Ergebnissen des Krankenbettes übereinstimmt und für dieselben eine genügende Erklärung liefert. Welches nun ferner innerhalb der Säfte die Angriffspunkte des verbrennungshemmenden Alkaloides sind, wird erst durch weitere Forschungen zu bestimmen sein. Vorläufig wird es kaum mehr bezweifelt werden können, dass der Stoffwechsel nach der besprochenen Richtung hin bei der Chininwirkung unmittelbar betheiligt ist, dass demnach die bisher geltende **ausschliessliche** Auffassung des Chinin als eines directen Nervinum sich nicht halten lässt.

Auch die Heilung der intermittirenden Malariafieber ist wahrscheinlich von diesem Gesichtspunkt aus aufzufassen. Sie sind der Ausdruck einer durch das Gift verwesender oder faulender Pflanzentheile zu Stande gekommenen, in ihren schwereren Formen unter den Erscheinungen der putriden Zersetzung einhergehenden Säfteinfection; und wenn sie in ihrem Verlauf den besonders in der Peripherie deutlich zu beobachtenden tetanischen Krampf der vasomotorischen Nerven und damit die subjectiven Frosterscheinungen darbieten, so geschieht das Nehmliche, was wir bei pyämischen Fiebern wahrnehmen, die doch zweifellos humorale Erkrankungen sind, und was wir durch Injection putrider Flüssigkeiten beim Thier erzeugen und durch Chinin heilen können [1]. Dass die Schüttelfröste und was ihnen folgt in der Malariainfection rhythmisch, in der Pyämie unregelmässig auftreten; dass der fiebererregende Stoff hier continuirlich, dort nur anfallsweise in den Kreislauf und zur Wirkung gelangt; oder dass der Einfluss auf das Nervensystem vorzugsweise intermittirend sich geltend macht: ändert in dem Grundcharakter jener Krankheit nichts Wesentliches, denn der Rhythmus der Paroxysmen ist zuweilen nur angedeutet und kann selbst ganz

[1]) Dieses Archiv Bd. XLVI. S. 80—98.

fehlen, während doch eine unzweifelhafte Malariainfection vorliegt [1]). Und wenn diese der Regel gemäss Symptome von Seiten des Nervensystems hervorruft, so finden wir solches von Vergiftungen und von Krankheitsprozessen, die anerkannt nur auf heterogenen Stoffen im Blute beruhen, weit überboten. „Dass es sich, sagt **Griesinger** mit Recht, beim Wechselfieber von etwas ganz Anderem als einer Neurose handelt, das zeigt die auffallende Blutveränderung und allgemeine Ernährungsanomalie in den stärkeren Fällen, die Milzerkrankung und die so oft schon **vor** dem Fieberanfall erhöhte Eigenwärme des Körpers“ [2]). Jedenfalls schliesst meine Hypothese sich eng an die Mehrzahl der ätiologischen Bedingungen des Wechselfiebers, sowie an die leicht zu constatirenden Eigenschaften an, welche das Chinin in seinen äusseren Beziehungen gegenüber den ätiologischen Momenten darbietet. Eine Entscheidung wird freilich erst dann möglich werden, wenn das fehlende Mittelglied zwischen primärer Ursache und vollführter Heilung — die toxische Materie innerhalb des Kreislaufs — chemisch oder mikroskopisch wird nachgewiesen sein.

Die unmittelbare mikroskopische Beobachtung des Chinineinflusses gegenüber einem hervorragenden und ausserordentlich häufigen Krankheitsfactor, den zu Eiterzellen werdenden weissen Blutkörperchen, verleiht der Einsprache gegen die Auffassung des Alkaloides als eines directen Nervenmittels die sicherste Stütze. Alle Beziehungen zwischen Wirkung und Ursache liegen hier durchsichtig genug vor, um jeden Nerveneinfluss als weder zugehörig noch nothwendig erscheinen zu lassen. Ueber die Thatsachen selbst haben auch die von **Martin** und von **Kerner** angestellten ausführlichen Prüfungen wohl genügende Klarheit gegeben [3]). Da ferner die Bildung des Eiters, sowohl im Sinne der älteren Auffassung als in dem der neueren von **Cohnheim**, auf die nehmliche weitere Grundlage, gesteigerte Thätigkeit von Protoplasmagebilden, zurückzuführen ist; und da, soweit meine Erfahrungen reichen, **jeder** menschliche

[1]) Nach **Fischer** werden die Schüttelfröste in der Pyämie bedingt von einer rhythmisch eintretenden Infection des Blutes durch die allmählich sich erweichenden Thromben. Vgl. Ueber den heutigen Stand der Forschungen in der Pyämie-Lehre. Erlangen 1869. S. 17.

[2]) a. a. O. S. 40.

[3]) Vgl. dieses Archiv Bd. XLVII. S. 159. **Pflüger's** Archiv Bd. 3. S. 129—138.

Eiter dem Chinin gegenüber sich qualitativ gleich verhält: so bleibt es für den therapeutisch zu verwerthenden Einfluss sehr wahrscheinlich ohne Belang, welcher Quelle das pathologische Product entstammt.

Bei der Bestimmtheit, womit die Wirkung des Chinin bisher stets in ausschliesslichen causalen Zusammenhang mit dem Nervensystem gebracht worden war, musste es naturgemäss erscheinen, auch das in neuerer Zeit immer deutlicher erwiesene Wärmehemmungscentrum zur Erklärung heranzuziehen. Die so vielfach acceptirte „tonische Wirkung auf die Gefässnerven", von der freilich experimentell noch nichts bekannt geworden, würde damit ganz gut harmoniren. Trennung des Rückenmarks in den oberen Partien ruft allgemeine Gefässlähmung der abhängigen Regionen und damit, wenn die umgebende Temperatur nicht zu niedrig steht oder das Thier nicht zuviel Wärme wegen seiner Kleinheit abgibt, bald sehr hohe Blutwärme hervor. Umgekehrt liess sich gemäss der bisherigen Auffassung des Chinin als eines nervinen Arzneimittels erwarten, dass es als Agens von unbestritten wärmeerniedrigendem Einfluss diesen durch Reizung der vasomotorischen Nerven von jenem Centrum aus erzeuge. Zur Stütze konnten dieser Anschauung möglicherweise die von Chapéron gewonnenen Resultate dienen [1]. Wie frühere Forscher hatte er gefunden, dass sehr starke Dosen Chinin beim Frosch die Reflexerregbarkeit herabsetzen, und dass dieses, abweichend von der bisherigen Anschauung, lediglich auf erhöhter Erregung der am Gehirn liegenden Hemmungscentren, speciell der Vierhügel und der Medulla oblongata, beruhe.

Die experimentelle Bearbeitung der Frage nach dem Antheil des moderirenden Wärmecentrums bei der antipyretischen Chininwirkung ging von den Einrichtungen aus, welche Naunyn und Quincke dem mehrfach schon vorher durchgeführten Fundamentalversuch gegeben haben [2]. Es wurden nur Hunde verwendet, dieselben wurden möglichst kräftig gewählt, und zur Verhütung oder Abkürzung des im ersten Stadium nach Abtrennung des Rückenmarkes sich geltend machenden protrahirten Temperaturabfalles diente

[1] Unter Fick's Leitung geschriebene Dissertation: „Beitrag zur Kenntniss der physiologischen Wirkung des Chinin." Würzburg 1869.

[2] Reichert's und Du Bois' Archiv 1869. S. 178.

ein auf etwa 25° C. erwärmter und gut ventilirter grosser Kasten mit zwei Glaswänden.

Die Versuchsthiere wurden zuerst durch eine subcutane Injection von Morphin erschlafft und dann vorsichtig chloroformirt. Die einmal eingetretene Narcose dauerte dann so lange, dass nur hier und da noch eine kleine Quantität Chloroform nöthig war, um das Thier in bewusstlosem Zustand zu erhalten. Der kurze Dornfortsatz des siebenten Halswirbels wurde durch vorsichtiges Präpariren in der Mittellinie blossgelegt. Statt der „sehr eingreifenden Operation", den Wirbelbogen abzutragen, wie sie die Berliner Autoren ausgeübt, befolgten wir ein einfacheres Verfahren. Mit einer Knochenzange wurde der Dornfortsatz entfernt, auf den Stumpf eine Trephine gesetzt und dieselbe senkrecht in den Wirbelbogen eingebohrt. Am Nachlassen des Widerstandes und an den Zuckungen des Thiers liess sich erkennen, wann die Krone des Instruments im Wirbelkanal angekommen war, wobei natürlich die Drehungen zuletzt sehr vorsichtig und mit nur sehr gelindem Druck ausgeführt wurden. Das dem Kreis der Trephine entsprechende Knochenstück liess sich nun ziemlich leicht abheben; einigemal war es sammt der Dura mater in dem Instrument bereits haften geblieben. Auf dem Grund der Oeffnung lag das weisse Mark so deutlich zu Tag, dass eine genaue Zertrümmerung desselben mit der Kornzange in den meisten Fällen keine Schwierigkeit mehr darbot. Verschiedene Male wurde nicht in dieser immer noch umständlichen Weise das Mark zertrümmert, sondern durch Einführen eines doppelschneidigen Scalpells zwischen dem letzten Hals- und ersten Brustwirbel einfach durchschnitten. Da es sich zeigte, dass diese Methode die nehmlichen Resultate gab wie das Zerquetschen nach vorheriger Eröffnung des Wirbelkanals, so kam sie zuletzt ausschliesslich zur Anwendung. Die hierdurch bewirkte Blutung stand bei jungen Thieren nach Eindrücken eines mit kaltem Wasser gefüllten Schwammes sehr bald, bei alten Thieren erfolgte mehrmals Verblutung oder eine solche Schwäche, dass der Tod schon im ersten Stadium des Versuches eintrat. Die Hautwunde wurde mit einfachen Nähten jedesmal sorgfältig geschlossen.

Des nothwendigen Vergleiches wegen schicke ich hier den Normalversuch voraus. Ich habe denselben nur zweimal angestellt, da nach den bis jetzt vorliegenden anderweitigen Untersuchungen ein Zweifel an dem Vorhandensein eines wärmehemmenden Centrums, an dessen Lähmung durch die beschriebene Operation, sowie besonders an dem typischen Gang der hiernach auftretenden Erscheinungen nicht wohl mehr möglich ist. Dass auch der Wärmekasten als solcher nicht Schuld an der Temperatursteigerung ist, beweisen die zahlreichen Krankengeschichten von Zertrümmerung des Halsmarks beim Menschen [1]) und die ohne einen solchen Apparat

[1] Vgl. H. Weber, Transactions of the clinical society. London. 22. Mai 1868.

bei gewöhnlicher Zimmerwärme angestellten Versuche[1]), zu denen auch unsere Versuche II und III gehören.

I. Versuch.

Spitz von 7 Kilogr. Normaltemperatur 38,2. Zertrümmerung des Markes am 7. Halswirbel. Geringe Blutung. Wird mit Watte gut bedeckt in den Wärmekasten gebracht und zeigt darin 15 Minuten nach der Operation:

Zeit.		Temperatur.	Puls.	Athmung.	Kastenwärme.	Bemerkungen.
Uhr	Min.					
12	15	36,2	—	16	23	
12	30	36,0	—	—	24	
12	45	35,9	—	—	24	
1		36,0	—	—	26	Ist aus der bisherigen Narkose erwacht, liegt aber ganz ruhig. Kräftige diaphragmale Athmung.
1	15	36,0	112	18	25	
1	30	36,1	—	—	25	
1	45	36,3	—	—	26	Das Einführen des Thermometers in den Anus löst jedesmal leichte Reflexbewegung des Schwanzes aus.
2		36,5	—	—	27	
2	15	36,8	120	20	27	
2	30	37,0	—	—	28	Die Hinterextremitäten und der Rumpf sind vollständig gelähmt.
2	45	37,1	—	—	28	
3		37,3	—	—	28	
3	15	37,5	120	20	29	
3	30	37,7	—	—	28	
3	45	37,8	—	—	28	
4		38,0	—	—	29	
4	15	38,3	128	20	29	
4	30	38,6	—	—	29	
4	45	38,7	—	—	29	
5		38,7	—	—	27	
5	15	38,9	128	20	27	
5	30	38,8	—	—	26	
5	45	39,2	—	—	27	
6		39,2	—	—	27	
6	15	39,3	132	22	28	
6	30	39,6	—	—	27	
6	45	39,8	—	—	26	
7		40,0	—	—	26	

Um 6 Uhr 50 Min. ist die Respiration unregelmässig und schnappend geworden, der Puls schwankt zwischen 80 und 100. Einige Minuten nach 7 Uhr verendet das Thier unter leichten Krämpfen.

Bei einer Zimmertemperatur von 15 — 10° C. während der Nacht bleibt das Thier im beiderseits offenen ungeheizten Wärmekasten liegen. Das in den Anus

Im Gegensatz zu den Fieberzuständen, die man gemäss ihren Ursachen als entzündlich, septisch oder pyämisch bezeichnet, könnte man die nach Rückenmarkstrennung eintretende charakteristische Wärmesteigerung sachgemäss als paralytisches Fieber aufführen.

[1]) Naunyn und Quincke a. a. O. S. 322.

tief eingeführte englische Maximumthermometer zeigt am folgenden Morgen eine postmortale Temperatur von 41,5°.

II. Versuch.

Bastardneufundländer von beinahe 23 Kilogramm und 38,6 Normaltemperatur. Durchschneiden des Rückenmarks dicht vor dem ersten Brustwirbel. Der Wärmekasten wird von unten nicht geheizt, sondern auf der einen Seite offen in die Nähe eines mässig erwärmten Ofens gesetzt. Das Thier ist mit Watte bedeckt.

Zeit.		Temp.	Resp.	Kasten.	Bemerkungen.
Uhr	Min.				
10	30	38,2	16	18	Das Thier ist während des ganzen Versuches bis um 3 Uhr sehr ruhig, was der zu Anfang gegebenen etwas kräftigen Dosis Morphin (0,015) zugeschrieben wird.
10	45	38,2	16	19	
11		38,2	16	19	
11	15	38,1	20	19	
11	30	38,0	20	21	Um 11 Uhr 15 Min. werden 190 Ccm. auf 40 C. erwärmte Milch in den Magen injicirt.
11	45	38,1	20	21	
12		38,1	20	22	
12	15	38,3	20	22	
12	30	38,4	20	23	
12	45	38,4	20	24	
1		38,7	20	24	
1	15	38,7	20	25	
1	30	39,4	20	25	
1	45	39,7	20	25	
2		40,0	20	24	Die Athemzüge fortdauernd tief und regelmässig.
2	15	40,3	20	24	
2	30	40,6	28	24	
2	45	41,1	40	25	
3		41,4	—	25	

Wenige Minuten nach 3 Uhr, als eben die sehr beschleunigte Athmung gezählt werden sollte, erfolgten einige kurze Streckkrämpfe besonders von Rumpf- und Hinterextremitäten, Stillstand des Zwerchfells und Tod.

Die sogleich angestellte Section ergab glatte und vollständige Trennung der Medulla an besagter Stelle. — Die Temperatur stieg postmortal auf 42,3.

Die beiden vorstehenden Versuche geben genau dasselbe Bild wie die, welche von den früheren Autoren angestellt wurden. Indem ich ihr Ergebniss gleichsam als Normalcurve vorausschicke, soll sich an sie die Beantwortung der Frage anknüpfen, ob unter den hier demonstrirten Umständen, also bei vollständigem Ausschluss des moderirenden Wärmecentrums und bei grosser Intensität der fiebererregenden Ursachen, die antipyretische Wirkung des Chinin noch zur Geltung gelange.

III. Versuch.

Metzgerhund von 25,5 Kilogr. Normaltemperatur des Thieres 39,4. Eröffnung des 7. Halswirbels mit der Trephine, Zertrümmern des Marks mit der Kornzange.

Wird mit Watte bedeckt in den Kasten gelegt. Die Extremitäten ragen daraus hervor. Sogleich nach dem Einlegen ist der Temperaturbefund um 11 Uhr 30 Min. 39,0.

Zeit.	Temp	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
11 30	39,0	—	12	32	
11 45	38,7	—	—	30	
12	38,8	—	—	29	Erhält durch die Schlundsonde 120 Ccm.
12 15	38,8	—	14	26	erwärmte Milch.
12 30	38,8	—	14	27	
12 45	38,8	—	14	27	Die künstliche Erwärmung des Kastens
1	38,8	—	13	28	wird eingestellt. Zimmertemperatur 27,5°.
1 15	38,9	—	18	27	Die Fenster des Kastens halb offen.
1 30	39,0	—	15	28	Um 1 Uhr 30 Min. wieder 120 Ccm.
1 45	39,2	—	18	29	warme Milch.
2	39,3	—	16	30	
2 15	39,8	—	16	28	Um 2 Uhr 20 Min. werden 0,25 salz-
2 23	40,0	—	—	—	saures Chinin subcutan injicirt.
2 30	39,9	—	22	26	
2 40	40,0	—	22	27	
2 50	40,2	120	18	28	
3	40,3	—	22	26	
3 7	40,5	—	—	—	Um 3 Uhr 10 Min. durch den Magen
3 17	40,7	—	19	26	0,25 Chinin mit etwas HCl.
3 25	40,7	124	19	26	
3 35	40,7	—	24	26	
3 45	40,8	—	—	—	Chinin 0,4 durch den Magen; 0,1 sub-
4	40,7	121	25	25	cutan. — Nach wie vor keine künstliche
4 15	40,5	118	30	25	Erwärmung.
4 30	40,4	128	30	25	
4 45	40,5	128	32	24	
5	40,6	116	31	25	
5 15	40,4	118	38	24	
5 30	40,4	116	32	25	
5 45	40,1	118	32	25	
6	—	—	—	—	Chinin 0,4 durch den Magen.
6 10	40,0	112	30	25	
6 25	40,0	114	35	25	Unruhig mit Kopf und Vorderextremitäten.
6 40	40,0	108	32	27	
7	40,0	112	35	27	
7 20	40,1	120	30	25	Puls sehr klein.
7 50	40,2	138	26	25	
8 25	40,7	154	31	26	

Das Thier liegt ruhig. Es soll ihm eine abermalige Injection von Chinin in den Magen gemacht werden. Die schon vorher mehrmals benutzte Schlundröhre war durch einen Biss schadhaft geworden. Aus der schadhaften Stelle strömte von der Lösung ein guter Theil in die Luftröhre und führte fast augenblicklichen Tod herbei.

Fünf Minuten nachher betrug die postmortale Temperatur 41,0; auch später war sie, wie das Maximumthermometer am folgenden Morgen auswies, nicht höher gegangen.

Die Wärme des Zimmers betrug 25,0° C. am Abend. Das Cadaver blieb während der schwülen Nacht in einem Raum daneben liegen. Am anderen Tage, 14 Stunden nach dem Tode, wurde die Section vorgenommen. Das Rückenmark war an der genannten Stelle total zerstört. Die Leber im Parenchym und an der Oberfläche emphysematös; die Milz klein, etwas knisternd, sonst in Consistenz und Aussehen unverändert. Das Blut meist gut geronnen. An keiner Stelle sonst irgend bemerkenswerthe Zeichen der Putrescenz. — In den Bronchen viel röthlich gefärbte Flüssigkeit.

Die Bedingungen zur Wärmeproduction waren in diesem Versuch sehr günstig. Ein grosses, sich im Anfang des Versuchs also wenig abkühlendes Thier, Bedecktsein mit einem schlechten Wärmeleiter, Injection warmer Nahrung und eine ziemlich hohe Zimmertemperatur. Alle früheren Versuche und Beobachtungen stimmen darin überein, dass in diesen Dingen das sich heranbildende Fieber eine mächtige Unterstützung findet. Demgemäss war auch der anfängliche Abfall nur gering im Vergleich zu der Mehrzahl der publicirten Versuche und hatte seinen tiefsten Stand schon 30 Minuten nach der Operation erreicht. Nach einstündigem Stillstand bekam die Wärmeproduction im Innern das Uebergewicht über die Abgabe durch die erweiterten Gefässe nach aussen, und es begann eine Steigerung, die um 2 Uhr 0,5 in einer Viertelstunde darbot. Die erste Chinininjection brachte nur einen geringen aber doch unmittelbaren Abfall zu Stande; der zweiten folgte nur eine Verflachung der Curve; dagegen zeigte die dritte ein deutlicheres Resultat, Abfall von 0,7 in zwei Stunden mit Beginn derselben in der ersten Viertelstunde nach der Einverleibung des Chinin.

In dem Sectionsbefund ist die Abwesenheit aller eigentlichen Putrescenz, mit alleiniger Ausnahme der Leber, bemerkenswerth, die sonst nach der ausgeführten Operation und dem durch sie bedingten hochgradigen Fieber die Regel ist, und zu deren Entstehung ebenfalls diesmal alle äusseren Bedingungen sehr günstig waren. Auch die postmortale Wärmesteigerung war, im Vergleich zu ihrer sonstigen Höhe in den nicht beeinflussten Versuchen, sehr gering.

IV. Versuch.

Bastardhofhund von 18 Kilogr. Normaltemperatur 38,3. Wird nach vollzogener Operation um 11 Uhr 30 Min. in den Wärmekasten gebracht. Die Watteeinhüllung ist sehr vollständig, den ganzen Körper mit Ausnahme des Kopfes umgebend. Die Gasflamme unter dem eisernen Boden des Kastens von 11 Uhr 35 Min. an sehr schwach.

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
11 35	39,3	—	28	30	
11 50	39,2	—	26	25	
12 15	39,7	—	20	26	
12 30	39,9	—	20	26	
12 45	40,1	160	24	26	Um 12 Uhr 52 Min. Chinin 0,3 subcutan, um 1 Uhr 8 Min. die nehmliche Dosis mit etwas HCl durch den Magen.
1 10	40,8	—	40	27	
1 15	40,8	—	—	27	
1 20	40,9	—	—	27	Um 1 Uhr 10 Min. heftiges Schütteln des ganzen Körpers. Anhaltende Unruhe, besonders der Vorderextremitäten. Athmung und Puls deshalb nicht zu zählen.
1 30	41,0	—	—	27	
1 35	41,1	—	—	26	
1 45	41,2	180	44	26	
1 55	41,2	—	40	27	Um 1 Uhr 32 Min. wieder 0,3 Chinin durch den Magen. 1 Uhr 35 Min. ruhiger. 1 Uhr 45 Min. abermals 0,3 Chinin ebenso, desgleichen um 1 Uhr 8 Min.
2	41,4	186	40	27	
2 10	41,5	—	48	26	
2 30	41,5	—	46	26	

Von 2 Uhr 40 Min. an der Puls über 200, die Respiration wird unregelmässig und schnappend. Bald darauf tetanische Streckung aller Extremitäten und Tod. Die postmortale Steigerung bot 41,9 dar. Wegen localer Hindernisse konnte die Section nicht angestellt werden.

Auch hier wurde die innere Wärmeproduction durch die äusseren Einrichtungen des Versuches sehr gefördert, mehr noch wie vorher. Dem entsprechend kam das Anfangsstadium des Abfallens nicht einmal zu einem rasch vorübergehenden Ausdruck. Das Gelungensein des operativen Eingriffes konnte schon aus der rapiden Steigerung und aus der completen Lähmung mit rein diaphragmaler Respiration mit Sicherheit auch ohne nachträgliche Section entnommen werden. Der Einfluss des Chinin ist unbedeutend trotz der sehr hohen Gaben. Es gelingt nur — und möglicherweise kann man auch da noch an Zufälligkeiten denken —, die steile Curve zweimal (1 Uhr 10 Min. und 1 Uhr 45 Min.) zu einem kurzen Stillstand zu bringen. Die heftige Unruhe des Thieres ist gemäss der Analogie mit einem späteren Fall auf eine nicht *ganz* vollständige Zertrümmerung des Markes zurückzuführen. Das Verenden machte durchaus den Eindruck einer Herzlähmung, veranlasst durch das Chinin [1]). Bemerkenswerth ist auch hier die geringe postmortale Steigerung.

V. Versuch.

Kräftiger Wolfshund, 26 Kilogr. schwer. Operation durch directe Durchschneidung des Rückenmarks zwischen dem letzten Hals- und ersten Brustwirbel. Die Schwierigkeit, das Thier zur Operation herzurichten, verursachte, dass erst unmit-

[1]) Vgl. Briquet, Traité thérapeutique du Quinquina. Paris 1855. p. 81.

telbar nach derselben gemessen wurde. Um 10 Uhr 40 Min. betrug die Temperatur demnach 41,5. Es ist anzunehmen, dass der heftige Widerstand diese abnorme Höhe zum grossen Theil wenigstens veranlasst hatte.

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
10 40	41,5	—	—	—	
11	41,1	—	224	20	
11 15	40,7	—	—	20	
11 20	40,8	—	—	20	
11 25	40,6	—	—	20	
11 30	40,3	—	—	20	
11 45	40,0	—	—	21	
12	39,9	—	—	23	
12 15	39,9	—	—	24	
12 30	39,9	—	—	24	
12 45	39,8	—	—	24	
1	39,7	—	—	25	Andauern der Morphinnarkose.
1 15	39,7	—	—	24	
1 30	39,7	—	114	25	
1 45	39,7	—	—	24	
2	39,8	—	—	25	
2 15	39,8	—	98	25	
2 30	40,0	—	—	24	
2 55	40,4	—	—	23	
3	40,5	—	—	23	Um 3 Uhr 5 Min. wird 1,0 Chinin mit 150 Ccm. Wasser in den Magen eingeführt.
3 15	40,4	—	—	23	Von 3 Uhr 15 Min. an ziemlich unruhig.
3 30	40,3	—	—	23	
3 45	40,4	—	—	23	
4	40,5	—	63	22	
4 15	40,4	—	94	22	Um 4 Uhr 20 Min. wieder 1,0 Chinin in 150 Ccm. Wasser auf 40° erwärmt.
4 30	40,4	—	112	22	
4 45	40,6	—	100	22	
5	40,5	—	—	22	Um 5 Uhr 1,0 Chinin mit 100 Ccm. auf 40° erwärmtem Wasser. Fortwährend unruhig. Kein Zeichen von Chinintrunkenheit.
5 15	40,6	—	—	22	
5 30	40,6	—	60	21	
5 45	40,8	—	—	20	Um 5 Uhr 30 Min. Injection von 1,0 Chin. muriatic. amorph. [1])
6	40,8	—	—	—	
6 15	40,9	—	90	21	Um 5 Uhr 55 Min. heftiger, etwa 10 Sec. anhaltender Tetanus.
6 23	40,5	—	—	21	

Gegen 6 Uhr 25 Min. erfolgt in einem wiederholten tetanischen Anfall der Tod. Um 6 Uhr 35 Min. zeigt das Thermometer 41,3, was später nicht mehr überschritten wird.

Am folgenden Tage, nachdem das Cadaver 15 Stunden lang in einem warmen Stall gelegen, wurde die Section gemacht. Sie ergab: Keinerlei Fäulniss. Milz und Leber gross und etwas weich, beide ohne Emphysem. Im Magen nur gegen 30 Ccm. wässriger Flüssigkeit. Das Rückenmark an besagter Stelle bis auf ein Viertel durchschnitten, das nur gequetscht und mit Blut unterlaufen erschien.

[1]) Präparat von C. Zimmer, das sich durch seine Haltbarkeit, leichte Löslichkeit (1 : 1 Wasser) und seinen billigen Preis auszeichnet.

Die Chininwirkung war schon nach der ersten Gabe sehr deutlich. Wenn auch der Abfall selbst kein bedeutender zu nennen ist, so muss doch die Dauer der Verflachung der Curve bei der sehr bedeutenden Steigung als Folge des Chinin gelten. Bestimmter wohl ist die Abwesenheit der Fäulniss darauf zu beziehen. — Dass der tetanische Anfall um 5 Uhr 30 Min. von dem Chinoidin abhängig war, einem Präparat, das in Folge seiner leichten Löslichkeit ungemein rasch resorbirt wird und dadurch leicht toxisch wirken kann, ist wohl entschieden zu verneinen. Es wird deshalb schon wenig wahrscheinlich, weil auch sonst, ohne irgend einen Eingriff, nach der beschriebenen Operation solche Anfälle zuweilen auftreten [1]).

VI. Versuch.

Munterer Rattenfänger von etwas über 12 Kilogr. — Temperatur 40,7. Operation wie vorher. Gute diaphragmale Athmung.

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
11 30	39,3	—	28	20	
11 45	38,4	160	28	24	
12	38,4	—	—	26	
12 15	38,4	160	28	25	
12 30	38,8	—	—	25	
12 45	38,9	160	24	25	
1	39,2	—	—	25	
1 15	39,4	—	—	24	
1 30	39,6	—	—	23	Um 1 Uhr 20 Min. Chinin 0,4 in 60 Ccm. Wasser.
1 45	39,9	—	—	24	
2	39,7	116	26	24	
2 15	39,6	—	—	23	
2 30	39,6	—	—	22	
2 45	39,5	—	—	23	
3	39,5	130	28	23	
3 15	39,8	—	—	23	
3 30	40,1	—	—	23	Um 3 Uhr 35 Min. Chinin 0,5 wie vorher.
3 45	40,3	—	—	23	
4	40,6	—	28	23	
4 15	40,7	—	—	23	Der Puls ist wegen Muskelzittern nicht zu zählen.
4 30	40,7	—	—	21	
4 45	40,7	—	—	22	
5	40,8	140	32	22	Stickkrämpfe der Halsmuskeln.
5 15	40,5	—	—	23	
5 30	40,1	—	24	24	Liegt ganz ruhig.
5 45	40,0	—	—	22	
6	39,9	132	30	27	

[1]) Archiv f. Anat., Physiologie u. s. w. a. a. O. S. 182 u. 194.

Während einiger Minuten nach 6 Uhr wurde der immer noch ruhig daliegende Hund nicht beobachtet. Als um 6 Uhr 12 Min. nachgesehen wurde, hatte er verendet. Das sofort eingeführte Thermometer zeigte nach 15 Min. 40,4, worauf es blieb. — Die Section wurde sofort angestellt und ergab complete Trennung des Halsmarkes am untersten Wirbel.

Bei starker Tendenz zum Steigen bewirkte hier die erste Dosis Chinin einen deutlichen Abfall. Die später wieder beginnende Steigerung wird durch die zweite Dosis zuerst angehalten, dann herabgesetzt. Der normale Puls kurz vor dem Ende lässt möglicherweise auf eine andere Todesursache als toxische Herzparalyse schliessen.

VII. Versuch.

Zarter Bastardwachtelhund von 4 Kilogr. und 39,8 Normaltemperatur. Operation ebenfalls durch Schnitt. Unmittelbar nach ihr ist die Temperatur 37,5.

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
11 55	37,5	90	20	—	
12 15	36,3	—	12	24	
12 30	36,2	—	—	25	Um 12 Uhr 25 Min. Einhüllen des ganzen Thieres in Watte.
12 45	36,2	80	12	25	
1	36,6	—	—	24	
1 15	37,0	—	—	23	
1 30	37,5	—	—	23	Um 1 Uhr 30 Min. Chinin 0,2 in 30 Ccm. Wasser.
1 45	37,8	—	—	24	
2	37,8	84	26	24	
2 15	37,9	—	—	23	
2 30	37,8	—	—	22	
2 45	38,0	—	—	22	Um 2 Uhr 50 Min. Chinin wie vorher.
3	38,2	100	28	23	
3 15	38,0	—	—	23	
3 30	38,3	—	—	23	
3 45	38,4	—	—	23	
4	38,4	—	29	23	
4 15	39,1	—	—	23	Um 4 Uhr 15 Min. Chinin 0,4 durch den Magen in Wasser.
4 30	38,6	—	—	21	
4 45	38,5	—	—	22	

Um 4 Uhr 50 Min. ist das Herz trotz der vollkommenen Ruhe des Thieres nicht mehr zu fühlen. Es tritt kurzer Streckkrampf und damit der Tod ein. Die Temperatur ist während des Krampfes 38,2 und steigt postmortal auf 38,6. — Die Section, welche sofort vorgenommen wird, ergibt totale Trennung des unteren Halsmarkes, entsprechend dem letzten Wirbel.

In diesem Versuch folgt den beiden ersten Gaben nur ein kurzes Flachwerden der Curve. Das Fieber zeigt sich von Anfang an höchst energisch (+1,6 in einer Stunde). Die dritte für das kleine Thier zu starke Dosis des Chinin verursachte den Tod unter den Symptomen der Herzmuskellähmung. Die postmortale Wärmesteigerung ist gering.

Vorstehende Versuche weisen darauf hin, dass **die antipyretische Chininwirkung unabhängig ist von dem moderirenden Wärmecentrum.** Etwa zu gleicher Zeit mit mir haben **Naunyn** und **Quincke** als Fortsetzung ihrer bereits citirten ersten Arbeit das nehmliche Thema zum Gegenstand experimenteller Untersuchungen gemacht und im Wesentlichen das gleiche Resultat erlangt [1]).

Dass das Centralnervensystem bei der antipyretischen Chininwirkung gar nicht betheiligt sei, folgt freilich aus vorstehenden Versuchen noch nicht. Da im Organismus ein und dieselbe Thätigkeit sehr oft von verschiedenen Punkten aus geleistet werden kann, so lässt sich ungezwungen auch hier an die Möglichkeit denken, in beliebigen Fieberzuständen komme durch Mithilfe des Gehirns unter dem Einfluss des Chinin ein um so stärkerer Abfall zu Stande. Directe thatsächliche Anhaltspunkte liegen für diese Auffassung aber nicht vor; und bedenkt man dazu die vorher und früher von mir und Anderen nachgewiesenen sehr ausgeprägten Beziehungen des Alkaloïdes zu den einfachen Mischungsverhältnissen der Körpersäfte, so wird man sich der Ueberzeugung kaum verschliessen können, dass jene Auffassung zum mindesten nicht nothwendig oder naheliegend erscheint.

Ein anderer Einwurf wäre vielleicht von grösserem Gewicht. Betrachtet man sich die Curven der Berliner Normalversuche, die ohne Chinin oder ein sonstiges Mittel wie No. I u. II unserer Reihe angestellt wurden, etwas genauer, so sieht man, dass unter 15 Malen deren 8 einen Temperaturabfall inmitten des bereits eingetretenen Fieberstadiums spontan darbieten. Derselbe könnte demnach auch ohne therapeutischen Eingriff zu Stande kommen. Viermal nun erklärt er sich in unmittelbar zureichender Weise. Es scheint nehmlich, dass der Wärmekasten nicht anhaltend bewacht und vor zu

[1]) a. a. O. S. 526. — Diese Untersuchungen wurden mir erst Ende Januar bekannt, wo das Decemberheft des genannten Archivs hier zur Ausgabe gelangte, nachdem ich meine Gesammtresultate bereits am 11. November v. J. der medicinischen Section der Niederrheinischen Gesellschaft vorgetragen hatte (vgl. Berliner klinische Wochenschrift vom 12. December 1869).

starker Abkühlung bewahrt wurde. Die Messungen fanden in Intervallen von einer halben bis zu fünf Stunden statt, und so wurden jene Schwankungen in der Körperwärme möglich, die zwar dem damals zu suchenden Endresultat durchaus keinen Abbruch thaten, jedoch bei Betrachtung der Frage nach vorübergehender künstlicher Temperaturerniedrigung nicht ohne Bedeutung sind. In den restirenden vier Fällen fehlt die Wärmeangabe über den Kasten für die betreffende Periode des Versuchs, und so ist aus der naheliegenden Analogie zu schliessen, dass auch in ihnen der intercurrirende Abfall seinen ausreichenden Grund in dem sehr bedeutenden Schwanken der äusseren Temperatur hatte. Es dürfte dies um so wahrscheinlicher sein, als in keinem dieser Fälle eine Angabe darüber vorliegt, dass die Wärmeabgabe durch Einhüllen in schlechte Wärmeleiter gehindert war.

Was in unserer Versuchsreihe die antipyretische Wirkung des Chinin vollkommen klar erscheinen lässt, ist das Unmittelbare des Abfalls oder des Stillstands der Curve bei fast jeder Darreichung, während ein irgendwie bemerkenswerthes Sinken ohne vorherige Chininaufnahme niemals vorkam. Wie die Untersuchungen von **Kerner** ausweisen, erfolgt die Resorption rasch genug, um binnen dem aufgeführten Zeitunterschied die Möglichkeit der Einwirkung mit Bestimmtheit darzubieten [1]). Unzulässig wäre der Einwurf, das Sinken der Temperatur sei nur unter dem Einfluss toxischer, das Leben allmählich aufhebender Gaben zu Stande gekommen. Es ist leicht zu ersehen, dass die antipyretische Wirkung mehrmals eintrat, als davon entfernt noch keine Andeutung vorlag.

Anderseits ist nicht zu verkennen, dass die Wirkung des Chinin zuweilen fast ganz versagt. In Versuch III ist sie höchstens durch einen kurzen Stillstand der Curve markirt, aber auf die Dauer geht das Fieber unbehindert seinen Gang trotz der bedeutenden Gabe von 1,2 Gramm auf 18 Kilo Körpergewicht. Ebenso finden sich bei **Naunyn** und **Quincke** drei Experimente verzeichnet, in denen trotz grösster Dosen das Fieber in voller Weise zu Stande kam. Vielfache Beobachtungen an Menschen mit continuirlichem Fieber lehren das Nehmliche.

Die Erklärung hierfür kann, was wenigstens unsere Versuchsreihe angeht, nicht schwer fallen. In dem verzeichneten Fall

[1]) a. a. O. S. 160.

waren alle Bedingungen, die das Fieber befördern konnten, so günstig eingerichtet, dass die Energie der Verbrennung eine ganz ungewöhnliche war. Schon 5 Minuten, nachdem das operirte Thier in den Kasten gebracht worden, hatte die Körperwärme einen höheren Stand als normal, später stieg sie um 0,7 in 25 Minuten ungeachtet des beigebrachten Antipyreticums. Auch die vorher genannten Forscher bringen für die Thatsache, dass Chinin zuweilen keinen bestimmten Einfluss auf die Fieberhitze ausübe, erklärende Gründe bei, neben denen die zweimal deutlich constatirte Einwirkung als zweifellos bestehen bleibt. Vielleicht hat man sich den Gang der Dinge so vorzustellen, dass bei zu energischer Oxydation im Organismus das Chinin in die von Kerner im Harn entdeckte und vermittelst hypermangansauren Kali's dargestellte Modification, das Dihydroxylchinin, bald verwandelt wird. Es ist ein ziemlich einfaches Oxydationsproduct des ursprünglichen Präparates, das zwar die meisten chemischen Erkennungsreactionen wie dieses gibt, dabei aber sämmtlicher vom Chinin bekannter physiologischer Reactionen vollkommen baar ist. Dieser Körper findet sich bei gesunden Thieren im Harn nur in geringen Quantitäten [1]). Bei sehr gesteigerter Verbrennung ist seine Formation und Anwesenheit in grösseren Mengen jedenfalls zu vermuthen. Man würde in der Bestätigung davon den hauptsächlichsten endgültigen Grund für die häufige Unzulänglichkeit selbst grosser und thatsächlich resorbirter Chinindosen besitzen. Mit genügendem klinischen Material kann es nicht schwer sein, jene Vermuthung in der einen oder anderen Weise zu entscheiden.

Aus den vorgeführten Versuchen folgt einstweilen, dass die wärmeerniedrigende Wirkung des Chinin unabhängig ist vom Gehirn; es bleibt die Möglichkeit davon für das Rückenmark übrig. Aus den Untersuchungen von v. Bezold, Ludwig und Thiry [2]) geht hervor, dass seine Reizung, nach Abtrennung vom Gehirn, allgemein gefässverengernd wirkt. Man hat dem Chinin nicht selten eine solche reizende Einwirkung zugeschrieben, bis jetzt ohne die Spur eines experimentellen Beweises. Wäre sie vorhanden, so liesse sich wohl denken, dass sie das Gegentheil von der durch obige Operation ausgehenden Gefässlähmung leiste; wie diese Fieber, so

[1]) a. a. O. S. 113.

[2]) Sitzungsberichte der Wiener Akademie Bd. 49. S. 421—454.

sie Fieberabfall. Die ganze Symptomenreihe der Chinineinwirkung weist jedoch nur auf Depression des Rückenmarks und seiner Adnexe hin, keineswegs auf eine Reizung. Nicht viel anders verhält es sich mit etwaigen Beziehungen des Chinin zur Medulla oblongata. Die oben mitgetheilten Tabellen bieten nichts dar, was darauf schliessen liesse, mancherlei experimentelle Thatsachen sprechen gegen einen solchen Zusammenhang [1]).

Die späteren Versuche werden es nothwendig machen, auf die Bedeutung der postmortalen Temperatursteigerung für dieselben näher einzugehen. Was wir vom Chinin darauf Bezügliches gewahren, ist freilich nicht absolut beweiskräftig für die von mir vertretene Theorie. Immerhin jedoch wird man zugestehen müssen, dass die postmortale Steigerung in den Chininversuchen, wenn sie auch nicht ganz ausblieb wie zweimal beim Alkohol, dennoch im Vergleich zu den rein pathologischen Fällen, in denen sie auch anderwärts gemessen wurde, eine sehr mässige ist.

Mit Recht weisen die Berliner Experimentatoren auf einen anderen Grund hin, aus dem die Nichtwirkung selbst grosser Gaben Chinin in febrilen Zuständen erklärt werden könnte. Es ist der Mangel genügender Resorption. Für unsere Versuche möchte ich kein besonderes Gewicht darauf legen, weil der Befund in No. V zu deutlich dagegen spricht. Die Einwirkung des Medicamentes war hier jedenfalls gegenüber den enormen Gaben eine nur sehr geringe zu nennen; wie aber später die Untersuchung des Magens auswies, war die Resorption ziemlich vollständig vor sich gegangen. Dagegen kommt der angeführte Grund in Krankheitszuständen beim Menschen gewiss oft genug zur Geltung, und wenn ich für diese Ansicht auch keinen stricten Beweis beibringen kann, so scheint sie mir doch in mancherlei Thatsachen hinreichend begründet, um bei der Wichtigkeit des Gegenstandes hier nochmals erörtert zu werden.

Das bisher fast ausschliesslich angewandte Präparat ist schwefelsaures Chinin, löslich in etwa 750 Theilen Wasser von Zimmertemperatur. Durch Zusatz freier Säure steigert sich die Löslichkeit wie bekannt in erheblicher Weise. Der gesunde Magen enthält freie Salzsäure in genügender Quantität, im kranken ist sie vermindert,

[1]) Vgl. dieses Archiv Bd. XLVII. S. 361. — Schon früher Schlockow in Heidenhain's Studien des physiolog. Inst. zu Breslau. 1861. S. 163.

im Magen eines stark fiebernden Menschen scheint sie, wie aus mehrfachen anderweitigen Anzeichen hervorgeht, so gut wie ganz zu fehlen. An ihre Stelle ist nicht selten eine grössere Menge **alkalisch** reagirenden Schleims getreten [1]). Schon bei Leichtkranken kann man bestätigt finden, was unter den practischen Aerzten allgemein bekannt ist, dass einigermaassen stärkere Dosen schwefelsauren Chinins oft gastrische Beschwerden veranlassen, wenn sie noch fehlten: Druck im Epigastrium, belegte Zunge, Aufstossen, von der Magenwand unmittelbar bewirktes Erbrechen und Durchfälle. Ungeachtet seiner grossen Vorliebe für das Medicament und obschon er stets die flüssige Form mit Säurezusatz anwendete, kommt **Briquet** nach zahlreichen Beobachtungen doch zu dem Schluss: „Dans l'état de fièvre, la tolérance de l'estomac pour le sulfate est moins absolue; il survient assez facilement de légères excitations qui n'ont jamais de conséquences sérieuses. Enfin, quand la fièvre est vive, ou quand la membrane muqueuse de l'estomac est déjà phlogosée ou disposée au ramollissement, le sulfate de quinine peut provoquer la formation de phlegmasies non douteuses..... L'intestin se comporte avec les sels de quinine de la même manière que l'estomac [2]).

Es braucht uns das nicht zu wundern, und ebensowenig werden wir es unerklärlich finden, dass unter solchen Umständen die Resorption des schwerlöslichen Salzes unterbleiben muss. Um einen Scrupel Chininsulfat (1,20 Gramm), diejenige Gabe, auf welche ein starkes Fieber mit einiger Sicherheit reagirt, in Lösung überzuführen, bedarf man beinahe ein Liter destillirtes Wasser. Statt dessen wird jene Dosis in Pulver- oder Pillenform meistens mit einigen Esslöffeln Brunnenwasser in den hyperämischen oder schleimbedeckten Magen gebracht. Wenn es Jemandem einfiele, unter ganz denselben Verhältnissen einen Scrupel Gyps zu verordnen, so würde man von wegen der künstlichen Magenbelästigung billigerweise alle Ursache zu haben glauben, das höchst irrationell zu finden. Und dennoch ist der schwefelsaure Kalk in nichtsaurem Wasser um einen guten Theil leichter löslich und örtlich jedenfalls weniger irritirend

[1]) Vgl. **Beaumont**, Neue Versuche u. s. w. Uebersetzt von B. **Luden**. Leipzig 1834. S. 72 u. 187. — Ferner experim. u. literar. Andeutungen darüber bei **Frerichs** in **Wagner's** Handwörterb. d. Physiol. Bd. III. S. 790.

[2]) a. a. O. S. 219.

als das gleichnamige Chinin. Seit der Anwendung grösserer Gaben des Alkaloides ist der gerügte Missgriff in einer grossen Anzahl von Krankengeschichten aus deutschen Hospitälern verzeichnet, denn wenn meistens auch einfach nur „1 Scr. Chin. sulph." aufgeführt wird, so lässt sich doch aus directer Anschauung leicht feststellen, dass man diese Quantität gewöhnlich in der vorher angegebenen Weise administrirt. Es dürfte schon dieser Grund hinreichend sein, um alle dabei gewonnenen **negativen** Resultate als **nicht** beweisend ansehen zu müssen. Ganz gewiss wird es Fieberzustände genug geben, in denen die an und für sich noch zulässigen Chiningaben in stärkster Quantität nicht im Stande sind, gegen die Heftigkeit der fiebererregenden Ursache mit Erfolg anzukämpfen; ebenso gewiss aber ist es doch zum mindesten unpractisch zu nennen, wenn man zur Erreichung eines Erfolgs sich einer möglichst unsicheren Methode bedient.

Vielleicht beruht auch in wissenschaftlichen Arbeiten der Widerspruch einzelner Resultate auf diesem Mangel an zuverlässiger Einrichtung des Versuches. So findet, um unter vielen Autoren den neuesten zu nennen, E. **Unruh** in seinen schätzenswerthen Untersuchungen über die Stickstoffausscheidung bei fieberhaften Krankheiten [1]), „dass die Wirkungen des Chinins äusserst verschieden sind; während in dem einen Falle die N-Ausfuhr sofort beschränkt wird, tritt im anderen Falle zuerst eine Vermehrung derselben und dann Verminderung ein, während wiederum in anderen Fällen gar keine Einwirkung weder auf die Temperatur noch auf die N-Ausscheidung ersichtlich ist." Der genannte Autor spricht nur von der Darreichung schwefelsauern Chinins, in der Dosis von 1—2 Gramm. Der gewöhnlichen Hospitalpraxis gemäss muss man an die Darreichung in Pulverform denken; jedenfalls erfahren wir aus den Versuchen nicht, ob das gereichte schwerverdauliche Medicament auch wirklich zum grössten Theil resorbirt wurde. Ohne diesen Nachweis aber lässt sich dem Inhalt des vorgebrachten Citats diejenige Genauigkeit nicht zuerkennen, welche er mit demselben möglicher Weise darbieten kann.

Was ich betreffs der besseren oder schlechteren Resorption des Chinin hier als Schlussfolgerung aus anderweitigen Thatsachen bringe, hat **Briquet** in genauerer Weise einer ganzen Fülle von klinischen

[1]) Dieses Archiv Bd. XLVIII. S. 293.

Beobachtungen entnommen. Wenn irgend ein Forscher, so ist er berechtigt, über diesen Gegenstand ein zuverlässiges Urtheil zu fällen. Ich sehe ab von einer Mittheilung aller darauf bezüglichen Versuche und Erfahrungen und gebe nur einige seiner resumirenden Worte. Er lässt dreimal 35 und 40 und zweimal 50 Centigramme Chininsulfat in Pulverform mit einer Gummilösung nehmen und beobachtet dann wie vorher bei 15 und 20 Centigramme unter sonst gleichbleibenden Erscheinungen sowohl die physiologischen Symptome wie den durch Jod-Jodkalium im Harn bewirkten Niederschlag. „Ce résultat indique une action égale à celle qui produirait de 15 à 20 Centigrammes de sulfate acide; par conséquent, l'influence du sulfate neutre est de plus de moitié moins forte que celle du sulfate acide [1]).

Aus einer Reihe von anderen Versuchen ergibt sich Seite 634 genau das nehmliche Resultat. „Par conséquent, sous le rapport de la puissance d'absorption et de la puissance d'action, la poudre du sulfate de quinine neutre est inférieure de plus de moitié au bisulfate de quinine en solution. En faisant le calcul, on trouve que 30 Centigrammes de la première n'équivalent pas à 15 Centigrammes du second;" und Seite 637: „En resumé, la forme pulvérulente est défectueuse; elle a une action peu sûre, lente et faible; elle ne pare à rien et expose à des inconvénients. On ne doit donc s'en servir que quand la répugnance des malades est telle qu'on n'en peut employer d'autre, et alors il faut augmenter la dose d'un tiers au moins, et donner immédiatement, des boissons acides. Passable dans les cas où il n'est pas nécessaire d'administrer de fortes, elle ne peut guère servir pour les doses élevées."

Einen anderen für die grössere Sicherheit der antipyretischen Chininwirkung wichtigen Gegenstand möchte ich hier in Erinnerung bringen, da auch ihm, wie die allermeisten klinischen Berichte darthun, nicht die gebührende Aufmerksamkeit geschenkt wird. Liebermeister hat denselben auf Grund ausgedehnter Erfahrung in die Worte zusammengefasst, „dass für die Wirkung eine grössere Opportunität besteht, wenn die Temperatur auch spontan im Sinken, eine geringere, wenn sie im Steigen begriffen ist; dass es demgemäss auf der Höhe des Fiebers im Allgemeinen zweckmässiger ist, das Chinin in den späten Abendstunden oder des Nachts anzu-

[1]) a. a. O. S. 629.

wenden [1])." Die einfachste Erklärung für diese Thatsachen würde ebenfalls darin zu suchen sein, dass zur Zeit der sehr energischen Verbrennung das Alkaloid sich zu rasch und in zu grosser Quantität in das pharmakodynamisch unwirksame Kerner'sche Oxydationsproduct verwandele. Ob diese Vermuthung sich bestätigen wird oder nicht, jedenfalls können nur die Angaben über Unwirksamkeit des Chinin in fieberhaften Krankheiten auf Geltung Anspruch machen, gemäss denen ein leicht verdauliches Präparat in genügenden Dosen und zur richtigen Zeit verwendet wurde. Der letztere Punkt involvirt, und es wird dies aus unseren Experimenten bestätigt, dass sehr oft überhaupt nicht viel erwartet werden kann, wenn die fiebererregende Ursache zu weit vorangeschritten, dass dagegen ein frühzeitiges Niederhalten der eben aufsteigenden aber noch nicht excessiven Temperatur bessere Aussichten gewährt. Es mag das vielleicht besonders für jene Fälle gelten, wo man aus epidemischen Gründen mit einiger Sicherheit auf die kommende Entwickelung von hochgradigem Fieber, z. B. der Scarlatina, wird schliessen können.

Erst wenn einheitlich nach solchen Gesichtspunkten verfahren wird, können Widersprüche, wie sie die neueste wissenschaftliche Literatur aufweist, ferner nicht mehr möglich werden. So möge hier nur erwähnt sein, was ein Bericht über die Typhusbehandlung auf der Abtheilung von Lindwurm im städtischen Krankenhaus zu München aussagt [2]): „Wenn bei einem Typhuskranken trotz der beständigen Applicationen der Eisblasen (auf Kopf, Brust und Bauch) die Temperatur doch in einer Weise zunahm, dass eine sehr grosse Anzahl Bäder nothwendig wurde, z. B. 8—10, so brachte sehr oft eine grosse Dosis Chinin (Gr. xx-xxx) die dem Kranken sehr angenehme Wirkung hervor, dass er vielleicht mit je fünf Bädern für diesen und den nächsten Tag auskam. . . . Zahlreiche Beispiele haben Verf. davon überzeugt, dass das Chinin die Wirkung der Bäder wesentlich unterstützt. Indess hebt Verf. besonders hervor, dass nur grosse Dosen einen so sichtbaren Einfluss ausüben, wie dass die Wirkung des Chinin eine rasch vorübergehende, höchstens

[1]) Archiv f. klin. Medic., von Ziemssen und Zenker. Bd. 3. S. 42.

[2]) H. v. Böck, Beobachtungen über die Kaltwasserbehandlung des Typhus. Referirt in der Allgem. medic. Centralzeitung (Berlin, red. von Dr. Rosenthal) 5. Febr. 1870. S. 124.

36 Stunden anhaltende ist. Zu entbehren ist es nicht: einmal in sehr schweren Fällen, dann in Fällen, wo man es mit widerstrebenden Patienten zu thun hat."

Und damit vergleiche man folgendes Urtheil [1]):

„Sulf. Chinin. Die Behauptung der Nutzlosigkeit bei Typhus selbst mit intermittirendem Typus ist das Resultat einer grossen Anzahl von Beobachtungen, und ein Fall, wo der Kranke in Lösung und Pillen innerhalb fünf Tagen 88 Gr. Chinin verbrauchte, ohne einen Einfluss auf Fieber oder Milztumor wahrgenommen zu haben, möge als ein exquisiter Beleg aus vielen erwähnt sein. Dagegen ist die Wirkung gegen pyämische Prozesse unleugbar, nur müssen die Dosen hochgegriffen werden."

Es wäre leicht, wissenschaftliche Zeugnisse beizubringen, welche nun ihrerseits die antipyretische Wirkung in der Pyämie nichts weniger wie „unleugbar" finden. Vor Allem bietet die Therapie des Puerperalfiebers dazu Gelegenheit. Wie da jedoch eine genügende Resorption des schwerverdaulichen neutralen schwefelsauren Chinin zu Stande kommen soll, wenn der Darm meteoristisch aufgetrieben, das Zwerchfell in die Höhe gedrängt, die Entzündung über das ganze Bauchfell verbreitet ist, bleibt schwer zu begreifen. Symptomatologie und pathologische Anatomie weisen darauf hin, dass Typhus und Puerperalfieber in Wien u. s. w. wesentlich nichts anderes sind als in München und sonstwo. Die Identität der Erscheinungen setzt voraus die des Giftes. Auch die Beziehungen des Chinin zum Organismus bleiben sich wohl aller Orten gleich; höchstens hätte man an verfälschte Präparate zu denken [2]). Hat der eine Beobachter Erfolge gesehen, der andere das Gegentheil, so ist nur zweierlei möglich: entweder sind dort die Fälle ungenau beobachtet oder ungenau statistisch verwerthet worden, oder man hat hier das Mittel in ungenügender und verkehrter Weise zur Anwendung gebracht. Es ist nicht Sache der theoretischen Forschung, darüber zu entscheiden.

[1]) Bericht der k. k. Rudolf-Stiftung in Wien vom Jahre 1868. S. 170.

[2]) Unlängst hatte ich Gelegenheit, ein zum Export für eine englische Colonie bestimmtes Präparat zu untersuchen, das als schwefelsaures Chinin bezeichnet war, aber nicht weniger als 72 pCt. Cinchonin enthielt.

II.

Prüfung der antipyretischen Thätigkeit des Alkohol in dem genannten Fieberzustande. — Chemische Analogien der Alkoholwirkung. — Verhalten beider Antipyretica zum Herzen.

Die Untersuchungen über Weingeistwirkung, welche Herrn C. Bouvier vor einiger Zeit in meinem Laboratorium beschäftigten, und die gerade auf die Beziehungen zur Bluttemperatur gerichtet waren [1]), legten es nahe, den genannten Körper auch in der erhöhten Wärmebildung zu prüfen, welche der Durchschneidung des Rückenmarks zu folgen pflegt. Selbstverständlich ging ich dabei von der Ueberzeugung aus, wie beim Chinin stehe die Vorbedingung fest, dass der Alkohol auch in nicht giftigen Dosen auf die Körperwärme erniedrigend einwirke.

Bereits in der citirten ersten Arbeit brachte C. Bouvier ältere Belege für diese Anschauung bei. Sie waren früher aus verschiedenen Gründen nicht zum Durchbruch gekommen, hauptsächlich wohl desshalb nicht, weil es bei dem Mangel genauer thermometrischer Messungen der einfachen Betrachtung doch zu paradox erschien, eine abkühlende Wirkung von dem Mittel zu erwarten, das unserem subjectiven Gefühl als ganz besonders erwärmend sich kundgibt. Die alte Anschauung, der Alkohol steigere die Blutwärme, war deshalb, wenigstens unter den deutschen Aerzten und Klinikern, die herrschende geblieben.

[1]) Pflüger's Archiv 1869. Bd. 2. S. 370—391.

Die Resultate Bouvier's haben die Sache in ihren Grundzügen vollständig geklärt. Zum ersten Mal ferner liegt hier der Beweis vor, dass auch das künstliche putride Fieber durch Weingeist niedergedrückt werden könne. Um die nehmliche Zeit war, wie sich durch weitere Publicationen bald herausstellte, das Thema der wärmevermindernden Wirkung des genannten Mittels mit dem nehmlichen Erfolg von anderen Seiten bearbeitet worden [1]). Ganz neuerdings hat P. Ruge Untersuchungen desselben Inhaltes veröffentlicht [2]).

Man konnte vielleicht noch daran denken, die Bouvier'schen Experimente am Menschen seien in ihrer Beweiskraft geschwächt durch die Möglichkeit des Einflusses der Ruhe und der normalen Schwankungen. In der zweiten Arbeit über diesen Gegenstand, die als separate Vertheidigungsschrift der ersten vorher citirten erschien, wurde auf das Klarste nachgewiesen, dass auch bei Ausschliessung jener beiden Fehlerquellen der Alkohol wie vorher temperaturerniedrigend wirkt [3]).

Beim gesunden Menschen steht gegen Abend die Temperatur am höchsten. Von etwa 2 Uhr 30 Minuten an pflegt sie bis gegen 6 Uhr langsam zu steigen. Das entspricht wenigstens der Regel; es gibt aber auch Individuen von untadelhafter Gesundheit, bei denen sie stabil bleibt. Dass sie um diese Zeit bei Gesunden jemals spontan abfällt, scheint nicht vorzukommen [4]). Nachdem

[1]) H. Zimmerberg, Ueber den Einfluss des Alkohols auf die Thätigkeit des Herzens. Inaug.-Dissert. Dorpat 1869. (Unter der Leitung von Schmiedeberg.) — O. Neumann, Ueber die Pachymeningitis bei der chronischen Alkoholvergiftung. Inaug.-Dissert. Königsberg 1869. (Unter Mitwirkung von Leyden und Jaffé.) — A. Godfrin, De l'accol, son action physiologique, ses applications thérapeutiques. Paris 1869, chez A. Delahaye). — W. Manassein, Zur Lehre vom putriden Fieber. Centralbl. f. d. med. Wissenschaften. 9. October 1869.

[2]) „Wirkung des Alkohols auf den thierischen Organismus". Dieses Archiv Bd. XLIX. S. 265. Wenn der Autor berichtet, dass bei betrunkenen Hunden der Abfall sich „über mehrere Grade C. erstreckte, in kleinen Gaben, die nicht zur Betrunkenheit führten, in der Regel nicht mehr als 0,2—0,6 C. betrug", und ferner dieses Resultat „constant" nennt, so muss S. 266 Z. 3 v. o. jedenfalls ein Druckfehler obwalten.

[3]) Ueber die Wirkung des Alkohol auf die Körpertemperatur. Bonn 1869.

[4]) Vgl. bei Wunderlich, Eigenwärme. 1868. S. 105.

durch einen viermaligen Versuch sich für die betreffende Versuchsperson das Erstere als zutreffend erwiesen hatte, wurde ebenso oft der Einfluss mässiger, nicht berauschender Dosen Weingeist geprüft. In allen acht Malen wurde vollkommene Ruhe im Bette eingehalten. Sie verhinderte in der ersten Reihe das Ansteigen um 0,1—0,2° niemals. Bei der Alkoholaufnahme erfolgte das Gegentheil, ein Abfall, welcher parallel verlief der Quantität des aufgenommenen Mittels. Er betrug

bei 25	Ccm.	Alkohol	von	98 pCt.	0,20
- 40	-	-	-	-	0,25
- 50	-	-	-	-	0,40
- 80	-	-	-	-	0,60

Es scheint das wenig; bedenkt man aber, dass die ausgebliebene Steigerung (0,1—0,2°) hinzugerechnet werden muss, dass ferner selbst ein so unbezweifeltes Antipyreticum wie das Chinin in der Gabe von 0,5—1,0 Gramm beim gesunden Menschen keinen oder nur einen sehr geringen Abfall zu Stande bringt, so wird eher das Anerkenntniss einer kräftigen Einwirkung sich herausstellen.

Mit diesen Versuchen ist dann ferner die Frage erledigt — falls dieselbe nach den mannichfachen Resultaten, die bereits vorlagen, einer weiteren und bestimmteren Erledigung überhaupt noch bedurfte —, ob auch kleine, nicht berauschende und nicht giftige Dosen Weingeist qualitativ den nehmlichen Effect wie starke Dosen darbieten. Wo die aufgenommene Menge des Weingeists eben hinreicht, um auf die Blutwärme thermometrisch erkennbar einzuwirken, da findet beim gesunden Menschen und Thier keine Erhöhung, sondern in der Regel eine, wenn auch geringe und rasch vorübergehende, Erniedrigung statt.

Wie von mir [1]) und von C. Bouvier [2]) früher ausdrücklich hervorgehoben worden war, sollte damit keineswegs gesagt sein, dass der Alkohol nun in Fieberkrankheiten des Menschen als Antipyreticum unbedingt verwendbar sein müsse. Die Abgrenzung und richtige Schätzung seines etwaigen Werthes auf diesem Boden bedarf erst erneuter und durch das Thermometer controlirter Unter-

[1]) Berliner klin. Wochenschrift. 1869. No. 31.

[2]) Pflüger's Archiv Bd. 2. S. 390.

suchungen. In einigen Mittheilungen liegen brauchbare Anfänge dazu bereits vor.

Sô berichten Ringer und Rickards, Lancet 1866, S. 208: „Numerous observations were made to ascertain the influence of alcohol on the temperature of febrile persons. To some of the patients very large quantities of alcohol were given. To a child of twelve years old eleven ounces of absolute alcohol were given on one day. From these observations the conclusion is drawn that ordinary and extraordinary quantities of alcohol cause only a slight and temporary depression of the temperature of febrile persons, and consequently alcohol cannot bring the temperature of febrile patients to that of health. But if alcohol should be indicated by the general condition of the patient, it will also to some extent act beneficially in virtue of its power to cause some diminution of the temperature of the body. In conducting this observations the following precautions were taken: the patients were kept in bed; all the conditions were kept the same; the thermometer was kept the whole time in the axilla, and the temperature was noted every few minutes. The observations were continued many hours — in some cases during the entire day."

In einer längeren, sehr sorgfältigen Abhandlung „Klinische Beobachtungen über Abdominaltyphus in England," sagt Ch. Bäumler, damals Arzt am deutschen Hospital in London, (Arch. für klin. Medicin. Bd. 3. S. 560): „Bei sehr schweren Erscheinungen der Adynamie wurde neben oder statt des Portweins Brandy (Cognac) gegeben. Nichts ist schwieriger, als über die Wirkung der Alcoholica auf einen so complicirten Zustand, wie ihn ein schwerer Typhuskranker darbietet, ein Urtheil zu fällen, eine temperaturerhöhende Wirkung kommt ihnen aber jedenfalls nicht zu."

Als Resumé aus einer Reihe klinischer Beobachtungen, die sämmtlich mit dem Thermometer controlirt worden waren, führt A. Godfrin Folgendes an (a. a. O. S. 89): „L'alcool est surtout indiqué dans les maladies fébriles aiguës, soit dans les phlegmasies comme la pneumonie, soit dans les fièvres intermittente, typhoïde et éruptive. Il agit directement en abaissant la température; c'est un antipyrétique direct . . . il doit être administré à la dose de 100 grammes par jour au moins, si l'on veut obtenir un abaissement notable de la température. Dans les cas pressés, il

faut le donner d'un seul coup, à l'état de cognac ou de rhum purs, ou en deux fois, à dix ou quinze minutes d'intervalle. En cas de calorification exagerée, comme cela se produit dans les fièvres malignes, on doit lui préférer l'hydrothérapie qui agit plus énergiquement. De nouvelles recherches sont à faire à cet égard."

Die nachstehenden Experimente sollten wie vorher für das Chinin so auch für den Alkohol lediglich die Frage erörtern, ob seine wärmeerniedrigende Wirkung hervorragend auf eine Irritation des wärmehemmenden Nervencentrums zu beziehen sei. Dass der Alkohol im ersten Stadium seiner Wirkung die Centren im Allgemeinen erregt, bedarf keines Beweises mehr. So wäre es ganz folgerichtig, zu schliessen, auch das genannte Hemmungsnervensystem sei diesem Einfluss unterworfen. Der Gang, den die experimentelle Prüfung unter Ausscheidung jenes Systems zu nehmen hatte, war im Wesen derselben wie von der bisherigen Versuchsreihe beschrieben.

VIII. Versuch.

Junger Bastardschäferhund von 11 Kilogrm. Wird nach der Operation, die ohne besondere Blutung vor sich geht, gut in Watte eingehüllt. Die Temperatur, wegen der enormen Unruhe des Thieres vorher nicht gemessen, zeigt nunmehr 37,2.

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
10 45	37,2	—	22	20	
11	36,8	120	22	22	
11 15	36,8	140	18	22	Ruhige Narkose noch v. Morphin herrührend.
11 30	37,0	140	20	22	120 Ccm. Milch von 36° vermittelst der
11 45	37,2	132	20	22	Schlundsonde.
12	37,4	140	20	22	
12 15	37,5	140	18	22	
12 30	37,7	140	20	24	Um 12 Uhr 32 Min. Alkohol 20 Ccm.
12 37	38,3	—	25	24	mit ebensoviel Wasser.
12 42	38,5	—	—	22	
12 50	38,7	—	—	22	Um 12 Uhr 50 Min. Alkohol wie vorher.
1	38,9	154	29	23	
1 10	38,95	148	22	21	
1 20	38,95	150	23	21	
1 30	38,8	156	24	21	
1 40	38,7	144	35	21	Puls sehr kräftig.
1 50	38,6	154	33	22	
2	38,6	144	33	21	
2 10	38,6	138	26	22	
2 20	38,6	144	34	22	
2 30	38,65	142	22	23	

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
2 40	38,7	146	30	22	
2 50	38,8	142	22	23	
3	38,9	146	28	21	
3 10	38,95	160	36	21	
3 20	38,95	146	28	21	
3 30	38,95	144	36	21	120 Ccm. warme Milch.
3 45	38,7	152	29	21	
4	38,65	150	40	21	
4 15	38,6	152	42	22	
4 30	38,9	148	42	22	
4 45	39,1	152	24	22	
5	39,3	150	24	22	Der bisherige Rausch wird durch lautes Stöhnen unterbrochen, das bis nach einer weiteren Gabe Alkohol von 20 Ccm. mit 40 Wasser, die um 5 Uhr 16 Min. beigebracht wird, andauert. Es tritt Schnarchen ein.
5 15	39,5	160	24	22	
5 30	39,6	178	48	22	
5 45	39,8	150	34	22	
6	40,0	—	—	—	
6 15	40,0	162	62	21	Um 6 Uhr die nehmliche Gabe Alkohol.
6 30	40,0	167	62	21	
6 45	39,5	160	62	22	
7	39,1	144	64	22	
7 15	38,8	176	60	22	
7 30	38,6	150	56	22	
7 45	38,4	160	58	22	
8	38,4	160	60	22	
8 15	37,8	148	60	22	
8 30	37,6	148	60	22	

Mit der Absicht, das Ende des Thieres rasch herbeizuführen, werden um 8 Uhr 35 Min. 50 Ccm. Alkohol mit ebensoviel Wasser injicirt. Nach etwa 20 Minuten fängt die Athmung an aussetzend und schnappend zu werden, der Herzstoss ist kaum zu fühlen. Das Thier verendet um 9 Uhr ohne eine Spur von Krampf zu zeigen. Die Blutwärme war um 9 Uhr 37,3.

Der Kasten wird geschlossen, das Thermometer ist im Rectum befestigt. Die Zimmerwärme ist 20° C. Am folgenden Morgen, nach einer milden Spätsommernacht, zeigt das Maximumthermometer 37,4.

Die Section, 14 Stunden nach dem Tode, ergibt: Bedeutende Starre. Nirgends ein Anfang der Fäulniss. Das Blut allenthalben gut geronnen. Die Leber normal, die Milz klein, fest, von heller Farbe, keine sichtbaren Follikel. — Das untere Halsmark bis auf einen kleinen mit Extravasat durchzogenen etwa ½ Linie breiten Streifen der vorderen Stränge glatt durchschnitten.

Der Verlauf dieses Versuches ist klar genug, um einer ausführlicheren Epikrise nicht zu bedürfen. Ich möchte besonders auf die gegen 12 Uhr eingetretene energische Steigerung der Temperatur hinweisen. Die erste Reaction auf den Alkohol trat etwa 30 Min. nach dessen Aufnahme ein, sie dauerte bis gegen halb fünf Uhr,

also 3½ Stunde. Im weiteren Verlauf ist besonders das Ausbleiben der gewöhnlichen postmortalen Steigerung von charakteristischer Wichtigkeit.

IX. Versuch.

Kräftiger Dachsbastard von ungefähr 11 Kilogramm. Die Normaltemperatur ist 38,6. — Wird um 10 Uhr 45 Min. in den gut erwärmten Kasten gelegt, nachdem die Operation ohne Störung eben beendet worden.

Zeit.	Temperatur.	Puls.	Athmung.	Kastenwärme.	Bemerkungen.
Uhr Min.					
11	38,1	108	16	28	
11 15	38,2	—	—	29	
11 30	38,3	—	—	28	
12	38,4	92	13	29	
12 15	38,5	—	—	29	
12 30	38,7	—	—	29	
12 45	39,0	98	—	28	Um 12 Uhr 45 Min. Alkohol 20 Ccm. mit 40 Wasser.
1	39,2	132	20	30	
1 15	39,5	—	—	29	
1 30	39,6	—	—	29	Um 1 Uhr 20 Min. Alkohol wie vorher.
1 45	39,9	—	—	29	
2	40,0	130	22	29	
2 15	40,1	—	—	29	Um 2 Uhr 20 Min. Alkohol wie vorher.
2 30	40,0	—	—	29	

Um 2 Uhr 42 Min. erfolgt der Tod genau in derselben Weise wie im Versuch VIII. Die Section der Wirbelsäule ergab vollständige Trennung des Halsmarkes dicht vor dem ersten Brustwirbel.

Der Alkohol blieb in diesem Fall ohne deutlichen Einfluss. Die Ursache davon ergibt sich aus dem Vergleich mit den übrigen Versuchen und auch mit den Erfahrungen, die bereits für das Chinin constatirt worden sind. Keines von beiden Mitteln kann seine antipyretische Kraft innerhalb der ihm möglichen Grenzen seiner Dosirung geltend machen, wenn die Vorbedingungen der Fiebererregung zu sehr überwiegen. Dies geschah hier durch die hochgradige Erwärmung des Kastens.

X. Versuch.

Weiblicher Spitz von 8 Kilogramm, dichtbehaart; 40,0 Normaltemperatur. Wird an den Hinterextremitäten und am Rumpf vollständig gelähmt um 11 Uhr 15 Min. in den Wärmekasten gebracht.

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
11 30	39,4	—	—	22	Sehr unruhig. Krämpfe der Vorderextremität.
11 45	39,0	—	—	25	Krampfhaftes Athmen. Muskelzucken im Gesicht.
12	39,1	—	—	25	
12 15	39,2	160	36	24	Um 12 Uhr 20 Min. Alkohol 15 Ccm. mit 35 Wasser. Das Thier wird bald darnach ganz ruhig.
12 30	39,1	—	—	24	
12 45	39,4	—	—	24	
1	39,3	196	26	23	Um 12 Uhr 50 Min. Alkohol wie vorher.
1 15	39,2	—	—	24	
1 30	39,1	160	21	24	
1 45	38,9	—	—	23	
2	38,8	—	—	23	
2 15	38,7	208	24	24	
2 40	38,6	—	—	23	
2 50	38,4	—	—	23	
3	38,5	—	—	23	Um 3 Uhr Alkohol wie vorher.
3 15	38,4	—	—	25	
3 30	38,4	160	28	23	
3 45	38,5	—	—	24	
4	38,6	—	—	24	
4 15	38,5	—	—	24	
4 30	38,4	120	32	23	
4 45	38,5	—	—	23	
5 10	39,0	—	—	23	
5 30	39,0	—	—	23	
5 45	38,7	—	—	23	
6	38,5	—	—	23	
6 15	38,3	—	—	24	
6 30	38,4	—	—	23	
6 45	38,4	—	—	25	
7	38,1	—	—	25	
7 15	37,9	144	32	24	Der Puls und Herzstoss deutlich und kräftig, ebenso die Respiration tief und regelmässig.
7 30	37,4	—	—	25	
7 45	37,1	—	—	25	
8	36,6	144	22	25	
8 15	36,2	—	—	25	
8 30	36,2	—	—	25	
8 45	36,0	—	—	25	
9	35,8	146	18	25	
9 15	35,7	—	—	25	
9 30	35,6	144	18	25	100 Ccm. warme Milch mit der Schlundsonde beigebracht.
9 45	35,7	142	18	22	
10	35,5	—	—	23	
10 15	35,5	—	—	24	
10 30	35,3	144	20	25	Puls und Respiration von derselben Beschaffenheit wie vor 3½ Stunden.
10 45	35,0	140	20	25	
11	35,1	144	20	25	
11 15	35,1	144	22	25	
11 30	35,3	144	22	25	

Das Thier ist sehr ruhig, halb schlafend. Nachdem es gut mit Watte bedeckt worden ist, wird das Maximumthermometer eingelegt und der Kasten bis auf die

Ventilationsöffnungen geschlossen. Die Nacht ist sehr milde, das Zimmer nach der Sonnenseite gelegen anhaltend hoch temperirt.

Am folgenden Morgen liegt der Hund ruhig im Kasten und ist vollständig wach. Seine Maximalwärme im Rectum während der Nacht war 36,3. — Die weitere Beobachtung ergibt nun:

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
8 30	36,3	—	—	20	Erhält 80 Ccm. warme Milch. Die Watte, welche am Abend aufgelegt worden war, bleibt.
9 30	36,1	142	24	27	
9 45	36,4	—	—	27	
10	37,3	144	28	26	
10 15	37,6	—	—	27	
10 30	37,9	—	—	25	
10 45	38,1	—	—	23	
11	38,3	142	24	26	
11 15	38,6	—	—	26	
11 30	38,8	—	—	25	
11 45	38,9	—	—	26	
12	39,1	146	32	26	
12 15	39,2	—	—	27	
12 30	39,4	—	—	27	
12 45	39,7	—	—	25	
1	39,7	146	28	24	
1 15	39,6	—	—	25	
1 30	39,6	—	—	25	
1 45	39,7	—	—	25	
2	39,8	150	28	25	
2 15	39,9	—	—	25	
2 30	40,0	—	—	25	Um 2 Uhr 30 Min. Alkohol 20 Ccm. mit 50 Wasser.
2 45	40,3	—	—	29	
3	40,7	—	56	28	Der Puls ist wegen heftiger Unruhe des Oberkörpers nicht zu zählen.
3 15	40,9	—	—	29	
3 30	41,5	176	104	30	Um 3 Uhr 10 Min. Alkohol wie vorher.
3 45	41,9	—	—	28	
4	42,1	—	176	30	Um 4 Uhr 5 Min. ebenso.
4 15	42,2	—	—	27	
4 30	42,1	—	—	27	

Um 4 Uhr 33 Min. erfolgt der Tod, ohne Krämpfe, wie in den vorhergehenden Versuchen. Das Maximumthermometer wird eingelegt, durch ein ungünstiges Zusammentreffen verschiedener Umstände jedoch die spätere Notirung verhindert.

Die Section, 1¼ Stunden später, ergab: Das Halsmark links vollständig durchschnitten, rechts zu ganz Dreiviertel. Der nicht durchschnittene kleine Theil, der dem Rücken des hier verwendeten einschneidigen Scalpells entspricht, ist gequetscht.

Wenn irgendwo so liegt es in diesem Versuche klar zu Tage, dass es keiner tödtlichen Gaben Weingeist bedarf, um auch bei Ausschluss des Wärmehemmungscentrums die beginnende Fieberhitze herabzudrücken. Am ersten Tage war die letzte Dosis um 3 Uhr gegeben worden. Es trat ein Rausch ein, der bis zum Anfang des folgenden Tages dauerte, dann aber vollständiger Besinnlichkeit wich. Auch das Gleichbleiben von Puls und Athmung durch ganze Stunden ist bemerkenswerth, ebenso die Nutzlosigkeit des Alkohol zu der Zeit, wo die Energie des neu entwickelten Fiebers dies seinem Höhepunkt näherte (2 Uhr 30 Min. am zweiten Tag).

Ich suchte nun die Wirkung des Chinin mit der des Alkohol zu verbinden, weil sich möglicherweise von der gleichzeitigen Thätigkeit beider Körper ein vermehrter Effect erwarten liess, ähnlich so, wie dies von der Anwendung kühler Bäder und intercurrirender Chinindarreichung bekannt ist.

XI. Versuch.

Bastardrattenfänger von 6 Kilogramm und 38,5 Normaltemperatur. Bleibt nach der Operation mit Watte bedeckt gegen 30 Min. im unerwärmten Kasten liegen. Um 11 Uhr 15 Min. wird mit gelinder Erwärmung begonnen.

Zeit.		Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr	Min.					
11	30	35,7	—	20	22	
11	45	35,3	—	—	25	Die Watte wird auch im Kasten aufgelegt.
12		36,9	—	—	25	
12	15	37,3	—	—	24	
12	30	37,4	120	16	24	
12	45	38,0	—	—	24	
1		38,2	—	—	23	
1	15	38,5	—	—	24	
1	30	39,0	84	17	24	
1	45	39,0	—	—	23	
2		38,9	—	—	23	
2	15	39,2	—	—	25	
2	30	39,5	96	22	24	Um 2 Uhr 33 Min. Chinin 0,2 in 10 Ccm. Alkohol und 30 Wasser.
2	45	39,2	—	—	23	
3		39,5	—	—	23	
3	15	39,8	—	—	25	
3	30	39,9	116	48	23	Um 3 Uhr 25 Min. ganz ebenso.
3	45	40,2	—	—	24	
4		40,5	—	—	24	Um 4 Uhr Alkohol 20 Ccm. mit 50 Wasser.
4	15	39,9	—	—	24	
4	30	39,5	126	88	23	
4	45	39,3	—	—	23	
5		39,1	—	—	23	
5	30	38,6	—	—	23	
5	45	38,6	—	—	23	
6		38,6	—	—	24	

Um 6 Uhr 5 Min. erfolgt das Ende unter den Erscheinungen der directen Herz- und Respirationslähmung, ganz ohne Krämpfe. Das Thermometer zeigte 10 Minuten später 38,3 und stieg in den nächsten 4 Stunden nicht höher.

Die Section (nach 4 Stunden) ergab totale Trennung des untersten Halsmarkes. — Das Herz weit in der Diastole. Ueberall Gerinnsel. Die Milz derb.

Die antipyretische Wirkung tritt hier nur so vorübergehend auf, dass daraus allein sich kein Anhalt für eine Beurtheilung ihres Zustandekommens ergeben würde; denn der von 4 Uhr beginnende starke Abfall kann nicht dahin gerechnet werden, weil er wahrscheinlich nur der Anfang tödtlicher Lähmung war. Ein besonders gearteter Einfluss der gleichzeitigen Verabreichung beider Arzneikörper ist nicht vorhanden. Von positivem Werth ist das Ausbleiben irgend einer postmortalen Temperatursteigerung.

XII. Versuch.

Kräftiger dickwolliger Spitz. Erhält nur Chloroform; kein Morphin. Durchschneidung des Halsmarkes unter dem letzten Wirbel. Geringe Blutung. Temperatur vor der Operation, nachdem schon lange fest aufgebunden: 39,4. Gleich nachher 38,1.

Zeit.		Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr	Min.					
10	40	38,1	—	—	17	Gut mit Watte bedeckt.
11		38,1	—	10	22	Sehr unruhig mit Kopf und Vorderfüssen.
11	15	38,1	—	12	22	Beisst wild um sich. An Rumpf u. Hinterfüssen total gelähmt. — Um 11 Uhr 20 Min. subcutan 8 Ccm. Alkohol von 98 pCt. — Um 11 Uhr 45 Min. 15 Ccm. Alkohol mit 25 Wasser d. d. Magen, ebenso um 11 Uhr 55 Min.
11	35	38,7	—	—	20	
12	5	39,0	—	15	23	
12	15	39,1	150	17	23	Fest schlafend.
12	30	39,2	150	20	22	Um 12 Uhr 20 Min. Alkohol 20 Ccm. mit 20 Wasser.
12	45	39,2	150	22	22	
1		39,3	160	34	23	Athemzüge seicht aber regelmässig. Puls stets kräftig.
1	15	39,0	168	36	24	
1	30	39,1	158	40	23	
1	45	39,2	172	39	24	
2		39,2	160	43	24	
2	15	39,3	160	50	24	
2	30	39,4	172	50	23	
2	45	39,4	160	52	24	
3		39,4	160	56	24	
3	15	39,4	160	50	23	
3	30	39,4	160	54	23	
3	45	39,4	166	45	24	
4		39,6	166	48	24	
4	15	39,5	160	52	23	
4	30	39,6	160	54	24	
4	45	39,7	160	52	24	
5		39,8	170	60	25	Andauerd in tiefem Schlaf. Um 5 Uhr werden 30 Ccm. Wasser von 10° C. in den Magen gespritzt. — Seit 5 Uhr 15 Min. wird
5	15	39,8	168	60	23	
5	30	39,8	176	46	25	

Zeit.	Temp.	Puls.	Resp.	Kasten.	Bemerkungen.
Uhr Min.					
5 45	39,9	160	30	24	die Flamme unter dem Kasten entfernt, da sich zeigt, dass die Wärmeausstrahlung des Thieres hinreicht, um die Temperatur des Kastens zwischen 23 und 25 zu halten. Die Zimmerwärme ist 19—20° C.
6	40,0	162	60	23	
6 15	40,0	160	60	23	
6 30	40,1	160	70	23	
6 45	40,2	160	65	23	
7	40,1	160	70	23	
7 15	40,0	160	40	23	
7 30	40,0	160	40	23	
7 45	40,1	160	56	23	
8	40,0	176	36	23	
8 15	39,9	160	70	23	
8 30	39,9	160	50	23	
8 45	39,8	160	56	21	Um 8 Uhr 45 Min. Sinken der Kastenwärme auf 21°, worauf vorübergehende Erwärmung mit der Flamme.
9	39,8	160	66	24	
9 15	39,9	168	70	24	
9 30	40,0	168	66	24	
9 45	40,2	160	56	23	Um 9 Uhr 45 Min. Erbrechen wässriger nicht nach Alkohol riechender Flüssigkeit. — Injection von 40 Ccm. Wasser mit 2 Tr. conc. Salzsäure.
10	40,1	160	70	22	
10 15	40,0	160	70	22	Um 10 Uhr 15 Min. wurde die Flamme für einige Minuten untergesetzt, woraus dann die Steigerung auf 25° resultirte.
10 30	40,1	160	30	25	
10 45	40,3	160	70	25	
11	40,3	168	60	23	Das Thier fortwährend sehr ruhig. Die Augenlider reagiren beim Berühren sehr gut, auch der Schwanz beim Einführen des Thermometers. Letzteres hatte vorher stundenlang nicht stattgefunden.
11 15	40,5	168	56	24	
11 30	40,5	160	60	22	
					Um 11 Uhr 20 Min. Injection von 20 Ccm. Alkohol mit 60 Wasser, was aber einige Minuten nachher zusammen mit bräunlichem Schleim wieder erbrochen wurde.

Der Kasten wurde an der einen Seite offen etwa 3 Meter von einem mässig erwärmten und für die ganze Nacht mit Brennmaterial versehenen Ofen entfernt aufgestellt. Das betreffende Zimmer selbst ist inmitten anderer gelegen und besitzt eine ziemlich constante Temperatur. In dem Bectum wurde ein Maximumthermometer befestigt.

Am folgenden Morgen 6 Uhr war das Thier noch am Leben. Kurz nachher machte es einige heftige krampfhafte Bewegungen und verendete. Die Temperatur des Kastens betrug etwas später 17° C. Das Maximumthermometer zeigte um 8 Uhr 41,6° C.

Die Section, um 10 Uhr angestellt, ergab als für den Versuch bemerkenswerth: Allgemeine Todtenstarre. Das Halsmark an besagter Stelle zu Dreiviertel ganz getrennt, das andere Viertel zum Theil zerquetscht (die Operation war auch diesmal mit einem einschneidigen Scalpell ausgeführt worden, die nicht durchschnittene Partie entspricht der stumpfen Seite). — Die Magenschleimhaut geschwellt, hyperämisch, stark gerunzelt, vielfach ecchymotisch, allenthalben mit glasigem, neutral

reagirendem Schleim bedeckt. Der Inhalt besteht aus 52 Ccm. braungefärbter zäher Flüssigkeit. — Die Blase strotzend gefüllt mit trübem, stark riechendem und intensiv sauer reagirendem Harn.

Es schien mir wichtig den Harn auf die Anwesenheit von Weingeist zu prüfen. Zur Entfernung etwaiger Kohlensäure wurde ein Theil mit gebranntem Kalk versetzt, filtrirt und in das Geisler'sche Vaporimeter gebracht. Das Instrument zeigte keine Spur von dem gesuchten Körper an, während eine Controle — ein kleiner Tropfen Weingeist dem 3 Ccm. haltenden Recipienten zugesetzt — sich sofort mit 0,7 Volumprocent auswies. Die grössere Hälfte des Harns mischte ich mit verdünnter Schwefelsäure und destillirte. Die zuerst übergehenden Cubikcentimeter wurden nun nach zweierlei Methoden auf Alkohol untersucht; mit doppeltchromsauerm Kali und concentrirter Schwefelsäure und ferner nach Lieben mit freiem Jod und Natronlauge. Beidemal trat eine deutliche aber trotz der grossen Concentration des untersuchten Objectes schwache Reaction ein, Bildung von grünem Chromoxyd in dem einen, von gelbem, krystallinischem Jodoform in dem anderen Fall. Diese Reactionen lassen sich jedoch, wie bekannt, wenn nicht ausserdem noch besondere Maassnahmen getroffen werden, auf anderweitige organische Verbindungen beziehen. Es hatte hier keinen Zweck, das weiter zu verfolgen. Jedenfalls war höchstens nur eine schwache Spur Alkohol vorhanden.

Der Mageninhalt wurde mit Kalk versetzt, filtrirt und ebenfalls im Vaporimeter untersucht. Es ergab sich ein Alkoholgehalt von 0,51 Volumprocent, was einem Gehalt des Ganzen von etwas über 0,2 Ccm. entspricht.

Das Ergebniss dieses Versuches ist abermals klar genug, wenn auch nicht in gleichem Maasse, wie beim vorvorigen Mal. Unter sehr günstigen Bedingungen zur Wärmebildung von 11 Uhr 15 Min. an steiles Aufsteigen der Curve, Einschränken desselben durch die ersten Dosen Weingeist, Herabdrücken durch die folgenden. Die Gesammtdosis war bedeutend, aber in keiner Weise das Leben bedrohend. Das zeigt besonders der gleichmässige Gang des Pulses während der ganzen Versuchszeit, der auch in seiner Qualität keine merkbare Abweichung darbot, sowie ferner das Aufhören des Rausches in den letzten Stunden der Beobachtung. Das Verenden unter Krämpfen (man vgl. Versuch VIII, IX, X und XI) sowie das späte Verenden selbst, erst 18 Stunden nach der Aufnahme des Alkohol, weisen ebenfalls darauf hin, dass das Eintreten des Todes mit dem Alkohol nichts zu thun hat. Im Gegentheil, wenn man die Versuche der ganzen Reihe mit No. XI und XII vergleicht, so wird man eher zu der Anschauung gedrängt werden, dass die lange Dauer des Lebens nach der eingreifenden Operation in diesen beiden Malen viel eher gerade der Einwirkung des Alkohol zuzuschreiben ist.

Die äusserst geringe Menge, man kann wohl sagen die Abwesenheit des Alkohol im Harn deutet darauf hin, dass derselbe im Blut rasch zerlegt worden war. Im Magen fand sich nur mehr ein Minimum vor, das wohl von der letzten zum grössten Theil sofort erbrochenen Dosis herrührte, die wegen des hochgradigen Katarrhes nicht resorbirt wurde. Wie energisch der Weingeist vom Blut zerlegt wird, ist mannichfach gezeigt worden. So berichtet Frerichs: „Ein erwachsener, an Spirituosen gewöhnter Mann nahm im Verlauf von einer Viertelstunde 200 Ccm. Alkohol mit 100 Wasser. Die ausgeathmete Luft wurde zwei Stunden lang in einem Kühlapparat condensirt. Sie enthielt nur Spuren von Alkohol, der gelassene Harn gar keinen [1]). Und Buchheim hat dargethan, dass frisches gesundes Blut zugesetzten Weingeist in nicht unerheblicher Quantität schon ausserhalb des Körpers so verändert, dass man nur mehr einen Theil davon wieder auffinden kann [2]). Eine Steigerung solcher Effecte bei Fieberwärme und innerhalb des Kreislaufes kann deshalb nicht auffallend erscheinen. Zum Theil wohl erklärt sich hieraus die bekannte Toleranz Schwerfieberkranker gegen starke Dosen Weingeist, die von einer grossen Zahl Beobachter übereinstimmend berichtet wird [3]).

[1]) Handwörterbuch der Physiologie. Bd. 3. S. 808.

[2]) Archiv der Heilkunde. 1866. S. 124 (Unter Betheiligung von Schulinus und Sulzynski).

[3]) Vgl. Anstie, Stimulants and Narcotics, their mutual relations. London 1864. p. 439. Besonders wichtig für die Wirkung des Alkohol ist hier eine Krankengeschichte über „severe erysipelas of the head and face“, in welchem „with obvious benefit and without the least symptom of intoxication“ einem 43jährigen Mann täglich 24 Unzen Cognac gegeben wurden. Der Fall dreht sich hauptsächlich um die Frage nach der Anwesenheit des Alkohol im Harn unter solchen Umständen. Nur ein kleiner Theil wurde wiedergefunden.

Als allgemeines Resultat dieser Versuche ergibt sich für den Alkohol noch deutlicher wie früher für das Chinin, dass seine antipyretische Wirkung zu Stande kommen kann bei Ausschluss des moderirenden Wärmecentrums, dass also jedenfalls ein auf dieses ausgeübter Reiz nicht die nothwendige Vorbedingung seiner Thätigkeit ist.

Eingangs dieser Arbeit habe ich Gründe besprochen, welche bei der Wirkung des Chinin in weitem Umfang auf directe chemische Beeinflussung der Oxydation in Säften und Geweben hinweisen. Wie es scheint, liegt dieser Hinweis für den Alkohol noch klarer vor. Knüpfen wir zunächst an unsere Versuchsreihe an.

Wo man bisher bei dem Normalversuch (I und II) oder auch in entsprechenden klinischen Fällen die postmortale Wärme gemessen hat, stets fand sich eine beträchtliche Steigerung. In den Alkoholversuchen war davon zweimal keine Spur, eher das Gegentheil zu gewahren (zweimal wurde nicht darauf geachtet, einmal war zur Zeit des Verendens die Weingeistwirkung längst vorbei). Auch beim Chinin sahen wir diese Wärmehemmung, wenn auch weniger energisch.

Diese Zahl von beweisenden Beispielen ist allerdings nicht gross, der Unterschied aber ist beidemal (Vers. VIII und XI) so schlagend, dass man wohl annehmen darf, man habe es hier mit keinem Zufall zu thun. Ich theile die experimentell begründete Ansicht von Ackermann [1]) und von Valentin [2]), dass die postmortale Wärmebildung vor Allem aus der Fortdauer der vitalen wärmebildenden Prozesse auch nach Aufhören des Herzschlages entspringe, dass gerade in Folge verminderter Wärmeabgabe, besonders durch die Haut, die Production noch für einige Zeit ein Uebergewicht geltend mache. Fand nun ungeachtet aller günstigen Bedingungen in VIII und XI dies nicht statt, so lässt sich sein Ausbleiben nur einer directen Einwirkung des noch vorhandenen Weingeists oder seiner Verbrennungsproducte [3]) auf die wärmebildenden Vorgänge zuschreiben. Das moderirende Wärmecentrum war von vornherein ausgeschlossen [4]); das immer noch sehr hypothetische wärmeer-

[1]) Archiv f. klin. Med. Bd. 2. S. 359. [2]) Daselbst Bd. 2. S. 200.

[3]) Besonders an die Kohlensäure wird man gemäss den Resultaten von Kerner, Pflüger's Archiv Bd. 2. S. 150 denken müssen.

[4]) Der etwaige Einwand, das Rückenmark sei nicht immer ganz durchschnitten gewesen, würde schon deshalb hinfällig sein, weil in Bezug auf die Entstehung des Fiebers stets ein voller Effect vorhanden war.

zeugende Nervencentrum könnte nicht wohl anders als nur während des Lebens activ gedacht werden; von Herabsetzung des Druckes im arteriellen System, welche wie wir später hören, die Temperatur wahrscheinlich ändert, kann in einem todten Körper natürlich keine Rede mehr sein. Es bleibt nach dem jetzigen Stand unserer Kenntnisse einzig die Behinderung chemischer Vorgänge durch den Alkohol unmittelbar übrig.

Anderweitige Thatsachen bieten hierfür manchen positiven Anhalt. Ich habe früher bereits auf die eudiometrischen Untersuchungen von G. Harley hingewiesen. Unter mehreren anderen Stoffen prüfte er auch den Alkohol in seiner Beziehung zum Blut und den respiratorischen Gasen und fand, dass ihm die Eigenschaft zukommt, im Blut die Bildung von Kohlensäure und die Aufnahme von atmosphärischem Sauerstoff beträchtlich herabzusetzen. Das Resultat eines Versuches, mit dem, wie Harley angibt, eine Reihe anderer übereinstimmt, zeigt dies auf das Deutlichste[1]). Ich übergehe hier die Anordnung der Experimente, weil ich dieselbe schon früher (dieses Archiv Bd. XLVI., S. 156) mitgetheilt habe[2]). Hier nur die gefundenen Zahlen:

Luft mit Ochsenblut geschüttelt zeigte nach 24 Stunden an Procenten:

$$O = 10,58 \qquad CO_2 = 3,33 \qquad N = 86,09;$$

mit dem gleichen Blut, worin 5 pCt. Alkohol, ebenso behandelt:

$$O = 16,59 \qquad CO_2 = 2,38 \qquad N = 81,03.$$

Es ist nicht zu erwarten, dass lebendes Blut sich viel anders verhalte, um so weniger, als wir sehen, dass der lebende Gesammtorganismus nach Alkoholaufnahme ganz dasselbe Endresultat leistet.

Gleich Prout hat auch Vierordt gefunden, dass die Kohlensäureexcretion selbst nach mässigem Genuss von spirituosen Getränken absolut und relativ vermindert wird. Es wurde dabei auch die Erfahrung bestätigt, dass die während der Verdauung eintretende Vermehrung der Kohlensäureexcretion durch Spirituosen sich erheblich beschränkt zeigt[3]). Sodann hat Böcker das Nehmliche dargethan. Der Alkohol verhindert nach ihm, und zwar in durchaus nicht giftigen Dosen, die relative wie absolute Menge der ausgeathmeten Kohlensäure „in sehr bedeutendem Grade" [1]). Die ausführlichen Versuche von W. A. Hammond hatten das gleiche Ergebniss [2]). Ausser für den Harnstoff weisen sie auch für die Harnsäure und andere Excrete eine Verminderung nach. In neuerer Zeit hat Berg in Dorpat auf Vogel's Veranlassung dieselbe Frage bearbeitet. Nach ihm wird bei Alkoholgenuss in der Zeiteinheit, falls die Tiefe und Frequenz der Athemzüge dem Athembedürfniss überlassen bleibt, weniger an relativer CO_2 durch die Lungen ausgeschieden, als wenn kein Alkohol genossen wurde. Die absolute CO_2-Menge steigt zwar zuerst, fällt dann und steigt später wiederum. Das erste Steigen aber, vielleicht nur eine Folge der vorübergehend erhöhten Pulsfrequenz, geht rasch vorüber, das Stadium des absoluten Sinkens hält mehrere Stunden an, und das Wiederansteigen ist einfach der Effect des nach Verbrennung des Alkohol wieder hergestellten Normalzustandes [3]).

[1]) Vgl. in der Originalarbeit S. 689.

[2]) Zeile 3 v. u. muss es statt 100 Theilen 100 Volumprocenten heissen.

[3]) Vgl. Lehmann, Physiol. Chemie. 1853. Bd. 3. S. 317.

Vom Harnstoff, diesem verhältnissmässig so sauerstoffreichen Körper, gilt dasselbe wie von der Kohlensäure. Seine Bildung wird durch Mischen des kreisenden Blutes mit mässigen, nicht vergiftenden Gaben Weingeist um ein Auffallendes vermindert, was schon Böcker und Hammond in den eben citirten Untersuchungen dargethan. Mit dieser Verminderung zweier Hauptproducte der thierischen Verbrennung geht eine Verlangsamung des gesammten Stoffwechsels einher, deren Ausdruck das geringere Nahrungsbedürfniss und die stärkere Fettansammlung in den Geweben ist.

Weiter sehr geeignet, um uns über die Beziehungen des Alkohol zu den Umsetzungsprozessen im Organismus Anhaltspunkte darzubieten, ist sein Einfluss auf die Thätigkeit von Fermenten, durch welche eine Uebertragung und moleculäre Bindung des atmosphärischen Sauerstoffs zu Stande kommt oder dieser sonst in directer Weise sich bethätigt. Es liesse sich eine grosse Anzahl solcher Vorgänge heranziehen. Hier nur die Skizzirung einiger wenigen.

Der Gerbstoff zerlegt sich wie bekannt unter Aufnahme von 4 Mol. Wasser — der sauerstoffreichsten gesättigten Verbindung, die wir haben — in Gallussäure und Zucker. Nach den Untersuchungen von van Tieghem geschieht dies nur bei freiem Zutritt

[1]) Beiträge zur Heilkunde. Crefeld 1849. Bd. 1. S. 240—255.

[2]) Ref. in Schmidt's Jahrbüchern Bd. 94. S. 281.

[3]) Archiv für klinische Medicin. Bd. 6. S. 375.

der Luft und der in ihr enthaltenen Sporen [1]). Der ganze Prozess verläuft unter Bildung eines dichten Myceliums. Giesst man, wenn dies sich ordentlich entwickelt hat und die Flüssigkeit gleich vielen oxydirten organischen Körpern braun geworden ist, nunmehr ab und versetzt damit etwas Leimlösung, so erhält man keine Fällung mehr, weil alle Gerbsäure verschwunden ist. Durch anfänglichen Zusatz jedoch einer kleinen Quantität Alkohol kann man die unter Mitwirkung des Sauerstoffs geschehende Zerlegung leicht hindern. Eine solche Tanninsolution hält sich unverändert schwachgelb und klar oder ändert sich, wenn der Zusatz gegenüber den anderen Agentien, wozu in erster Reihe auch die Temperatur gehört, zu unbedeutend war, dann doch viel später als ein Controlpräparat.

Noch klarer liegen die Beziehungen bei der Essiggährung vor. Lässt man sie unter dem Einfluss des Mycoderma aceti vor sich gehen, so geschieht das bekanntlich dadurch — was auch von Liebig zugegeben wird [2]) —, dass der Pilz die Uebertragung des Sauerstoffs der Luft auf den diluirten Weingeist vermittelt. Es entsteht eine einfache Oxydation des Alkohol, deren Resultat die Säure ist. Diese Oxydation aber ist nur möglich, wenn die Menge des Wassers ganz bedeutend überwiegt. Zusatz von weiterem Alkohol hindert die bereits begonnene Verbrennung sofort.

Gewissen Vorgängen aus der Pathologie des thierischen Körpers am nächsten steht die Fäulnissgährung. Sie ist unter den gewöhnlichen Umständen stets ein Oxydationsvorgang. Der Sauerstoff der Luft nimmt an ihr einen ganz bestimmten bedingenden Antheil. Betrachten wir die Fäulniss einer bekannten thierischen Flüssigkeit, des Harns. Frischer Harn in einem ganz damit angefüllten reinen Glasgefäss kann monatelang sich unzersetzt erhalten. Ist das Glas jedoch zum Theil mit Luft gefüllt, so wird der Sauerstoff absorbirt und eine entsprechende Menge Harnstoff in Ammoniak und Kohlensäure umgewandelt. Die weitere Zersetzung hat damit eine Grenze und beginnt erst mit der Erneuerung des Sauerstoffs wieder. Mit der Zersetzung des Harnstoffs geht die Oxydation der gefärbten Harnbestandtheile parallel, und es bildet sich eine kleine Menge Essigsäure [3]). Es bedarf nur einer geringen Quantität Alkohol, um

[1]) Comptes rendus de l'Acad. des scienc. 23. Dec. 1867.
[2]) Annalen der Chemie und Pharmacie. Bd. 153. S. 145.
[3]) Ebendaselbst Bd. 153. S. 147.

auch diese Vorgänge auf das Deutlichste zu hemmen oder ganz zu verhindern. Die Verbrennung der organischen Harnbestandtheile geht dann nur erschwert oder gar nicht vor sich, selbst wenn das Gefäss überwiegend mit Luft gefüllt ist und man es täglich mit derselben schüttelt.

Die putride Verbrennung der gewöhnlichen Proteinkörper wird durch den Alkohol ebenso behindert, wie die des Harnstoffs. Es ist das eine altbekannte Thatsache. Nur wird es schwer, den Grund dafür gelten zu lassen, welchen man noch heutzutag in manchem Lehrbuch vertreten findet. Es soll die Wasserentziehung sein, denn ausgetrocknete Körper faulen nicht. Man braucht sich jedoch nur die Präparate unserer anatomischen Museen anzusehen, um die Ungenauigkeit dieser noch vielfach acceptirten Ansicht zu begreifen. Sie sind — besonders die nicht compacten unter ihnen — bei dem oft nur etwa 35procentigen Alkohol, worin sie aufbewahrt werden, wasserhaltig genug, um faulen zu können. Ebensowenig wird man bei dem in einem gewöhnlichen Urin mit dem Zusatz von etwas Alkohol aufgelösten Harnstoff daran denken wollen, er faule nun nicht, weil ihm das Wasser entzogen sei. Der eigentliche Grund ist jedesmal ein vollkommen anderer. Wie das Chinin verhindert der Alkohol die faulige Verbrennung, also die langsame Absorption des Sauerstoffs der Atmosphäre und die Bildung von Kohlensäure, Ammoniak und putriden Stoffen, weil er die Entwicklung der dazu nöthigen Fermente nicht aufkommen lässt. Die mikrochemische Beobachtung gibt darüber ohne besondere Schwierigkeit sicheren Aufschluss.

Endlich seien als Stütze meiner Auffassung noch die interessanten Untersuchungen von Schmiedeberg hier erwähnt. Für Alkohol und ein anderes die Temperatur der Warmblüter erniedrigendes Arzneimittel, das Chloroform hat er durch spectralanalytische Untersuchung Folgendes nachgewiesen: Blut mit einem der genannten Stoffe versetzt, gibt unter sonst gleichen Bedingungen bei Gegenwart eines reducirenden Körpers seinen Sauerstoff viel schwerer ab, als das nehmliche Blut in unvermischtem Zustand. Da die Rolle des Hämoglobin im lebenden Organismus hauptsächlich darin besteht, den in den Lungen aufgenommenen Sauerstoff im grossen Kreislauf an oxydirbare Blut- oder Gewebsbestandtheile abzugeben, so kann eine in der eben erwähnten Richtung stattfindende Wirkung für die im lebenden Organismus ablaufenden Prozesse nicht ohne Bedeutung sein. Eine direct chemische Thätigkeit des Weingeistes, ohne Zuthun des Herzens oder Nervensystems, wird damit auch von einer anderen Thatsache aus näher gelegt, vorausgesetzt, dass die Gesetze, wonach der Weingeist auf das Blut ausserhalb des Organismus einwirkt, wesentlich keine anderen sind, als innerhalb derselben [1]).

Manassein hat gefunden, dass unter dem Einfluss kräftiger Gaben Alkohol bei Kaninchen das künstliche putride Fieber nicht aufkam. Das nehmliche Resultat gaben Chinin und Kochsalz. Für ersteres war dies durch mich und Fickert ebenfalls gezeigt worden. Gehen wir von der durch Billroth und O. Weber vertretenen Ansicht aus, dass die künstliche Septicämie ein fermentativer Vorgang sei, so dürfte es ungezwungen erscheinen, wenn wir die Beziehungen der antiseptischen Stoffe zu den Fäulnisserregern einerseits und zu der genannten Bluterkrankung andererseits in directen Einklang bringen [2]).

Aber auch grosse Gaben (0,12 Grm.) salzsaures Morphin haben einen gleichen Effect. Bei dem ausgesprochenen Charakter dieses Alkaloides als eines ächten Nervinum, liegt der Gedanke sehr nah, dass jene Verhinderung des putriden Fiebers dennoch vom Nervensystem aus zu Stande komme, und soweit sich aus der kurzen Darstellung Manassein's schliessen lässt, scheint er selbst dieser Ansicht zu sein.

Gegen diese Auffassung wäre eine andere bisher übersehene, nicht unwichtige Thatsache einzuwenden. Das Morphin nehmlich hat, wie die meisten übrigen officinellen Alkaloide, ebenfalls antiseptische Eigenschaften. Bei den kleinen Quantitäten, worauf das Nervensystem des Menschen mit tiefer Narcose reagirt, kommen sie natürlicherweise nicht in Betracht. Anders verhält sich das beim Kaninchen. Es verträgt ungefährdet, wie Manassein bestätigt, die für sein Körpergewicht enorme Gabe von 2 Gran. In diesem Verhältniss ist das Morphin ebenso wie das in nicht viel grösserer Gabe beigebrachte Chinin wohl geeignet, antiseptisch einzuwirken.

Um das Verhalten seiner desfallsigen Eigenschaft gegenüber

[1]) St. Petersburger medic. Zeitschrift. 1868. Bd. 14. S. 92—97.

[2]) Centralblatt 9. Oct. 1869.

anderen anerkannten Antisepticis zu prüfen, machte ich drei Lösungen von gleichmässig getrocknetem Chlornatrium, salzsaurem Morphin und salzsaurem Chinin, jede von 1 : 200. In jede wurde eine gleiche Quantität Eiweisswürfel eingelegt. Die Präparate standen in dem dunkeln Schrank eines bewohnten Zimmers. Vier Wochen später zeigten sie folgenden Befund: NCl ist trübe und undurchsichtig, die Ecken und Kanten der Eiweisswürfel sind abgestumpft, der Geruch ist intensiv faulig, die Flüssigkeit wimmelt von lebhaften Vibrionen. Beim Mo sind erst die Anfänge dieser Erscheinungen vorhanden, beim Ch ebenfalls, nur noch viel schwächer wie beim Morphin. Es kommen demnach, wie mir das bereits aus früheren gelegentlichen Versuchen bekannt war, diesem Alkaloid stärkere fäulnisswidrige Eigenschaften zu als dem Chlornatrium, einem anerkannten Antisepticum, geringere freilich als dem Chinin [1]). So lange demnach die nahliegende Möglichkeit einer solchen Einwirkung in den Versuchen Manassein's nicht ausgeschlossen ist, werden wir nicht berechtigt sein, die Verhinderung der Putrescenz bei den faulig vergifteten Kaninchen als einen vom Nervensystem aus geleisteten Effect anzusehen. Einfacher vielleicht zu entscheiden wäre die Frage, wenn es sich herausstellte, dass auch beim Menschen, wo zum Hervorbringen einer tiefen Narcose nur geringe Gaben Morphin nöthig sind, diese Narcose einen deutlichen Einfluss auf das Fieber darböte.

Für ein anderes Alkaloid, das Veratrin, wird die Theorie einer vom Nervensystem aus zu Stande kommenden Wirkung durch Kocher in seiner bekannten schönen Monographie vertreten [2]). Er nimmt an, dass es eine excitirende Wirkung auf das regulatorische Wärmecentrum ausübe. Bei der Kleinheit der zur Erreichung

[1]) Ich habe bei dieser Gelegenheit auch die Carbolsäure in derselben Weise geprüft und gefunden, was zu erwarten stand, dass sie das Chinin an fäulnisswidriger Kraft überragt. Nach diesen und früheren Versuchen von mir wäre demnach eine aufsteigende Reihe der Werthigkeit folgende, wobei natürlich eine ganze Menge Zwischenglieder fehlen: Kochsalz, Eisenvitriol, arsenige Säure, chlorwasserstoffsaures Chinin, Carbolsäure, Quecksilberchlorid. Dass unter den letzten fünf das Chinin sonst für den thierischen Organismus die mildeste Wirkung besitzt und in den bei weitem am grössten Dosen gegeben werden kann, unterliegt wohl keinem Zweifel.

[2]) Behandlung der croupösen Pneumonie mit Veratrum-Präparaten. 1866. S. 79.

des Fieberabfalls nöthigen Gaben lässt sich der Analogie mit anderen Stoffen nach an eine direct chemische Beziehung zwischen Veratrin und Organismus weniger leicht denken; im Uebrigen würde eine Entscheidung darüber im Sinne der Experimente mit Durchtrennung des Rückenmarks gegenwärtig wohl möglich sein.

Dass die antipyretische Wirkung vom Chinin und vom Alkohol nicht abhängig ist von dem Wärmeregulationscentrum, ist durch unsere Versuche erwiesen; dass sie zusammenhängt mit chemischen Beziehungen beider Körper zum Stoffwechsel, wurde theils durch die innere Verbindung gewisser Thatsachen, theils durch von mir früher veröffentlichte Versuche nahe gelegt. Es bleibt noch ein wichtiger Factor zu betrachten: der Einfluss auf Druck und Geschwindigkeit des capillären Kreislaufs.

Was man über diesen Gegenstand bis jetzt weiss, ist nicht genügend, eine annähernde Antwort zu ertheilen; aus dem einfachen Grunde, weil eine experimentelle Beantwortung der Vorfrage fehlt. Gewöhnlich wird es als selbstredend vorausgesetzt, dass einer erhöhten Propulsivkraft des Herzens eine erhöhte Wärmebildung entsprechen müsse; man kann sich aber eben so ungezwungen vorstellen, dass die beschleunigte Geschwindigkeit und der erhöhte Druck in Haut und Lungen auch eine beschleunigte Wärmeabgabe zur Folge hat, dass demnach die gesteigerte Action des Herzens bereits Ausgleichbedingungen in sich trägt, wie der Organismus dies so vielfach darbietet. Ebenso würde sich umgekehrt eine noch innerhalb der Grenzen der Nichtvergiftung befindliche Depression des Herzens verhalten können. Directe mit dem Thermometer und den neueren Hülfsmitteln der Untersuchung des Kreislaufs zugleich gewonnene Beobachtungen existiren aber meines Wissens noch nicht, und so ist jene Voraussetzung zu weiteren Schlüssen noch nicht verwerthbar.

Sollte sie sich als zutreffend erweisen, so wäre ihre pharmakodynamische Anwendung ziemlich einfach. Durch die ausführlichen Untersuchungen von Briquet und Poiseuille weiss man, dass nach Einführung schon mässiger jedoch nicht zu geringer Dosen Chinin sowohl die Zahl der Pulse als die Druckkraft des linken Ventrikels abnimmt [1]). Dasselbe hat H. Zimmerberg unter

[1]) a. a. O. S. 45–86.

Schmiedeberg's Leitung für den Alkohol dargethan, und wenn auch hier betreffs der Einwirkung auf die Pulszahl die Angaben mit anderweitigen Beobachtungen nicht ganz congruent sind — wahrscheinlich weil dieser Effect unter verschiedenen Umständen an und für sich ungewöhnlich variabel ist, — so scheinen die Resultate hinsichtlich des Druckverhältnisses doch ganz regelmässig zu sein [1]).

In den vorher von mir mitgetheilten Experimenten tritt eine Einwirkung beider Arzneikörper auf den Puls (und ebenso wenig auf die Respiration) nirgendwo charakteristisch hervor; und dem entsprechend weisen klinische Beobachtungen für das Chinin nach, dass die Frequenz des Pulses sehr oft noch nicht alterirt erscheint, wenn die temperaturerniedrigende Wirkung deutlichst schon eingetreten ist [2]). Beides verhindert freilich nicht, dass dort beim Thier hier beim Menschen eine Abnahme der Spannung im arteriellen Gefässsystem vorlag, die man nur deshalb nicht gewahrte, weil man sie mit den geeigneten Mitteln nicht aufsuchte.

Bei einer experimentellen Bearbeitung der Frage wird es zuerst nöthig sein, den rein physiologischen Theil in's Auge zu fassen. In Betreff des zweiten Punktes, der Einwirkung von Chinin und Alkohol auf den linken Ventrikel gesunder Thiere könnte man sich vorläufig den Angaben der genannten Forscher anschliessen. Der dritte Theil jedoch, die Uebertragung auf pyretische Zustände, wäre nur am fiebernden Thier hämodyanamometrisch zu erledigen. Da es feststeht, dass die Antipyretica in mässigen Fieberzuständen einen Temperaturabfall viel leichter zuwege bringen, als wenn kein Fieber vorhanden ist, so würde man hier vorzüglich mit relativ kleinen Gaben arbeiten müssen. Es bleibt immerhin möglich, dass solche mit den klinischen Resultaten parallel einen Abfall der Temperatur bewirken, ohne dass, wie bei grossen Gaben constant, das Hämodynamometer einen Abfall seinerseits schon aufweist. Würden sich, die Feststellung des oben erwähnten ersten Punktes vorausgesetzt, die Dinge jedoch so gestalten, dass der Abfall der Temperatur stets mit einem Herabgehen der Quecksilbersäule zusammenfiele, so wäre wohl jeder Zweifel über die unmittelbare Beziehung zwischen der Wirkung auf das Herz und der auf die Temperatur damit ausgeschlossen.

[1]) a. a. O.
[2]) Liebermeister a. a. O. S. 61.

Ueber eine neue und wie es scheint höchst wichtige Thatsache, betreffend den Einfluss des Chinin auf eine Thätigkeit sensibeler Nerven, berichtete Heidenhain in neuester Zeit [1]). Vielleicht liessen sich seine Resultate in Verbindung mit den von mir und Bouvier angestellten Versuchen, wo das sensibele Nervensystem des ganzen Körpers mit Ausnahme von Kopf und Vorderextremitäten gelähmt war, hier näher besprechen, wenn mehr als der einfache Protocollauszug vorläge.

Die directe, vom Nervensystem unabhängige Wirkung des Chinin hat sich kaum irgendwo so deutlich gezeigt, als bei einer Krankheit, die bisher immer noch für eine Neurose gehalten wurde. Im Anschluss an die Beobachtung, welche wir Helmholtz verdanken [2]), lege ich hier eine zweite mir durch die Güte des Herrn Hofraths Frickhöffer in Bad Schwalbach zugegangene vor. Sie lautet:

„Capitain R. aus England, ein sehr kräftiger und gesunder Mann von 30 Jahren, dessen Frau ich im Lauf des Sommers 1869 hier behandelte, klagte mir eines Tages, dass er schon seit mehreren Jahren am Heufieber leide. Es stelle sich regelmässig Ende Mai bei ihm ein. Während seines Aufenthaltes dahier (es war im Juni) litt er wieder daran, jedoch in nicht heftigem Grade. Die Hauptererscheinungen waren starkes, sich täglich wiederholendes Niesen mit verstärkter flüssiger Absonderung der Nasenschleimhaut und Thränen der etwas injicirten Augen. Diese Anfälle stellten sich meist des Morgens ein und repetirten sich drei- bis viermal. Sie hinterliessen ein geringes Eingenommensein des Kopfes und eine gewisse Reizbarkeit der Augen. Eigentliches Fieber und die Gruppe der Respirationserscheinungen (nach Phöbus) fehlten.

Eingedenk der Helmholtz'schen Erfahrungen verordnete ich dem Patienten eine tägliche Einspritzung von Chinin (Gr. vj auf Unc. j). Vom dritten Tage an

[1]) Tageblatt der Naturforscher-Versammlung in Innsbruck 1869. S. 203.

[2]) Dieses Archiv Bd. XLVI. S. 100. — In einer freundlichen Besprechung meiner Untersuchungen sagt Polli (Annali di chimica etc. Gennajo 1870): „Almeno il dott. Binz avesse imitato nelle sue richerche microscopiche intorno all' azione di chinino il sagace dott. Balestra di Roma, che facendo analoghe indagini etc.“ Die Schrift des genannten Römers (Del miasma palustre) erschien 1869 und wurde mir erst 1870 bekannt. Ich finde in ihr meine mikroskopischen Untersuchungen von 1867 mit meinem Namen bereits citirt. In einem Referat über meine Arbeit in den Schmidt'schen Jahrbüchern (1869) sagt H. Köhler gelegentlich des Helmholtz'schen Beitrags: „B. schliesst aus dieser einen Beobachtung, dass Chinin u. s. w.“ ... Wer sich die Mühe gibt, die oben citirte Stelle nachzusehen, wird finden, dass ich überhaupt gar nichts schliesse. Selbst von einem offenbaren Tendenzreferat darf man doch verlangen, dass es sich nicht von den Thatsachen entfernt.

hörten die heftigen Niesanfälle gänzlich auf und stellten sich auch in den vierzehn Tagen, während deren ich den Kranken noch sah, nicht wieder ein.

In Folge unerwarteter Abreise desselben konnte meine Beobachtung sich nicht auf einen grösseren Zeitraum erstrecken. Immerhin aber ist die schnelle günstige Wirkung der Chinininjectionen auch in diesem Fall eine bemerkenswerthe Thatsache. Die mikroskopische Untersuchung des Nasensecretes hat leider nicht stattgefunden, da mir um die genannte Zeit die Musse dazu fehlte.“

Bei dieser Gelegenheit sei es gestattet, der Einwirkung des Chinin auf einen anderen specifischen Katarrh zu gedenken. Was ich früher über den günstigen Einfluss desselben bei Pertussis mitgetheilt habe [1]), muss ich auf Grund weiterer Erfahrungen aufrecht halten. Ich habe dieselben zwar nicht in einem stationären Hospital gemacht, was wohl das sicherste Resultat geben würde, aber die Reihe von Fällen, die ich ambulatorisch zu Gesicht bekam, ist doch lang und der Erfolg bei ihnen constant genug, um das Urtheil zu begründen, dass Chinin im Keuchhusten eine bedeutende Besserung herbeiführt. Sie macht sich sehr bald und deutlich durch Abnahme der Zahl und Heftigkeit der Hustanfälle geltend und bringt unter günstigen Aussenverhältnissen dieselben in wenigen Wochen ganz zum Verschwinden. Freilich kommt es auf einige Punkte in der Methode der Arzneiverordnung ganz wesentlich an. Das Chinin muss in flüssiger Form gegeben werden; die Dosirung darf nicht zu niedrig sein (wenigstens 1 : 100, wovon kleinen Kindern 6—8 Mal in 24 Stunden einen Theelöffel, grösseren einen halben Esslöffel voll zu geben); die Lösung darf keine einhüllenden Stoffe wie Syrupe, Succus Liquiritiae und Gummi enthalten; und sie darf kein Erbrechen hervorrufen — was sie bei unüberwindlichem Widerwillen gegen den bitteren Geschmack oder in Folge zu kräftiger Einwirkung auf das Gehirn zuweilen thut —, weil die Würgbewegungen und der Mageninhalt das im Pharynx örtlich wirkende Mittel verdrängen und fortspülen. Unter Beobachtung dieser Punkte würde wahrscheinlich auch Henoch bessere Resultate erhalten haben [2]). In wie entschiedener Weise eine nicht zu verdünnte Chininsolution auf die Schleimelemente zerlegend wirkt, zeigt der einfache mikro-chemische Versuch. Der Verlauf der Pertussistherapie durch Chinin ist im Grunde kein anderer, als wir ihn von den Reinigungsgasen der Gasfabriken, von den ätherischen Oelen, dem

[1]) Ebendaselbst S. 104.

[2]) Jahresbericht. Berlin 1869. Bd. 2. S. 654.

Tannin und Alaun und von sonstigen concentrirten Salzlösungen gewahren, vorausgesetzt, dass auch diese Dinge im zweckentsprechender Weise angewendet werden können. Vermöge der specifischen Giftigkeit des Chinin für die Schleimgebilde, welche man doch wohl als die Träger des Infectionsstoffes anzusehen hat, ist der Einfluss desselben bei gleicher Gabe jedoch viel energischer. Für die Verdauungsorgane und den Gesammtorganismus ist es in der angegebenen Dosirung und in Form eines leicht löslichen Salzes unschädlich, und so ermöglicht der Schlingact, das örtlich wirkende Desinficiens wie beim Heufieber direct mit der inficirten Schleimhaut in Berührung zu bringen. Wo man, bei älteren Kindern, mit Inhalationen oder noch besser Bepinselungen von Tannin u. s. w. in concentrirter Form die Gegend der Epiglottis unmittelbar treffen kann, gelangt man natürlich noch sicherer zum Ziele. —

20. März 1870.

II.—Epidemiological Conclusions and Suggestions. By Gavin Milroy, M.D., F.R.C.P., Vice-President of the Epidemiological Society, &c.

I. All who have attended to the history of epidemic diseases must be surprised to find how little we have yet really learned of their attributes or qualities as objects of physiographical research, and how small and uncertain has been the advance from one age to another of anything like sure knowledge respecting them. No other branch of natural science has been so stationary and unprogressive, and in none can its votaries point to fewer established truths or demonstrated general principles. The accumulations of experience do not seem to have rendered the conclusions of epidemiology at all more stable or steadfast. What has been confidently asserted one year is often contradicted a few years later, and doctrines accepted at one time are rejected at another, to be again, perhaps, brought forward with favour, and to be again repudiated or neglected.

"Our little systems have their day,
They have their day and cease to be."

Whence all this fluctuation and uncertainty? and how comes it there should be a ceaseless conflict of opinions and opposition of views in respect of most epidemiological questions? Is it because the subject matter is intrinsically so very intricate and obscure, and the phenomena with which it has to deal are so fleeting and variable as to defy all attempts to systematically note or to classify and arrange them, and thus to frustrate any reasonable hope of ever explaining their nature, and of determining their relations either to each other, or to other cosmical conditions or events? Or, is it because the mode hitherto of investigating this branch of natural knowledge has been itself at fault, and that the chief cause of failure in the past lies less in the essential difficulty of the problems to be solved than in the defective or improper method of our seeking to effect their solution?

Both reasons have had to do with the result. The general subject of epidemiology is indubitably mysterious and complex from the very nature of the phenomena and relations to be investigated; and then the plan ordinarily followed in endeavouring to interpret them has certainly not been the one which is recognised to be indispensable to the successful study of every other branch of physical inquiry. All will admit that the investigation of the attributes and actions of living bodies is necessarily more difficult than that of the qualities and properties of inanimate matter; and to this must be added that the investigation of the phenomena of life when disordered from disease is more difficult and complex than when it is healthy and normal. The difficulty becomes greater still when the disease is of only occasional occurrence, and its invasions are of merely temporary duration. It is only now and then that epidemics appear and can be watched. Moreover, different epidemics of the same distemper exhibit, under what seem to be similar conditions, signal differences in their general manifestations in various ways; and even in respect of its ordinary natural attributes or qualities—as, for example, of the period of its incubation, the activity of its power of propagation and diffusion, &c.—there is often much variableness and a remarkable want of congruity. The difficulties thence arising are not a little increased by the circumstance that many epidemiological phenomena and events are the products not of a single determinate cause, but rather of the combined operation of several concurrent elements or factors, each of which is liable to vary in potency at different times, and under different conditions and combinations.

If then epidemiological inquiries are from their very nature the very reverse of being simple and easy, it is obviously the more incumbent that the method adopted in exploring and interpreting the occurrences and questions with which they deal be such as invariably to insure the greatest amount of probability at least, if complete certitude is not always attainable. There is but one method of scientific research that can be successfully used in our endeavours to explain the phenomena taking place around us in all other departments of natural knowledge, and that, it is well known, is the slow but certain one of inductive investigation. What, therefore, has been the instrument of achieving such remarkable and often unlooked-for successes in their case, may reasonably be expected to lead to equally valuable results in respect of epidemiology, if we will but be content to submit to its requirements and to obey its precepts. A good deal of misconception often exists as to the exact meaning of the term 'induction' and of what it exacts at the hand of the student. As used by Bacon and his disciples, it is not equivalent to 'inference,' as is sometimes imagined, and as, indeed, the French employ the word; and it must be carefully distinguished from mere 'deduction.' In the inductive process, to be rightly pursued, three acts or steps have to be gone through. There is *first* the collection of a number of facts or instances, which have been carefully observed and accurately recorded. All data before being accepted must be thoroughly authenticated, and free from doubt; they must, moreover, be duly circumstantial and detailed. The bare register of phenomena or events without any notice of the attendant conditions is rarely of much use; while uncertified, or partly conjectural, statements respecting them are generally worthless, and oftener mislead than

otherwise, when we come to reason about their causation or connection. Induction refuses to admit any datum, or to accept any statement, on hearsay or mere authority; and, in short, declines all evidence whatever except that of direct experiment or observation. It never rejects or seeks to set aside any evidence because this seems to be opposed to a doctrine, or theory, we happen to have in hand at the time; for it is quite as important to collect the negative as the affirmative evidence in the elucidation of the truth. It is only when we have gathered a due store of authentic facts, that we can proceed to the *second* step, that of collating and comparing our data for the purpose of discovering wherein, and to what extent, they agree or disagree in respect of certain features or characters which we are endeavouring to determine. To this end, they must be scrutinised and sifted, eliminating whatever is irrelevant and extraneous; and then be grouped in order, according to their points of agreement or disagreement, of resemblance and apparent connection. The result or results thus arrived at have then (and this is the *third* act in the process) to be expressed in a simple formula or proposition, declarative of the conclusion or outcome obtained from the evidence which has been subjected to the foregoing examination. Such a proposition, provided the evidence on which it rests and from which it has been derived is sufficiently ample and complete, will then stand as an axiom or established principle, serving to point and pave the way to the discovery of larger and more comprehensive general axioms or truths, which, almost invariably, prove to be "fructiferous" as well as "luciferous," by suggesting useful applications in practice. A single illustration derived from epidemiology itself will suffice to show this. That diarrhœal ailments are very generally prevalent among a community in which epidemic cholera has appeared, and that, in the vast majority of instances, individual attacks of cholera are preceded by signs of disordered bowels of longer or shorter continuance, are inductive truths that serve not only to illustrate the physiology of the disease, but also to suggest an important precaution in the way of its preventive treatment.

Very different from the inductive method is the deductive or pre-Baconian method, which was generally followed in physical investigations prior to the publication of the 'Novum Organon.' The latter first lays down as truths or axioms one or more propositions, and then proceeds to explain or account for the phenomena or facts which may be observed, showing that their occurrence is in accordance with the formulæ propounded. The propositions are always more or less hypothetical; sometimes purely so, and in other cases only partially so, being derived in part from observed facts, and in part from speculation or conjecture. In deduction, we reason *down* from generals to particulars, from accepted principles to the interpretation of individual facts or phenomena; in induction we reason *up* from the examination of numerous ascertained or established facts to the discovery of a general principle or law which serves to link the facts together. Induction has been defined to be the process of discovering laws from facts and causes from effects; and deduction that of deriving facts from laws and effects from their causes. Let it be ever borne in mind that to endeavour to establish, or to enunciate, a general proposition from the consideration of uncertified data is to sin against the fundamental rules of the inductive method. The data must first be proved to be facts or certainties before we proceed a step further. Not that hypotheses can be altogether foregone or rejected; as suggestions and incentives to further and more accurate observation, they are often most useful; but they can never form a safe basis or scaffolding for a superstructure. "Our varying hypotheses," said Faraday, "are simply the confessions of our ignorance in a hidden form; and so it ought to be, only the ignorance should be openly acknowledged." Again, however much the inductive method of investigation may differ in its procedure from the deductive method, there is one form or act of deduction or reasoning downwards, which it accepts and recognises as most valuable. What Bacon calls 'deductio ad praxim,' is the application of established physical truths to the uses and needs of human life. "When we have attained," says an able writer, "to propositions of any degree of generality in science, we can often by pure reasoning *deduce* from them consequences recommended by their curiosity or utility. And the more general are the truths which we have reached by our inductive ascent, the more copious and varied will be the inferences which we may obtain by reasoning downwards from them. This mutual dependence and contrast of induction and deduction, this successive reasoning up to principles and down to consequences, is one of the most important characteristics of true science."[1]

Just in proportion as the study of epidemiology is pursued according to the inductive or the deductive method, will its future course be either steady and progressive, or wavering and insecure. In other departments of natural science in their infancy the latter method, being the more prompt and easy, was long the one adopted in the schools: and so it has hitherto been with this branch of medical research. At the present time, epidemiology is little better than a maze of vague opinions and speculations, of *dicta* rather than of *data*, of surmises and hasty inferences rather than of thoughtful and logical conclusions. The character of the disputations respecting most of its problems generally savours much more of that of theological discussions than of philosophical reasonings, being efforts

[1] 'Quarterly Review,' No. 90, 1830. The article is a review of Sir John Herschel's 'Preliminary Discourse on the Study of Natural Philosphy,' then recently published, and which affords so admirable an exposition and illustration of inductive investigation.

rather to establish a favourite proposition than to demonstrate a truth. A total change must take place, if we hope ever to raise our branch of research to the same level with that already reached by other and kindred fields of scientific investigation.

II. A cursory retrospect of the last twenty-five or thirty years will show how wide and diversified is the field (rather only a part of the field) of inquiry which epidemiology presents to the scientific physician, while it serves also to attest the magnitude and variety of the interests, social and national, involved in a right understanding of the questions which appertain to it. Most of my illustrations will be taken from the history of those pestilential diseases whose career, since the beginning of the present century, I have endeavoured to sketch in previous numbers of this journal.[1] 1846 saw the publication of the 'Report of the French Academy on the Plague.' This most valuable document, the fruit of long and earnest labours, undertaken at the instance of their government, on the part of some of the most eminent professional and scientific men in France, scattered to the winds most of the doctrines long dominant respecting this dreaded disease, shed light where all had previously been the darkness of ignorant prejudice, calmed exaggerated alarms, and shook the foundations of the whole quarantine system throughout Europe—a system which had, for more than a century, acted as an oppressive burden on commerce and bar to international intercourse, without affording all the time any reliable protection to public health.

The disappearance of the plague for now nearly thirty years from the Turkish dominions, which had for ages been its principal seat and stronghold, is a very noteworthy event in epidemiological history; and not less interesting, in a scientific point of view, is the occurrence of, at least, two isolated and local outbreaks of the disease of distinctly spontaneous growth, in districts far apart from each other, and with an interval of several years between them, viz., on the north coast of Africa in 1858, and in the valley of the Euphrates in 1867.

As to yellow fever, never has this pestilence been known to be more widely and more fatally prevalent in the New World than during the last twenty years or so. It was in 1848-49 that Brazil, after a lengthened immunity from the scourge, again became the seat of its ravages. Prior to that period, the Brazilian station was one of the most healthy of our naval stations; since then, it has been far otherwise. But it has not been the western coast only of South America which has suffered so much of late years from this deadly fever; Peru and Chili appear, from the valuable observations of the late Dr. Archibald Smith, to have been the scene of frequent visitations of fever of

[1] Nos. for April, 1864; July, 1864; Oct. 1865; January, 1868; July, 1868; and July, 1869.

a kindred type, if not identically of the same nature. Throughout most of the Mexican gulf, there have been in different years a great amount of deadly sickness from this cause, in several of the West India islands, and also at various points on the mainland of North and South America. The great island of Cuba seems to have been never free for a single year from the fever; and St. Thomas, the entrepot of so much commercial activity of recent years, has become a fatal nest of disease and death. Jamaica, which had for many years prior to 1852 remained comparatively unscathed, has, again been the seat of frequent visitations; and the fever there has on more than one occasion, appeared at higher elevations above sea level than it was formerly known to do, a fact of no small interest as respects the natural history of the disease. The outbreak in New Orleans in 1853 exceeded perhaps in fatality that of any previous invasion; and our own colony of Bermuda has suffered more frequently since that year than it ever did before. Moreover, what adds not a little to the interest of this subject, there is the distressing fact that at no former period of our navy's history have our ships of war sustained more disastrous losses from yellow fever than during the last twenty or twenty-five years. The terrible outbreak in the "Eclair" on the West African coast in 1845, leading to the destruction of a third of her crew, and the prostration of all on board, was followed by the sad histories of the "Dauntless" in 1853, of the "Malacca" in 1856, and of the "Icarus" in 1860, not to specify other instances that might be cited in numbers. Naval medicine seems to have made no progress yet in preventing such disasters.

Nor has Europe quite escaped. Lisbon in 1856 experienced a visitation of no common virulence; and Oporto has more than once in late years been touched by the breath of the pestilence. The occurrences at St. Nazaire, at the mouth of the Loire, in 1861, and on our own shores at Cardiff, in 1865, are also not without importance.

But it is the recent history of epidemic cholera which presents the most striking illustrations of the great national importance of epidemiological inquiries. So far from decreasing in activity in its 'home,' it has recently manifested greater power and virulence in India than ever. Notwithstanding all that has been written and done about the disease for the last fifty years, we are still nearly, if not utterly, powerless as respects alike its prevention and cure. At the present moment, the Government of India is preparing a new scheme for the more thorough and profitable investigation of this mysterious foe, which has hitherto so signally baffled our researches. Thrice within the last twenty years, has it passed over Europe and extended to America, spreading dismay everywhere, and showing how futile are the attempts of man to stop the progress of the "pestilence which walketh in darkness." On two different occa-

sions during that time, did the principal nations of Europe convoke an assembly of professional savans to enlighten them, if possible, respecting the nature and attributes of the disease which had menaced or assailed them, and to advise them as to the best measures for its prevention or arrest. On both occasions the results were discouragingly fruitless. Nor can it be now predicted in respect of the next European invasion that it will find greater unanimity of opinion among different nations as to what should be done to ward off the pestilence, or to mitigate its diffusibility and destructive power, than there was when it first loomed like a dark cloud on the eastern frontiers of Europe in 1830-31.

It is not only in reference to epidemic diseases abroad that the last quarter of a century has been noteworthy; the period has been one of no common interest in respect of our domestic epidemiology also. To mention only one or two events. There has been the outbreak of diphtheria, and its first recognition by us as a distinct specific disease, coupled with the curious fact of its speedy development, about nearly the same time (as far as we know), in different parts of the world, so remote from each other as Nova Scotia and Australasia. Whether there has been any connection or relation between this event and the marked increase, in this country at least, in the diffusion and virulence of scarlatina is an important question that awaits solution. The unusual prevalence of carbuncular disease generally, during a great portion of the time, is another fact deserving record. But it is more particularly with respect to the epizootics and epiphytics—the murrains and blights—that the last quarter of a century has been remarkable. It was about the beginning of that period that the great blight, known as the "potatoe disease," first infected that important article of human food in this country. Although from that time to the present it has never ceased to manifest itself among us to a greater or less extent, no real progress seems to have been made by botanists or agriculturists in the discovery of its leading attributes, in respect of its predisposing or exciting causes, of what favours its persistence or recurrence, or as to any reliable means for its prevention. There has been no geographical or chronological record, I believe, of its early history; so that we are quite unable to determine when and where this mysterious distemper was first observed, or to give a trustworthy account of its subsequent course and distribution over different regions of the world. The same thing may be said as to the defective state of our knowledge in regard of the epizootics, which have of recent years attracted so much the attention of most European countries, and are of so great economic interest to all peoples. Extremely little is known of the natural history of these murrains, of their climatic and seasonal relations, or of their geographical extension; nor has any systematic investigation of these phenomena been set on foot.

It may be reasonably presumed that whatever advance is made in elucidating the genesis and spread of murrains and blights, will pretty surely react advantageously on the science of epidemiology; and *vice versâ*; for it seems far from improbable that the same or, at least, similar cosmical agencies influence the production of disordered phenomena in the different families of organic existence.

If the number and variety of the topics demanding investigation may seem, from what has been said, to have increased of recent years, equally so have our means of intelligence, and our opportunities for acquiring information respecting them, if these means and opportunities are all turned to the most profitable account. The annual reports, now regularly issued, of the health of our army and our navy, of the different presidencies of our great Indian empire, of our colonial possessions, as well as of our domestic hygienics in the reports of the Medical Officer of the Privy Council, and of the registrars of England, Scotland, and Ireland, are valuable stores of authentic instruction respecting epidemiological occurrences which our predecessors did not enjoy, and the like of which no other country can produce. It behoves us to profit by such advantages.

III. The general characteristic attribute of epidemics is their tendency to alternating periods of development and disappearance, or of irregular recurrence and decline or extinction. For years, the disease may be entirely absent, or it may be seen only sporadically and partially; the cases being few, scattered and occasional, occurring singly, or in small detached groups of two, three, or so. These individual isolated attacks may be, at times, of a severe and malignant type; but the malady nevertheless shows no tendency to spread or multiply, even when the more obvious surrounding circumstances and conditions are notably favorable to its development and activity. In other years, it manifests from the first a marked disposition to increase and be diffused, and, ere long, it becomes rampant and widely disseminated. This signal difference in different years is to us a mystery, towards the physical elucidation of which no step has yet been made in advance. Many phenomena, it may be observed, in biology and in meteorology appear to be subject to cyclical changes of increase and diminution in point of activity, frequency of recurrence, &c., which it is equally beyond our power to explain. Dr. McDonald, F.R.S., remarks, in reference to the irregularly periodic occurrence of yellow fever in the tropics with intervals of immunity, that this circumstance "has its parallel in a fact well known to students of the diatomaceæ and desmidiaceæ, viz., that particular species, which are known to exist in a definite pond or pool one season, may be at another replaced by forms never before detected in the same spot; while again the original species, under favorable and often unaccountable circumstances, reappear after the lapse of a

certain time."[1] Analogous occurrences are not unfrequent both in the animal and in the vegetable world.

It will be shown in the sequel that certain external agencies relating to the state of the weather and other meteorological as well as terrestrial conditions, to the household accommodation of human beings, the supply of the necessary articles of their food and drink, together with other matters connected with their physical constitution and general status, have much to do with the extension and persistence, if not with the primary development, of some epidemic diseases in certain years. But beyond the truths discoverable from the consideration of these external adjuvant circumstances, all is dark and mysterious about the genesis of epidemics, or the primary cause of their upspringing in one season and not in another. Plagues and pestilences are doubtless to be regarded as judgments of the Almighty Ruler, which play their appointed part in the scheme of Providence; but the recognition of this revealed truth does not of course exclude the rightfulness and duty of seeking to discover the links in the chain of material causes which lead to their development and extension. Each epidemic disease requires to be investigated for and by itself, and its natural history—in other words, its attributes in relation to other phenomena or events in the world of nature—needs to be ascertained by examination of the leading distinctive signs and properties which it exhibits in successive outbreaks; and, when practicable, not in one region or country only, but in diverse parts of the world. Fallacies and errors will almost inevitably be committed in seeking to establish any general propositions as regard either the physiography of the distemper, or its prophylactic and preventive treatment, from the experience of any single visitation, and particularly when the area of observation has been of limited extent. The inductive investigation of an epidemic is a much slower and more toilsome process. Conclusions, which seemed just and reasonable on the occasion of one visitation, will often be found to be scarcely tenable upon ulterior experience, and to require modification, if not total abandonment. The careful comparison of successive outbreaks, in respect of all their features and characteristics, is indispensable to our true knowledge of the natural history of the disease. If once we were in possession of such accurate knowledge of epidemic diseases derived from, and based on, such a full investigation of each of them, we should then have the necessary elements for the foundation of Comparative Epidemiology; a branch of scientific research hitherto quite unexplored; but which, if rightly pursued, might lead to many curious and important results, just as a similar method of inquiry has proved so prolific of good in other departments of natural knowledge. However much the attributes and general career of

[1] 'Report on the Health of the Navy,' 1860, p. 72.

certain epidemic diseases may differ the one from the other, as, *e.g.*, influenza and the plague, still they will be found to present some points of not unuseful comparison; and the history of the one may possibly serve to cast some light on that of the other. The course of the 'black death' in the fourteenth century seems to have been not dissimilar to that of influenza in the last and the present centuries.

On comparing different epidemic diseases, we are soon led to recognise a marked distinction, in several respects, between the order of the proper Exanthemata (to which hooping-cough and diphtheria must be added), and that of most other forms of zymotic febrile disease. The persistence and, often, all but permanence of their presence (subject however to notable exacerbation and abatement from time to time) in most countries which they infest; the lengthened duration of their epidemic visitations, continuing for many months, and even for one, two, or three years; their special tendency to prevail chiefly in infancy and early childhood; their greater and more indisputable contagiousness; and their more marked independence of the ordinary local unsanitary influences in respect of their development and general dissemination, suffice to indicate peculiarities of character, which seem to stamp them as a special order or sub-class by themselves. It will be prudent, therefore, to exercise considerable reserve in interpreting the phenomena of other epidemic diseases by analogical comparison with the exanthemata, as is not unfrequently done. But here, as with every other topic of epidemiological inquiry, the grand desideratum is, first of all, to obtain thoroughly accurate accounts of the natural history of each disease, so as to enable us then to compare and contrast the records with each other, and thus discover the points wherein they mainly agree or differ. In the sequel, most of the data used in the way of illustration will be taken, as already intimated, from the history of the three epidemic diseases whose geographical and chronological distribution since the beginning of the present century has been sketched in former numbers of this journal. They are not all given as certified or indisputable facts, but only as the best that I know of; it is the want of trustworthy "pièces justificatives" that ever has been, and still is, the great obstacle to the progress of sound epidemiological knowledge. The use, moreover, of shaded maps to indicate the areas affected by epidemic outbreaks will be found to aid greatly in giving precision as well as interest to statistical details, and thus serve to engage and fix the reader's attention. They were employed, with much advantage, in the reports of Drs. Baly and Farr in illustration of the spread of the cholera epidemic of 1849 over England and Wales; and it is much to be regretted that similar charts have not accompanied the official narratives of subsequent visitations. No description however vivid, or statistical narrative however exact and detailed, will ever fix the

leading facts as to the topographical course and extension of an epidemic like a pictorial illustration. In following the career of wide-spread physical phenomena, the student will invariably find it to be most useful to have a map of the country or region continually before him; many suggestions will thus occur to his mind which otherwise he would not have dreamed of.

Besides the diseases usually regarded as strictly epidemic from the circumstance of their generally being more or less widely prevalent among a community, it is to be remembered that various maladies, which ordinarily occur only sporadically, are liable in certain years and seasons to become so frequent in districts of varying extent, as to manifest there a decidedly epidemic character. Erysipelas, ophthalmia, metria, and several cutaneous affections may be cited as examples in point. The carbuncular epidemic of late years in this country is also a notable instance. Even such a disease as hydrophobia is known, in some countries, to have been so unusually common in certain years compared with others, as to have been described as prevailing almost epidemically. The systematic registration of such events, together with a brief notice of synchronous biological and other cosmical phenomena, if continued over a long series of years, might probably lead to the detection of many curious and instructive inter-connections. When we read of meteorologists giving the averages of half a century's daily observations, and of other physicists keeping regular records of solar spots for upwards of forty years, we forcibly feel how much in arrear has been hitherto our branch of physical inquiry in respect of its very A B C.

IV. Are epidemic outbreaks usually sudden and unheralded events? or are they generally preceded by discoverable nosological indications and premonitions of their advent? To answer this most interesting question, it is obvious that the exact health-state of the place or district for weeks or months previous to the earliest case of the incipient distemper must, among other data, of course be known. Such information is unhappily seldom to be had in respect of epidemics in former times, and it is far too rare even in the present day. No value should ever be attached to the common vague statements that the health of the community was quite good, or was as good as usual, prior to the first case of the epidemic: unless reliable statistics of the antecedent death-rate at least, if not of the prevalent sickness as well, can be referred to, so as to ascertain the facts relating not only to the actual season but also as compared with those of other years, they are all but utterly worthless. In the large majority of instances where this point has been rigorously inquired into, traces and signs of a pre-existing *dyscrasia*, or sickly diathesis, seem to have been detected. The French commissioners have given in their report many examples, which show that epidemic outbreaks of plague in the present century were very generally preceded, sometimes for many months, by the greater prevalence than usual of bad forms of fever, periodic or continued, accompanied occasionally with bubos or other pestoid symptoms, so that often it was scarcely possible to determine when the earliest attacks of the true plague really occurred. Sydenham and other physicians in the 17th century expressly refer to this very point; and in the recent outbreaks at Benghazi, and near to Bagdad, we knew that typhus had been prevailing among the affected communities for a considerable time before any mention was made of the existence of plague in either district. In the case of the plague at Malta in 1813, Hennen has told us that the season was remarkable for the extraordinary prevalence of carbuncular disease in Valetta, and the general tendency of wounds and ulcers to take on an unhealthy character, prior to the first cases of the pestilence.

With respect to epidemics of yellow fever, the existence of a precursory sickliness was very conspicuous in several recent outbreaks, of which we have fortunately detailed narratives. For at least one or two years prior to the appearance (after an absence of more than half a century) of this pestilence at Rio Janeiro and other seaports of Brazil in 1849, a notable change in the character of diseases had been observed; "and the ordinary endemic fever had been becoming less and less remittent, and more decidedly continued in type, so much so that many cases were regarded as of a new form of fever, to which the appellation of 'insolation fever' was given, and which resembled, it is said, in many respects, the milder cases of yellow fever. These occasional cases became from year to year of a more aggravated and fatal character, and in several instances the matters vomited had much of the appearance of the true black vomit." ('Brit. and For. Med.-Chir. Rev. for July, 1864,' p. 182). The outbreak of the severe epidemic at Bermuda, in 1853, the earliest cases of which occurred among convicts on board the hulks, was preceded by a season of great sickliness from flux, enteric fever, and 'dengue;' and Dr. Lyons tells us that, before the earliest cases of the Lisbon epidemic of 1856, together with the great prevalence of gastro-intestinal affections and cholera, fevers, both continued and periodic, had been extremely prevalent, and often manifested a marked hæmorrhagic tendency. Nowhere do we meet with more striking manifestations of a similar character than in the recent history of the disease in our ships of war. In almost every severe outbreak recorded in the reports of the navy, the health of the crew had been, for some time previously, suffering from a large amount of fever and diarrhœa. This ætiological point has been worked out at considerable length, and illustrated by numerous examples, in a recent number of this journal (April, 1869). Important practical lessons are obviously deducible from the position there laid down, if it be

confirmed by subsequent experience. Certain it is that the extension and virulence of yellow fever on board ship seem to be invariably in proportion to the unsanitary condition of the vessel and the unhygienic condition of the crew. Part of a healthy ship's company, who have caught the disease by visiting another ship or a locality infected with it, may on returning to their own ship sicken and die; but then the fever will often not spread beyond those primarily affected, and ceases with them, whether the cases have or have not proved fatal. A striking instance of this sort occurred two or three years ago in the "Bristol" frigate soon after arrival at Sierra Leone, when a boat's crew contracted the fever by having gone on board a very sickly ship, and most of the men attacked died soon after their return to the "Bristol," without a single case occurring among any other persons of the crew. How different from the usual career of the disease when it imperceptibly, so to speak, becomes developed among a crew who have been suffering from flux and fever, especially if crowded together, and the vessel be at the same time unwholesome. Under such circumstances, it is frequently impossible to determine when the first unmistakeable instance occurs, as many of the previous cases may have, for some time before, begun to exhibit symptoms which awakened the suspicions of the medical officers.

In the history of cholera, particularly in that of its extra-Indian career, no feature in respect of its epidemic development, in different places and seasons, has been more conspicuous than that of its invasion having been very generally found (whenever full and accurate intelligence of the event has been procurable) to have been preceded by an increased amount of intestinal disorders among the affected population. As regards Great Britain, where the detailed records of the three last visitations have been far more complete than in any other country, this fact stands out in strong relief. For several years prior to the visitation in 1848-49, the mortality from alvine flux (diarrhœa and summer cholera) had risen in a very notable degree over the metropolitan area. Again, before that in 1853-54, there was a marked increase; and the same phenomenon was still more striking on the last occasion. From the ampler information we have of that visitation over the continent of Europe, no former one furnishes so many illustrations of the point in foreign localities as the epidemic of 1865-66. At Malta, the health-state of the island had been bad, and the mortality from fevers, &c., had been in excess for several months prior to the arrival of any infected vessels from Egypt; diarrhœa had also been prevalent, for some time previously, in that very part of Valetta where the earliest cases of cholera are known to have occurred. At Algiers, where the earliest cases of cholera occurred in the first week of September, alvine flux had been unusually prevalent and severe throughout the summer. The information respecting the public health at Marseilles prior to the earliest cases in June is conflicting; some medical men affirming, while others denied, the existence of antecedent sickliness among the population generally. That several deaths among adults from diarrhœa, gastro-enterite, and 'miserere,' occurred in the months of May and June, is admitted; but unfortunately, the mortuary registration had been so loosely and imperfectly kept, that no comparison could be made between the number of such casualties in 1865 and that in previous years. At Trieste, another Mediterranean port which was in constant intercourse with Alexandria, bowel disorders were unusually frequent and severe for between two and three months before the pestilence distinctly manifested itself there, about the end of September. The public health at Ancona, also, it may be fairly presumed from the observations of Dr. Ghinozzi at the time, seems to have been anything but satisfactory, previous to the arrival of any infected vessels in June; and we know that at Lisbon, and other places in Portugal, there was a veritable epidemic of "cholerine" during the summer and early autumn months before the earliest cases of cholera were observed. The same thing was notably the case at Lisbon in the former epidemic in 1856. The remarkable outbreak which took place at Altenburg in Saxony, the history of which has been so frequently and emphatically dwelt upon by writers on the epidemic of 1865, affords another striking illustration. The general death-rate had been greatly in excess for many months, particularly during the summer (apparently from the extreme prevalence of intestinal affections), before the earliest case of cholera occurred in a stranger recently arrived from Russia, and who, it has been confidently maintained by the Constantinople Conference, as well as by most writers on the history of the visitation, imported the pestilence from that country. In Russia itself, as we learn from the official report of the epidemic in that country, "in almost every place before the appearance of the cholera (in 1865), the prodromata of the disease, especially diarrhœa, were observed."

These data seem to afford reasonable indications that a greater sickliness than usual, manifested mostly by the prevalence of a "diarrhœal diathesis" so to speak, existed during the early summer of 1865 over southern and central Europe, prior to the appearance of the epidemic that year. A similar state of things was present in northern Europe at the same time, as I learn from some highly interesting communications which I have received from Dr. J. W. Moore of Dublin, and from which it appears that both in Copenhagen and in Christiania, and in Stockholm, bowel disorders were much more frequent and severe that season than is ordinary.[1] It was not

[1] The data communicated to me by Dr. Moore have since been published in the 'Medical Times,' for May 14th, 1870.

till the following year (1866) that the cholera appeared in any part of Scandinavia.

In respect of that puzzling incident in the history of the epidemic of 1865, the outbreak of the disease in the French West India island of Guadaloupe in the last quarter of the year, it appears from the report of M. Cuzent that, for several months previously, there had been numerous deaths at Point-a-Pitre by what is designated "une fievre algide choleriforme," from the resemblance of the attacks to those of genuine cholera. "L'ouragan electrique du 6 Sept. a eté la cause determinante de l'evolution spontanée du fleau, en donnant alors le caractere infectieux et epidemique du cholera Indien à l'affection endemique et localisée jusqu'à ce moment dans les faubourgs."[1]

It has been often asserted that outbreaks of cholera have occurred among perfectly healthy populations, and without any indications whatever of precursory sickliness. In the cholera epidemic of 1865, the two most conspicuous instances of this sort that I know of are those of Constantinople and of Gibraltar. In respect of the outbreak in the Turkish capital, it has been stated that, prior to the arrival of the infected frigate from Alexandria at the end of June, nothing in the condition of the public health had indicated the approach of any epidemic sickness. It may be so; but in the want of all mortuary registration, and of the means of comparing the mortality of one season with that in former years, it is obviously impossible to determine the point in question with any degree of accuracy. With regard to the case of Gibraltar, the source of difficulty is of another kind. A regiment of apparently healthy men, arriving by sea from a place where cholera existed, was landed and camped out on the neutral ground between Gibraltar and the Spanish frontier in the second and third weeks of July, when the general health of the garrison and civil population was, and had been throughout the previous season, unusually good. Three fatal cases of cholera occurred among the corps before the end of the month. The regiment, being on its way to Mauritius, was immediately embarked, and reached its destination without another case of the disease having appeared among the men. But within a week of their departure, it began to manifest itself in the garrison, and it became epidemically diffused during the next two months. The question comes to be whether the disease, or its heralds, existed in that part of Spain adjoining to Gibraltar at the time of the landing of the troops. Unfortunately, no reliable information on this point has ever been given by the Spanish authorities; and all that can be positively asserted is that the pestilence had appeared on the west

[1] 'Epidemie de la Guadaloupe' (1865-66). Par Gilbert Cuzent, Chevalier de la Legion d'Honneur. Paris, 1867.

coast of the peninsula in the beginning of July, and therefore prior to the earliest case on the neutral ground. Whether any towns on the south coast of Spain had also begun to suffer about the same time, it is impossible from want of intelligence to determine.

On the whole, it may be fairly concluded, I think, that epidemic outbreaks are very generally not sudden events, but are preceded by an unusual amount of sickliness, and that this sickliness often partakes of the nature, or at least exhibits some of the features, of the coming disease. The true history of an epidemic begins, it may be said, with that of its antecedents. Many illustrations of the truth of this remark might be drawn from the history of other epidemic diseases abroad besides those to which reference has been made in the previous observations. The case of the fatal fever which has been decimating our once flourishing colony of Mauritius is much to the point. For two years, at least, before it acquired the force of a disastrous epidemic, there had been a notable increase of general sickliness, and of fevers in particular; and prior to that time, the frequent return of cholera visitations, in an island which had long enjoyed a remarkable immunity, was a feature in its recent medical history of significant interest.

It would be highly instructive to be able to follow up the foregoing remarks on outbreaks of foreign epidemic diseases with similar notices respecting analogous outbreaks at home; but I have neither the necessary space nor materials to do this. There is much reason to believe that the same general law would be found to hold in both alike, and that the upspringing and development of most epidemics among ourselves are by no means such sudden events, unpreceded by any discoverable signs, as they have been often imagined to be. To quote the most recent instance, that of relapsing fever in the metropolis, it appears to be unquestionable that cases of the disease had been observed many months previous to the commencement of its epidemic prevalence.

V. What is the manner in which a *new* epidemic outbreak—*i.e.*, one quite unconnected with the recent existence of the disease in the locality—usually appears and becomes diffused? It has been so often confidently asserted that it has passed into a general belief, in respect more particularly of the distempers deemed pestilential, that the disease radiates from one or more spots where the earliest cases have occurred, as from a centre or centres, and that it gradually spreads thence from place to place and from district to district, according to their contiguity and to the amount of their intercourse with each other, until perhaps the whole area has come gradually to be more or less deeply infected. When, however, a systematic step-by-step inquiry has been instituted, this mode of explanation has generally been found to break down, the evidence

as to a traceable connection between many of the early attacks in different localities being discovered to be quite uncertain and only conjectural. The first case or cases almost always occur among the poor, and in unhealthy localities. Within a short time, other cases, single or in small groups, appear successively, or it may be synchronously, in other similar spots more or less distant. Attacks multiply near to or around these several foci, while isolated cases occur elsewhere, perhaps in places remote from the early "centres of infection" (to use a common but doubtfully correct phrase), and milder and less distinct forms of the malady may be becoming more or less prevalent among the community generally. If represented pictorially, the affected area would exhibit a mottled or dappled appearance, the coloured spots varying in shade and size according to the virulence and extent of the disease in different localities. Many facts seem to indicate that morbific miasms are diffused in the atmosphere not uniformly or equably as gases or vapours are diffused, but rather in, at first, scattered (*éparpillés*) points or patches, which afterwards become larger and more numerous, and eventually coalesce into broad and wide nebulosities, somewhat as we see take place with clouds before a thunderstorm. Various cosmical phenomena appear to be developed after this fashion; and so it may be, perhaps, with those of epidemic disease. This is, however, as yet only a conjecture; and we want exact data respecting the chronology and topography of all the earliest manifestations of the nascent malady to test its soundness. Every one will perceive what important bearing all trustworthy facts relating to the *synchronism* of the early cases, in different localities, must have on our true knowledge of the natural history of an epidemic. Exact intelligence as to dates and localities is the foundation of all sound epidemiological research; without it, no real progress can be made.

The rise and early spread of epidemic cholera in London in 1848-9, and again in the visitations of 1853-4, and of 1865-6, seem to have taken place in the way indicated above. Nothing like radiation or step-by-step advance, as from a centre or centres to more distant parts, could be traced, although every genuine case of the disease as it occurred was at once known and recorded. Attacks took place nearly contemporaneously in different districts, remote from each other, and evidently independently of inter-communication. The same may be said as to the mode of the upspringing of the disease in other large towns of England, where its origin and cause were followed. Notwithstanding the most vigilant search, it was generally found to be impossible to discover any connection between very many of the early attacks. This was, for example, conspicuously the case in the late epidemic at Bristol, where, from the completeness of the supervision exercised, every circumstance relating to the outbreak was known with the greatest exactitude.

In two thirds of the twenty-five separate localities where the disease made its appearance in and around the city (between July 21 and Nov. 12) its source could "neither be made out nor guessed." In six of the remaining instances, the disease was directly imported by strangers arriving with it upon them. In two instances only, could the attack be distinctly traced to communication with the sick. All the while, diarrhœa was very prevalent throughout the city. And as with towns or small districts in respect of the mode of the spreading of the disease, so it was with the whole area of the United Kingdom; in which, thanks to our excellent statistical machinery, the death-progress of the epidemic can be easily followed out. The diffusion appeared to be not by radiation, but rather by the independent up-springing or evolution of the morbific element in numerous different localities. What was observed to take place in this country seems to have occurred, in like manner, in cities on the Continent; as well as can be judged of from the imperfect statistical accounts that we have of the inception and early diffusion of the pestilence in their midst.

We have no such detailed and circumstantial narrations of the rise and spread of epidemic yellow fever in towns as we have of malignant cholera; but from what information we have, it would certainly seem that the outbreak of this pestilence also is usually after the same fashion. In the epidemic at Bermuda in 1853, "the fever broke out," according to the official report, "in different and distant parts of the colony nearly simultaneously," p. 12. In that of 1856, the Commission of Inquiry came to the conclusion that "it made its first appearance not at one, but at several points, distinct and widely separated, and simultaneously at two such points," p. 9; and in the last visitation in 1864, we find that the Commissioners on that occasion failed to discover any connection between the early cases, which appear to have occurred in different localities. Moreover, the first attacks among the military could not be traced to communication with the previous cases in the town; nor did the one seem to be connected with the other. Again, Dr. Lyons, in his history of the Lisbon epidemic of 1856, states his "conviction, based on the results of inquiries made amongst medical men of all shades of opinion, that, prior to the declared and fully recognised existence of the epidemic, isolated cases presented themselves in various parts of the city." To the same effect are the remarks in most of the narratives of severe outbreaks of the fever elsewhere, as at New Orleans, and other cities in the Southern States, and in Demerara, &c.

If our information respecting the authentic early history of yellow fever epidemics be so imperfect, far more so is our knowledge respecting that of the plague. But from incidental remarks we gather that, at a very early period of most outbreaks, the disease did begin to manifest itself in several distinct localities, between which it was

impossible to trace direct intercommunication. Thus we learn from Sir B. Faulkner's narrative of the plague at Malta in 1813, that soon "the contagion began to diverge in so many directions, that it would have been extremely difficult, if not impracticable, to follow up the direct line of contaminations;" and we also know, that in the outbreak at Corfu in 1815, the attempt to trace connection between the earliest cases quite failed, and, moreover, that the disease appeared about the same time in several villages not far from the district where its existence was first recognised.

And is it not the case that the history of most of our domestic fevers and other zymotic diseases reveals a similar mode of rise and development, viz., by the occurrence at first of sporadic cases, single or in small groups, in various localities of a town, village, or district, and by the subsequent multiplication and irregular extension of these "foci," until the whole or greater part of the area, it may be, becomes more or less sensibly affected? Is not this the usual course to be observed in respect of epidemics of typhus, of enteric and of relapsing fevers, of puerperal fever and of diphtheria, not to mention other kindred maladies, which from time to time appear in the land? The question, however, needs scrutiny.

VI. A fact, which the epidemiological student has often occasion to observe, is the marked uncertainty that very generally will be found to exist as to the exact date of the earliest case or cases of many epidemic outbreaks. And yet, without accurate knowledge on this point, it is obviously vain to speculate with any advantage as to the probable origin and cause of the disease. Various circumstances serve to bring about this uncertainty. Zymotic distempers, when they do not prove fatal, often fail to manifest some of their pathognomonic phenomena. Thus, the milder attacks of plague may not exhibit any carbuncles and petechiæ; the cases of yellow fever which recover are almost always without black vomit; and those of cholera are seldom accompanied with total suppression of urine and complete pulselessness. In short, the attack of plague is then often not distinguishable from one of typhus; that of yellow fever from one of tropical remittent or continued fever; and that of cholera from cholerine or of summer cholera. Yet these milder forms are as capable, it is generally maintained, of giving rise to attacks of a formidable type in other persons as those cases where the symptoms are more developed and more grave. In narratives of epidemics, the commencement of the outbreak is generally dated from the earliest deaths from the disease, whereas the morbific poison may have been at work in the locality for some time previously, indicated by the greater prevalence than usual of the milder forms of its manifestations. Much of the ambiguity respecting the origin of an epidemic outbreak, and many hot disputes on the question whether it was due to foreign importation or to indigenous development, may be traced to this very circumstance. It is, moreover, to be remembered that there is always an unwillingness on the part not only of the general public, but often of many medical men also in a community, to admit the existence of a pestilential disease in their midst, if there be a doubt upon the matter, and there be a mode of explaining the phenomena which have given rise to the suspicion in any way short of admitting the unwelcome truth of its indigenous development. If the imposal of any penalties or of restrictions, affecting the freedom of personal or commercial intercourse, be dependent on the decision, affirmatively or negatively, of the point in question, the difficulty of arriving at the truth will be increased tenfold. No reliance whatever can then be placed on most of the statements either one way or the other; they will be infallibly warped by the exigencies of a misleading partisanship. And this result will be the more marked just in proportion to the stringency of the prohibitions resorted to, in the way of prevention. Hence the invariable impossibility of obtaining accurate information about the rise and early progress of such diseases as pestoid and yellow fevers, or of spasmodic cholera in any city, especially maritime, of Spain or Portugal; and hence too the common practice in these countries, and in their dependencies, of their seeking to explain away, or of directly denying, the occurrence of cases of sickness which are of a suspicious character, until the fact becomes so notorious as to compel their tardy avowal of the truth.

It not unfrequently happens that among the first to be attacked by an incipient epidemic are persons recently arrived, it may be from a distance, in the locality. The suspicion of its having been thus introduced is a natural one; and if the stranger or strangers have come from a place where the disease exists, the suspicion usually becomes a conviction, on the part of the general public, that the disease has been imported by them. It is, however, well known to medical men that none are so susceptible to the poison of zymotic diseases generally as strangers and occasional visitors to a place where they either actually exist or are impending. The extreme liability of new comers to be attacked by yellow fever in the West Indies, and by cholera in lower Bengal, is notorious; and the fact is often little less conspicuous in respect of our own indigenous fevers, when persons from the country or from shipboard resort to localities in our large towns which are extremely unhealthy, but which may be, or seem to be, at the moment to be free from overt infection. The history of the last three visitations of yellow fever in Bermuda may be cited to illustrate the occasional ambiguity that there is in determining the primary source of an epidemic outbreak. In 1853, and again in 1856, the disease was considered by the Commission of Inquiry to have originated indigenously, the first cases having occurred in residents,

and without any reasonable ground for believing that they had had previous communication with either infected persons or places. In 1864, the earliest *fatal* case (two or three suspicious milder cases had previously taken place) occurred in a man who had left Nassau, where the disease existed, a fortnight before, and had been in Bermuda for ten days. Within the next week or so, two other deaths occurred in persons who had not had any communication with the former patient; and the fever thereafter speedily appeared in different localities, separate from each other. It would obviously be most unwise to argue from such an instance as this that the disease must have been imported into the island from the West Indies; and the more so as in a subsequent year several well-marked cases of the fever occurred about the same season, as on the former occasion, and when there was no suspicion whatever of introduction *ab extra*. Among other incidental topics suggested by the above instance is that of the incubative period of the disease, a question to be hereafter considered.

No medical men have such favorable opportunities of ascertaining with exactitude the precise dates of the earliest attacks of epidemic sickness as the medical officers of the army and navy. It is to them we may reasonably look for most valuable aid in determining many points about which there is still no small uncertainty, and often much discrepancy of opinion. I would therefore urgently appeal to our *confrères* in the public service to scrutinize with the utmost vigilance, and faithfully to record, all details relating to the history of the first few cases of epidemic outbreaks which come under their observation, without regard to hypothesis or theory. They have much in their power; their labours, if conducted according to the strict rules of induction, might soon render stable and defined what is now fluctuating and ambiguous, and shed light on much that is hazy or obscure.

My intention was to proceed now to expound some of the most remarkable features and relations of epidemics in respect of their geographical distribution and chronological recurrence; but this subject must be reserved for another occasion; and I shall close this present paper with briefly discussing a problem of State medicine which is at once very interesting and important.

VII. Have epidemic diseases been kept out from countries by any external means of protection or defence? This question, viewed historically, and tested simply by the results of past experience, has never, as far as I am aware, been examined on a broad basis by medical writers; yet it is one of primary moment both scientifically and practically. It is impossible to discuss it as fully as it deserves to be, from the want of sufficiently ample and accurate evidence; but the mere attempt to open it up for future inquiry may perhaps be useful. No one has ever dreamed of trying to stop the progress of influenza from one district or region to another; for it seems to be carried on the wings of the wind, and to defy all artificial arrest or control. And yet, if we may trust many narratives, the disease seems to have been distinctly introduced into small islands, far distant from other land, and rarely visited, as St. Kilda, Tristan d'Acunha, &c., by foreign vessels, after whose arrival it speedily followed.

It is more than doubtful whether the immunity of certain countries from some of the exanthemata can fairly be ascribed to the quarantine precautions resorted to for their exclusion. The extent to which this family of epidemic diseases is diffused over different sections of the world's surface seems to differ a good deal. I do not remember having ever heard or read of any of our colonies, or other distant lands to which our countrymen have gone, being exempt from measles, for example, or from its common accompaniment, hooping-cough. Scarlatina, although very generally present also at times, seems to be not so universally diffused. In India it is little known; and probably it is comparatively rare in some other regions in the East; but information is wanting. Smallpox appears to have penetrated almost everywhere, whether attempts are made to exclude it by quarantine or not. Wherever samllpox occurs, there varicella is, it is believed, also occasionally seen. The only notable exemption on a considerable scale appears to be the case of our Australian colonies, and of the adjacent islands of Tasmania and New Zealand. Their immunity however from an epidemic visitation of smallpox can scarcely be attributed entirely to the successful operation of their extrinsic defences; for it is well-known that, on several occasions, a few scattered cases have occurred both at Sydney and at Melbourne, but fortunately without manifesting any decided tendency to spread, and the disease happily ceased to exist in a short time, although a large proportion of the population is unprotected by vaccination.

It would be difficult to prove if any, and what, countries were preserved from invasion of the plague, when the disease was endemic in Egypt and the Levant, by the prophylactic measures employed for their protection. On the one hand, the disease exhibited at times no tendency to spread beyond a limited area around the seat of its original uprise; and, on the other hand, the quarantine measures employed were often so notoriously lax and irregular that no reasonable reliance could be placed on their efficiency. The mere circumstance of the exemption of a place or of a country from visitations of the disease was no proof that this was due to the precautions used for its exclusion. For half a century and more before the abolition of quarantine against the plague in our own country, Holland had continued to be quite as free from any traces of the distemper as Great Britain, although there had been unimpeded

inter-communication on the part of the Dutch with the Levant all the time: and so it was in many other places. It had been long notorious that it was often next to impossible to prevent the clandestine arrival of smuggling and other coasting vessels from the Barbary coast at Malta for many years prior to the outbreak of the disease there in 1813—an outbreak which has never been explained, although at the time it was attributed, on merely conjectural evidence, to direct importation by a smuggler from Egypt. With regard to the subsidence and subsequent disappearance of the pestilence from Syria and Lower Egypt during the last five-and-twenty years, this phenomenon cannot, it is obvious, have been owing to *external* measures of defence, viz., against its introduction *ab extra*, as these were the very regions of its home growth; whatever credit may be due to the *internal* sanitary precautions which were being adopted throughout these countries, about the same time, under the surveillance of the able French physicians who were located in different parts of the Levant.

The whole history of yellow fever during the present century affords continuous evidences that no system of artificial restrictions and extrinsic precautions hithertoemployed will suffice to exclude that disease. No example, as far as I know, can be cited of any country which is known to have been once visited by the pestilence being entirely protected from its recurrence. It may be needless to refer to the experience of any West India island or of any place on the mainland of the Caribbean Gulf, where the fever is believed to be indigenous, but where nevertheless measures of stringent exclusion are still from time to time adopted. The same want of success has attended their use in places at a distance from this great central region of infection. Within the last thirty years, our colony of Bermuda has been four times the seat of epidemic invasion, notwithstanding the vigilance of an officer of health appointed expressly to prevent the apprehended evil. Nor have the colonies of other countries, which adopt more rigorous measures of exclusion, fared better. The groups of the Cape de Verde and Canary islands afford a proof of this. When the outbreak of yellow fever occurred in 1845 at Boa Vista, one of the former, after the visit of H.M.S. *Eclair*, it was maintained at the time, by one party of the disputants in respect of that disastrous event, that the Cape de Verdes were not liable to invasions of the disease, and that the fevers which are endemic there are only of the tropical remittent type. Very little was then known of the nosological history of the island. In the following year, and at the same season, Teneriffe, one of the Canary group, ten degrees further northward, was attacked, the epidemic lasting for three or four months; and again, in 1847, Palmas, another of the same group, was the seat of a similar invasion (*Parliamentary Papers relating to Quarantine*, 1860, p. 16). In

1861 the Cape de Verdes were suspected of being infected; and in 1862 both these islands, and also Teneriffe and Palmas, are known to have suffered from the genuine "vomito" for several months at the end of the year and the beginning of the following one, as stated in this Journal for July, 1869, p. 219. If such facts as these can be gathered from the meagre scraps of intelligence which find their way into the public prints (for no systematic information is ever given by the authorities of Spain and Portugal on such matters), it would seem that yellow fever is far from being so infrequent in these island groups as has been imagined. Even the mother countries of these colonies, Spain and Portugal, have not escaped, notwithstanding their extraordinary vigilance and the extreme rigour of their precautions. After repeated outbreaks in several of their maritime towns during the first ten or fifteen years of the present century, a more stringent quarantine system was adopted, and with successful results, it was for a time believed. But in 1819, the pestilence reappeared in Cadiz, Seville, and other places in Andalusia, and again in 1820 and 1821. In the latter year, Malaga also suffered, and a fatal outbreak happened at Barcelona, Tortosa, and in Catalonia, at the opposite end of the peninsula. In 1823, a partial outbreak occurred at Passages on the coast of Biscay, connected with the arrival of a vessel from Havanna, after a ten days' quarantine; a case very similar to that at St. Nazaire, at the mouth of the Loire, in 1861. In 1828, the disease reappeared at Gibraltar after an absence of fourteen years. In the autumn of 1851, there was a partial outbreak at Oporto, and again in 1856, when Lisbon also became affected. Next year, the pestilence was fatally epidemic in the Portuguese capital, besides appearing partially in other towns of the kingdom. About the same time, Ferrol, Corunna, and other places on the coast of Gallicia in the north-west of Spain, were also affected. These repeated occurrences, which are known to have taken place in spite of the endeavours by the authorities to stifle all information about the appearance of any of the diseases which they seek to exclude, suffice to show how inefficacious have been the prophylactic measures hitherto chiefly relied on in the Iberian peninsula to guard their country from the occasional appearance of yellow fever.

Still more conspicuous has been the failure of all external defences against the incursions of the cholerific poison. It would be tedious and quite unnecessary to revert to the history of the earlier European visitations in connection with this subject further than to say that, at the first International Quarantine Conference held in Paris in 1851, the strongest evidence that was adduced by any of the delegates in proof of the credited efficacy of restrictive measures as a means of defence was the example of the small island of Elba, which escaped in 1836-37, and also in 1848-49, while the plague was

widely spread over the Italian peninsula. Our own Isle of Man was equally fortunate on both occasions, although Liverpool and other places in Lancashire and Cheshire with which it continued freely to communicate were severely smitten. In the last epidemic, the immunity of Greece, when the disease prevailed more or less extensively in the countries around, was regarded by many persons, and among others by the Constantinople Conference, as a strong proof of the efficacy of quarantine as a means of protection in epidemic seasons. Greece appears to have enjoyed a remarkable exemption on former occasions also as well as in 1865; and indeed, with the exception of 1854, when the French and English troops quartered in the Piræus and (two or three months subsequently) Athens, were attacked, we have no knowledge of the disease having visited the country previously, even during the first European visitation in 1831-37, before any regular quarantine system was established in the kingdom. Almost all the islands of the Ægean Archipelago seem to have been equally fortunate; nor do even the large islands of Crete and Cyprus appear to have ever encountered an epidemic invasion of cholera. The hilly or mountainous character of their geography, the small size of the towns, the dispersed and scanty population, and their comparatively inconsiderable intercourse or traffic with other lands, may all have had something to do with the immunity they seem to have enjoyed, although certainly these circumstances alone do not suffice in themselves to account satisfactorily for the fact. But Greece and Greek islands are not the only places which have more or less completely escaped. Other Mediterranean islands have been seldom and sparingly visited by cholera, from the influence of causes which are very imperfectly understood. Corsica has suffered very little, and Sardinia was not, I believe, invaded till 1854. It is much to be regretted that (to the best of my knowledge) no authentic or complete narrative has been published in Italy of the true history of the disease as it appeared in Sicily in 1865-66. It has been confidently asserted that the island continued to be quite free from any traces of the pestilence throughout the former and during the first half of the second of these years, until indeed the landing at Palermo in the autumn (1866) of a body of troops from Naples where the disease was then existing; and that, from that date, the outbreak and spreading of the disease commenced. If such was exactly the case, the inference is certainly very reasonable that the two events, viz., the invasion of the malady and the arrival of the military from the mainland were connected, the one with the other, as cause and effect. Unhappily for the sake of truth, no accurate information has been published, and we are left to unauthentic rumour rather than to sure evidence to form our opinions. Upwards of two years ago I wrote in the pages of this Journal (for July, 1868, p. 15), "It is not possible from the want of reliable evidence to determine whether the disease had not been in the island previous to the landing of troops from Naples at Palermo, at the end of September. More than one of its seaports had been quarantined by other Mediterranean ports in the course of the summer (1866); and the extreme rapidity with which the disease appeared, according to report, at Catania and other places far distant, at Palermo, after the landing of the troops which were accused of having imported the pestilence, is not to be overlooked. Very speedily, nearly the whole of the island seems to have become infected." No contradiction or explanation of these statements has been given. On more than one occasion, the towns of Messina and Reggio, on the opposite sides of the narrow straits separating Sicily from the mainland, were mentioned in the public journals of this country as places where cases of the disease had occurred; but we are still, as far as I know, without the means of affirming or denying the rumours. The total want, too, of any evidence respecting the state of the public health in Palermo, or in any other places in the island prior to the occurrence of the first cases, presents a grievous obstacle to any trustworthy conclusions. Unless such *lacunæ* be in future filled up, it will be in vain to look for the establishment of truth. However ignorant we are about the cholera history of Sicily in 1865-66, we have complete knowledge of that of Malta in those years, and also of what occurred in 1867 when, in spite of the unusual vigilancy of the authorities and the extraordinary rigor of the precautionary measures which had been adopted since the cessation of the disease in 1865, the pestilence again found its way into Valetta.

The scope of the foregoing illustrations has, it will be observed, been simply historical. How far the appearance and development of epidemic diseases in many countries *can be* traced to importation *ab extra* by the introduction of infected persons or things from infected countries, or can be prevented by quarantine, is another problem which must be treated by itself, and which I shall afterwards have occasion to consider. At present, I would merely invite the reader's attention to the question with which we started, "Have epidemic diseases been kept out from countries by any external means of protection or defence?"

CHOLERA:

ITS DIFFUSION, PROPHYLAXIS, SYMPTOMS, AND TREATMENT.*

By JOHN MURRAY, M.D.,
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As cholera is regularly advancing from India through Russia and the Baltic ports towards England, and as the disease has pursued this course on several previous occasions, its arrival here may be shortly anticipated. As August and September are the months during which the last three epidemics prevailed in England, it is now time for every precaution to be taken to guard against its attack. As I have had opportunities of seeing this disease in India every year since 1833, I have been requested to relate the results of my experience in that country; and I willingly do so in the hope that it may be useful.

It is important that information should be given regarding the early symptoms of cholera and its treatment, as a delay of two or three hours will in many instances allow the disease to advance to a stage where those remedies will prove powerless, which, if exhibited sooner, would have controlled the disease and saved the life of the patient.

Much of the difficulty in understanding cholera has arisen from writers only acknowledging the presence of the disease when it has reached collapse—the third stage. I beg to state that the opinions to which I shall call attention on most of the important points in this disease are supported by a very great majority of the medical officers now in India, as shown in a recent report submitted by me to the Indian Government, in which the opinions of 505 medical officers are carefully tabulated.

In no malady are there more marked and characteristic symptoms than in cholera; but the symptoms, which are only found in one stage, are so dissimilar to those induced by the poison in an earlier stage or mild attack, that the connection has by many been overlooked. The value and importance of the early detection of the presence of the poison cannot be over-estimated, as we all know that, after a certain stage, the vital organs are rendered, by the absent or diminished absorption of the stomach, little sensible to the action of remedies; their functions

* Read in the Medical Section at the Annual Meeting of the British Medical Association in Plymouth, August 1871.

are paralysed by the presence of the poison. It is in preventing the supervention of this stage that most life has been saved; and the physician who can soonest detect the presence of the poison will be most successful in his practice.

The earliest symptoms that can be recognised are those of *malaise*, viz., depression of spirits, want of appetite, torpidity of the bowels, and desire for stimulants. That the *malaise* I have described arises from the presence of the poison of cholera in the system, is an opinion strengthened by the fact that, during an epidemic attack, when this feeling exists, the action of a purgative—especially salts—will almost always develop the other symptoms of cholera, and also by the circumstance of its frequent occurrence in men who have left the infected locality in apparent health and have been attacked within one or two days.

To the symptoms of *malaise* succeed *diarrhœa*, nausea, and vomiting; the urine is scanty; the stools light-coloured, then colourless, like rice-water, with occasional cramps, heart-burn, and slight headache. The countenance is dark and the eye-balls congested. This is followed by *collapse*, great prostration of strength, burning in the epigastrium, congee or rice-water vomiting and purging, with cramps and suppression of urine; cold clammy perspiration, feeble pulse and cold breath, broken voice and shrunk and livid face. When reaction takes place, the burning pain in the epigastrium disappears, the restlessness subsides, the stools become coloured, urine is secreted, warmth returns to the palms of the hands, the countenance improves, the pulse becomes stronger, and sleep ensues.

In many instances the disease does not progress beyond the stages of *malaise* or *diarrhœa*. The poison appears to be digested or eliminated by the *vis medicatrix naturæ* through the natural functions of the system —hence the great importance of supporting these, and avoiding their being overtaxed, exhausted, or depressed.

Cholera is a specific disease, caused by the presence of a specific poison in the system; it multiplies or is reproduced; it must be vital and amenable to the ordinary laws which regulate other specific poisons, modified by the peculiar structures of the body which are chiefly affected. It must enter the body through some of the ordinary channels. Before health can be restored, it must be eliminated either in a vital state or after being decomposed or digested. Although we are unable, in many instances, to trace the manner in which the poison enters the system, yet in others the channel is evident and the source clear. The instances where cholera has appeared after drinking water contaminated by cholera discharges from wells, streams, tanks, or vessels, are numerous and well authenticated. The most convincing case which has come under my own notice occurred during the Hurdwar epidemic of 1867, when a pilgrim arrived at Joogh Kullan in the Punjaub, on the 28th April. He

was taken ill with cholera near a well and died next day. He had bathed, and his soiled clothes were washed in an adjoining pond. Other parties who afterwards visited the well and the pond for water and ablution were attacked two days afterwards; and up to the 15th May, fifty-three were attacked in that village, of whom twenty-seven died. There had been no cholera there for the previous twenty-four years.

I have witnessed numerous cases and heard of many more where the poison must have entered through the lungs; those attacked having been in the vicinity of cholera patients, or of their evacuations. It is difficult to show that the skin is the channel of communication, because the poison may be disseminated in the surrounding air, and thus enter by the lungs. But there are many instances on record of the disease appearing after the wearing of clothes worn by cholera patients; and Dr. MacLean mentions that the last case that occurred in the attack at Malta in 1865, was that of a washerwoman who stole a soiled shirt and wore it.

The poison leaves the body through the same channels by which it enters; viz., the bowels, the lungs, and the skin. Its presence in the discharges from these organs is recognisable in most instances by the smell—a mawkish, sickening odour, well known to those who have seen much of the disease. In the earlier stages the poison appears to be destroyed, or digested, without exciting any active symptoms, and this is the safest way of nature getting rid of it. The first *active* symptom is diarrhœa; and here we can be useful, as the system during this stage is amenable to remedies.

I shall now allude to some facts that are important in a practical point of view, as tending to promote or restrain the diffusion of the disease. I carefully examined the Records in the Medical Department in Calcutta from the year 1814, and found that, with the exception of the Delta of the Ganges, where the disease is never absent, its appearance in an epidemic form is limited to certain seasons and extending over three months in the year, and that this period of the year varies in different parallels of longitude; but there were sharp occasional attacks at other seasons.

The period of observation in Europe is shorter. It is probable that the disease may have appeared in former ages under the general names of plague, black death, or sweating sickness; but all the ordinary symptoms of what are called sporadic cases have frequently been found. The epidemics of 1849, 1854, and 1866, occurred in August and September; and we may infer that the arrival of the disease which is now gradually advancing therefore will be at the same season, and it becomes our duty to prepare for its advent.

All experience of the course of the disease teaches us that it rages with the greatest intensity, and proves most fatal where people are collected in great numbers—where there is crowding and filth, defective ventilation and impure water; and that it is aggravated by want and bad food. It is here that the circumstances of civilisation bear on the propagation of the disease.

Great benefit has arisen from a careful attention to the laws of sanitation, which have been assiduously studied of late years by many zealous investigators of this branch of medical science. Sanitation is of the greatest importance in the precautionary treatment of this epidemic, in ameliorating the virulence of the attack, restraining its dissemination, or warding it off entirely. On sanitation there is no difference of opinion; and both the medical profession and the public are aware of its importance, and its direction is in good hands.

There is another point of no less importance in the precautionary treatment, on which the profession was not in former years so unanimous: I allude to the contagious nature of the disease, meaning thereby the transmissibility, directly or indirectly, of the specific poison from a sick to a healthy person. I am unable to state the exact proportion of those who consider cholera as non-communicable in Europe or America; but in India there were only five out of five hundred and five medical officers who stated that it is not communicable. There is a slight divergence of opinion regarding the channel through which the communication takes place, and the changes undergone by the poison after leaving the diseased person, but none as to a diseased person being a source of danger if admitted into a healthy locality. The precautionary measures indicated by this opinion consist in the exclusion and isolation of the affected, and the removal of the healthy from the infected locality.

There has been a great diminution of mortality from epidemic cholera in India, particularly amongst the European troops, and in the gaols, since precautionary measures founded on these indications have been carried out. In many cases, cantonments and prisons have remained free from attack whilst the disease has raged amongst the population of the surrounding districts, and the mortality has been materially diminished in most of the stations where it did break out; still, in several stations and gaols where the sanitary arrangements have been carried out by most efficient officers, there have been very severe and fatal outbreaks of the disease under my own superintendence, particularly in the cantonments of Peshawur, Kohat, Allahabad, and Gwalior, and in the Central Prison at Agra, which has long been celebrated for the completeness of its sanitary arrangements.

There was great difficulty in carrying out measures for its exclusion even from the gaols and cantonments; and it is questionable if they were ever strictly observed in large towns. In England the disease must enter by the sea-ports; and the admission of ships with the disease on board is more under control, and should be restricted, and the

cases treated on board special cholera hospital-ships. I question the possibility of perfect isolation of any hospital on land, unless it be an island. When the disease has once appeared in a port or town, special cholera hospitals should be established, with which there should be no unnecessary communication. Small detached buildings, tents or huts, are best for cholera cases: the air is purer, and the depression from the sight of the suffering and death of other patients is diminished.

The removal of troops or prisoners is a simple process in India. The supply of tents and means of carriage is ample, and the surrounding country open. In small villages the inhabitants desert their homes and live in the open air; and many who have the means leave the large towns. The precautionary arrangements in towns have been limited to the construction of special cholera hospitals and the general distribution of medicines. People have generally a great aversion to leaving their homes and entering a cholera hospital; but these hospitals were generally filled with travellers and poor people. Restrictions were occasionally attempted in infected localities in the towns, but it is questionable if they did any good. The free use of disinfectants applied to the evacuations has been employed with marked advantage.

In thus prominently calling attention to the communicable nature of cholera, I do not wish it to be inferred that I think it as communicable as many other diseases, or that there is *very* great danger in attending on the sick, especially in Great Britain, where decomposition is comparatively less rapid than in India. I have attended thousands of cases, and only contracted the disease three times, which yielded readily to early treatment. The exemption of hospital attendants, where prompt treatment is available, is remarkable; and the number of relatives and friends who attend in private houses and escape, or only have very slight indisposition, is generally observed. This should encourage all to neglect no duty to suffering humanity, while at the same time all unnecessary exposure should be avoided and every means used to prevent the disease being disseminated to others.

I now come to the most important branch of the subject—the medical treatment, in which the practice has been most uncertain in late epidemics, whilst some are of opinion that it is of little or no use in the advanced stages. This latter statement is not correct, and has a most injurious tendency by depressing the spirits of the patient and destroying his confidence in the physician. This confidence is a most powerful means of supporting hope when life is wavering in the balance. I have seen a patient's life hang on the expression of my face when, in deep collapse, his pulse was imperceptible at the wrist.

My object in giving in detail the symptoms of this well known disease is to enable the early presence of the poison in the system to be recognised, as it is here that medical treatment is most efficacious. It is of little importance that analogous symptoms may have been induced by other causes if the remedies used be not injurious to health, and if they check the farther development of this *disease*, which in an advanced stage is almost beyond the control of medicine. It is our duty to assist Nature and to relieve pain. In the stage of *malaise* the poison is thrown off without any violent or very prominent symptoms by the natural functions of the system. Our task here is to support nature, avoid indigestible food and depressing causes. The only medicine that I have found useful in this stage is a little quinine every day. The subsequent indications of the treatment are to remove the abnormal symptoms as they appear, of which the most early is *diarrhœa*. The first indication is to *check* this and bring the case back to the stage of *malaise*, then remove the cause and restore the natural secretions. Irritating or indigestible food in the bowels is the most frequent cause of diarrhœa; and should this not previously have been discharged in the evacuations, it should be removed and a recurrence of the looseness guarded against, as I have always found it the most powerful exciting cause of collapse. I have found this best carried out by a combination of opium with carminatives in the form of a cholera pill, composed of one grain of opium, two of black pepper, and three of assafœtida. It appears to check the looseness and stimulate the secretions. This pill does no harm if needlessly administered. It should be repeated should the looseness continue. It will cure most cases, and in all restrain the symptoms until regular medical advice can be procured. This is a most important point in the use of this simple remedy. It may be distributed to every house and be available in a few minutes, whereas the delay of a few hours may allow the disease to advance beyond control. I know no better remedy for this stage. These pills have been distributed in tens of thousands in the towns and villages of India with most satisfactory results. Some surgeons prefer red to black pepper, and others add camphor to the opium and assafœtida, and report favourably of the combination. They are distributed in the dispensaries, and are placed in the charge of the police in India. In this country, similar arrangements might be made.

In collapse, our power is limited by the circumstance that the vital organs are insensible to the ordinary action of medicines. Experience shows that opium, astringents, and alcohol, lie inert in the collapsed stomach, though these are the ordinary remedies for pain, looseness, and debility. It is also my experience that the free use of these remedies at this stage causes death, either by preventing reaction, or by causing local complications should reaction appear.

There is another cause of death which is not generally understood, but which it is in the power of all sufferers or attendants on the sick to check or to prevent. I allude to the extreme danger of assuming the

erect posture, or even of sitting up in bed, during collapse, or the earlier stage of reaction. I have seen myself, and I have heard of many cases, where fatal syncope instantly followed sitting up in bed or rising to go to stool.

It must be borne in mind, while indicating the treatment in this stage, that the poison of the disease is contained in the congee evacuations in an active form, and also that the first sign of reaction is coincident with the appearance of bile in the evacuations. The dilution of the irritating contents of the bowels and the restoration of the watery particles of the blood are indicated and best fulfilled by frequent small quantities of cold water, to which a little soda or carbonate of ammonia may be added with advantage. In protracted cases I have seen decided benefit from the use of Liebig's extract of meat, made fresh and given frequently. I have also seen most marked benefit from the exhibition of hot saline enemata given after each motion. In some instances this has acted like magic, the symptoms subsiding after one injection, but in many others it has been powerless. I have thought that the artificial supply of Nature's own remedies in the stage of *malaise*, the secretion of which is suspended by the action of the poison as the disease advances to collapse, might be useful, and the results in a few cases in which they were used previous to my departure from India were highly satisfactory; seven out of nine having recovered, and the two fatal cases having been pulseless and dying before the remedies were used: these remedies were gastric juice and bile, in the form of acidulated pepsine, fifteen grains, and inspissated bile fifteen grains, given alternately every hour. The first dose of bile was followed by vomiting; but bile soon appeared in the evacuations, and mild reaction set in gradually. Shampooing with warm turpentine liniments gives relief to the cramps, and mustard poultices on the epigastrium restrain the vomiting. I have used a little quinine, as I think, with advantage when Nature made an effort at reaction.

I have only time to allude to some of the other remedies which have been highly recommended. I found bleeding unsuccessful in 1833, and I have seen no reason to change my opinion. The practice is generally condemned in India. Calomel I have found inert in collapse, both in large and small doses, and I consider that the benefit attributed by many to its use arises from its being employed instead of spirits or strong remedies. There is danger of its being accumulated in large quantities when reaction takes place. Sulphuric acid and acetic acid are less dangerous; but I have not seen decided benefit from their use. I have not found advantage from ammonia, except when added in small quantities to the cold water. I found, in 1833, that the transfusion of saline fluids into the veins caused most hopeful reaction; but it was only temporary, and this is the general result of numerous trials made by other medical officers in India. Brandy I consider dangerous in proportion to the quantity given in the stage of collapse, and opium as decidedly poisonous in this stage. Chloroform, though it may give temporary relief, tends to induce dangerous head symptoms on reaction. Astringents are not beneficial. Purgatives are dangerous in the earlier stages, and not useful in collapse; they are generally condemned in India. Heat has been extensively tried by warm baths, but the fatigue entailed is dangerous. It has been tried in the form of hot-air baths, but the result has not been encouraging.

When reaction takes place, rest and careful nursing will complete the cure where collapse has not lasted long; but in protracted cases, in addition to these remedies, medical treatment may be required for low fever, uræmia, or local complications, regulated by the ordinary rules.

One of the inducements which have led me to write this paper is the hope that the confidence in the treatment of cholera, particularly in its earlier stages, which my long experience has given to me, may be imparted to others through this National Association; and be extended to the people of this country, and help to ward off panic when the disease appears amongst us. I also wish to condemn the use of violent remedies in cases that appear to be hopeless, knowing the dangers they induce, and to recommend the milder expectant treatment which has in many instances been followed by favourable results.

INTERIM REPORT BY THE WORKS COMMITTEE OF THE EDINBURGH AND DISTRICT WATER TRUSTEES.

28th November 1870.

OF this date a letter, signed "Physician," appeared in the 11th Nov.
Scotsman newspaper, impugning the quality of the water of 1870.
St Mary's Loch, and predicting various injurious results to the communities of Edinburgh, Leith, and Portobello from the use of water drawn from that source. This letter was followed of this date by another letter bearing the same signa- 14th Nov.
ture, and in which the objections of the writer to the water of St Mary's Loch for town supply were reiterated and extended.

The unconcealed hostility of purpose which pervaded both of these letters, deprived them of all claim to consideration as the opinions of a citizen animated, as the writer professed to be, by a pure spirit of zeal for the public interest. Nor did it need much acquaintance with the subjects on which the writer professed to offer an opinion to see, not only that he looked at the subject from a wholly erroneous standpoint, but that his science was as faulty as his knowledge of the facts upon which he ventured to speak. At the same time the Committee were not ignorant that an attempt was being industriously made to prejudice the public mind against the St Mary's Loch Scheme by objections such as were advanced in the letters of the "Physician." It therefore appeared to the Committee that these documents afforded a legitimate opportunity for dealing with and confuting the objections which they contained, and which might impose upon, and give some uneasiness to, persons not acquainted with the subject.

A copy of these letters, with the Report of the Committee, dated 26th October last, having appended Dr Frankland's analysis of the water of St Mary's Loch, the Talla and the Heriot, and his reports dated 5th, 6th, and 14th October were accordingly furnished to each of Dr Frankland, Dr Alexander Wood, and Dr Littlejohn, and they were requested to favour the Trustees with their views as to the "Physician's" objections to the water of St Mary's Loch, and as to the suitability of that water for the supply of Edinburgh, Leith, and Portobello.

It was considered desirable that each of these gentlemen should be asked to give an independent report, without communication each with the other of any kind, and the reports now submitted have been accordingly so prepared. It may be added that none of these gentlemen has had any communication with the members of the Works Committee or with each other; that they have not seen each others reports; and that the instructions under which they have all acted were in effect those which are contained in Mr Marwick's letter to Dr Frankland, which was in the following terms :—

EDINBURGH AND DISTRICT WATER TRUST,
CITY CHAMBERS, EDINBURGH, 16*th November* 1870.

DEAR DR FRANKLAND,

I enclose a print of a Report which the Works Committee of the Edinburgh and District Water Trust have made to the Trustees. You will observe from it that the Trustees have resolved to go to Parliament this year for an additional supply of water from St Mary's Loch, and the requisite notices are now being given with that view.

On the publication of the Parliamentary notice, a lengthy letter, from a gentleman who subscribes himself "Physician," appeared in the *Scotsman* newspaper, reflecting upon the quality of the water of St Mary's Loch as unfit for town supply. I enclose a copy of that letter, cut out of the newspaper dated 7th November (but which appeared on 11th November), and also a second letter from the same "Physician," dated 12th November (but which appeared on 14th November), and I am to ask you to favour me with your views, in detail, as to the statements contained in these letters.

Obviously the quality of the water is the *first* consideration; an adequate supply of the best obtainable water is the *second*; and the matter of cost is the *third*, though in every view a most important consideration. This is the order in which the Trustees have dealt with the several considerations above indicated. But if the "Physician" be anything like correct in his statements the Trustees have gone wrong in the very first and cardinal matter of the *quality* of the water which they propose to introduce. It is the last thing the Trustees would think of to "shirk" the objection which has thus been stated, and you will give them a frank and unbiassed judgment on the question which has thus been raised.

Will you be so good as return me the newspaper slips along with your observations. The Trustees are to meet on Friday, but I presume I may not expect to hear from you in time to report your letter to that meeting.

Yours faithfully,

J. D. MARWICK.

The Committee regret exceedingly the great expense to which the Communities are put in obtaining such reports. But the responsibility for this must rest with those whose misrepresentations it is the duty of the Trustees to prevent as far as possible from prejudicing the public mind.

If anything further were necessary to show how completely the opinions of "Physician" and those who hold his views are opposed to the science of the present day, the report of the Royal Commissioners on the water supply of the metropolis and other large towns, presided over by the Duke of Richmond, and presented to both Houses of Parliament in 1869, should be conclusive.

DAVID LEWIS, *Convener*.

I.—REPORT BY DR FRANKLAND.

ROYAL COLLEGE OF CHEMISTRY,
315 Oxford Street, London, W.,
Nov. 21*st*, 1870.

SIR,

I HAVE read in the *Scotsman* of the 11th and 14th inst., two letters, signed "A Physician," in which the writer expresses himself opposed to the introduction of the St Mary's Loch water into Edinburgh on the following grounds :—

1st, Because it is not spring water.

2d, Because it is soft water, and therefore unwholesome.

Whilst fully recognising the importance of a consideration of the points thus raised by "A Physician," I cannot but express my regret at the spirit which pervades his letter. The writer appears to assume that the Town Council are blindly prejudiced in favour of the St Mary's Loch scheme, and are determined at all hazards to force it upon the community. He says, "It is thus by no means easy to determine the process by which the Town Council have reached the conclusion which they desire to force upon a too supine communtiy ; and it becomes even painful, in as far as it might lapse into a consideration of possible motives, to pursue the inquiry." I have but little personal acquaintance with the Town Council of Edinburgh, but with reference to this water scheme, I cannot too strongly express my conviction that these unworthy reflections are peculiarly inapplicable to that body, for in all my interviews with them and their officers no attempt whatever has been made to influence my judgment in favour of any one of these schemes. I have been left to pursue my investigations in the most free and unfettered manner, and every one with whom I came in contact seemed to be actuated only by the desire to secure for Edinburgh the best available supply, whatever its source might be.

With several of the reasons upon which "A Physician" grounds his first objection I cordially agree. *Ceteris paribus*, I prefer for the supply of a town the water of deep-seated springs to lake water, and lake water to river water.

Spring water, as he rightly contends, preserves a more uniform temperature during the whole year; having percolated through a great thickness of earth or rock it does not require artificial filtration, and it is almost invariably more free from organic matter than either of the other descriptions of water. Spring water is therefore more palatable—in summer on account of its low temperature, and at all seasons by reason of its brilliancy and freedom from colour and organic matter. But it is exceedingly difficult to obtain spring water of unimpeachable quality and in sufficient quantity for the supply of a city like Edinburgh, since such water is obviously liable to be contaminated with certain impurities which are often present in the soils through which it percolates. Thus spring water is generally hard, and it not unfrequently contains the washings of highly manured fields, as is the case, for instance, with the Comiston water at present supplied, although in small proportion, to Edinburgh. This water is gathered from several springs rising in the midst of highly cultivated fields, to which, amongst other things, Edinburgh night-soil is sometimes applied. That a population should wash its excrements with its drinking water is an arrangement which "A Physician" would doubtless condemn both in the interests of the public health and of common decency; nevertheless the Comiston water is "sapid, sparkling, and aerated," and is, I believe, a favourite drinking water in Edinburgh—so little can popular instinct be trusted in these matters.

"A Physician" does not clearly indicate the source or sources to which he would have the Water Trust go for their supply of spring water, but I gather from his remarks that he considers the Talla or the Heriot would be a suitable source. The spring waters which fed these rivers at the time my samples were taken for analysis, were of excellent quality, and either of them would form a most desirable supply for Edinburgh; but if "A Physician" had regarded these sources from an engineering point of view, such as even an outsider may obtain, he would have seen that to render either of these rivers available for the supply of the city, it would have to be impounded in a storage reservoir, and would thus become, to all intents and purposes, lake water. Moreover, as I have already pointed out to you in my Report, the condition of these rivers, at the

time of my visit of inspection, was very exceptional, a long continued drought having dried up all surface drainage, and left nothing but spring water to feed them; but from the character of the ground around them, I have no doubt that, after heavy rain, these rivers are filled with water of a very different quality (indeed I was informed, on good authority, that it is on such occasions frequently muddy and coffee coloured). Instead of clear, colourless, and tasteless spring water, they would then yield a turbid, brownish, peat-flavoured beverage. Such flood waters lose much of their objectionable qualities by storage in large lakes, especially if these be deep natural lakes with pebbly beaches. The water of such lakes is also likely to be of more uniform quality than that stored in smaller and shallower artificial reservoirs, the soft beaches of which, unless protected at great expense, are constantly being lashed into mud by the waves breaking upon them. On these grounds, and also because the samples of St Mary's Loch water were likely to represent, more nearly than the river samples, the average quality of the supply from this source, I came to the conclusion that the St Mary's Loch scheme was preferable to the Talla or Heriot scheme, although the water of these rivers was, at the time the samples were taken, of markedly superior quality. In truth, the ultimate decision of the Edinburgh and District Water Trustees must have been rendered difficult by a veritable *embarras des richesses*, as the quality of each of the samples is excellent for every domestic purpose, including drinking.

Whilst I thus agree in principle with "A Physician's" first ground of objection to the water of St Mary's Loch, I entirely dissent from his second—that it is soft and *therefore unwholesome*. The question of the comparative wholesomeness of soft and hard waters has of late years received the earnest attention of our highest medical and chemical authorities, and whilst, on the one hand, opinions have differed considerably as to the wholesomeness of hard water, on the other there has been and now is an almost complete unanimity as regards the wholesomeness of soft water.

During the recent sittings of the Rivers Commission in Scotland, the almost unanimous testimony of the medical officers of the chief Scotch cities and towns has been in favour of soft and opposed to hard water. The Board of Health also collected evidence on this point some years ago, and expressed their opinion "that the presence of lime and other mineral matter deteriorates the wholesomeness and value of waters for the purposes of drinking." In his evidence before the recent Royal Commission on Water Supply, Dr Lyon Playfair considers the substitution of soft for hard water in towns to be a hygienic improvement; but with regard to hard water he says, "I do not think that mere hardness is of much importance as to health; in extreme cases I would consider a hard water injurious to health. In some cases hard water might prove injurious, as in calcareous affections and in dyspepsia."

Dr Parkes, F.R.S., Professor of Military Hygiene in the Army Medical School at Netley, who has had great experience on the effect of water upon the health of our troops, gave evidence before the same Commission as follows:—"The carbonate of lime waters appear in some cases certainly to produce some effect upon health, for instance, dyspepsia, and they do not agree with some class of persons, whereas to others they appear to be quite harmless. In Germany especially, there is a very strong opinion in certain parts that the phosphate of lime calculi, and calculi generally, are more common in districts where the inhabitants use very hard waters, but in this country the evidence is so far negative. I think that 16 or 20 degrees of hardness would be certainly prejudicial. I think that very probably it might disagree with a great many persons. *In all cases we would prefer a soft water if it were possible to obtain it.* I think the hardness should not exceed 10 or 12 degrees, if possible. At the same time I should wish to state that one would prefer water free from that even, if it were possible to get it. *Question.*—Have you known any instances where troops have been located in districts where they have been using water of a moderate degree of hardness, and have suffered when they have been removed to a district where the water was soft? *Answer.*—I have never seen any reports of that kind. *Question.*—Are you aware whether a certain quantity of carbonate of lime may not in many cases be rather beneficial than otherwise to health? *Answer.*—I think that is again very doubtful. The fact is, that almost all kinds of food contain enough lime for the supply of the body."

Mr Simon, F.R.S., the Medical Officer of the Privy Council,

said before the same Commission—"I do not think that the question of a few grains of lime in a gallon of water can be regarded as a very important sanitary question. *Question.*—For drinking purposes, probably a little hardness would add to its life and pleasantness to the taste? *Answer.*—I would not quite say that; I have found soft waters, or at all events, hard water artificially softened, very agreeable. As regards drinking purposes, I am not sure of any important difference, but am inclined to prefer the soft water. As regards health, my bias is in favour of soft water, but I cannot say that I think the case established against hard water that it acts injuriously on health."

Sir Benjamin Brodie, F.R.S., said that he had no reason to think that the use of soft or hard water, as a drinking water, produces any difference of effect upon health.

The late W. A. Miller, M.D., F.R.S., Professor of Chemistry at King's College, stated that so far as observation goes, he thought it a matter of indifference whether hard or soft water be drunk.

Dr Angus Smith, F.R.S., says—"I should think that the tallest people I have seen in Great Britain are to be met in soft water districts; for instance, in Cumberland and, probably, in Aberdeen. I may say that the tallest people I have seen in Great Britain are in Aberdeen, which is a very soft water district."

It was certainly not without careful consideration of this subject that the medical officers in Her Majesty's service sanctioned the use of distilled water, absolutely free from lime, for drinking purposes in the navy.

I might easily greatly extend this list of authorities, but a sufficient number have been quoted to show that, whilst there is a difference of opinion as regards the salubrity of hard water, there is a singular unanimity amongst some of our highest medical and chemical authorities as to the wholesomeness of soft; it is, therefore, not to be wondered at that "A Physician" should have been driven to seek for authorities abroad. With one exception—that of Professor Johnston, whose opinion, given more than a quarter of a century ago, is now somewhat antiquated—all his evidence in favour of hard water is derived from Continental sources. Admiring, as I do, the vigorous manner in which scientific research generally is prosecuted in Germany and France, and feeling strongly the comparative inactivity which has prevailed in this country as regards scientific discovery for many years past, I shall not be deemed prejudiced when I say that, in respect of that section of sanitary science which is devoted to town drainage and water supply, our Continental brethren are at least a quarter of a century behind us. They are only now beginning to imitate, in their large cities, sanitary works of this description which have been executed here, even in most of our small towns, long ago. Many of the Continental authorities cited by "A Physician" are men of the highest culture in abstract science; but, in regard to water supply, they have not had the advantage of the great experience enjoyed by our own medical officers and chemists; and it is, therefore, in no way derogatory to them if their opinions on these matters are regarded as formed upon a narrower basis of facts than our own.

But why need "A Physician" travel abroad for his illustrations when Scotland ought to furnish him with abundance of evidence of the baneful effects of soft water? The following list contains the names of some of the chief cities and towns of Scotland with the hardness of the water with which they are respectively supplied; and, for comparison, the hardness of St Mary's Loch water is added:—

	Hardness in 100,000 parts.
St Mary's Loch (at foot), ...	2·16
Glasgow (Loch Katrine), ...	0·88
Selkirk, ...	3·41
Peebles, ...	4·04
Paisley, ...	4·16
Greenock, ...	1·91
Aberdeen, ...	2·03
Perth, ...	2·92

If the theory of "A Physician" were correct, the people of Perth and Aberdeen, who have been, for a long series of years, imbibing such remarkably soft water, ought to exhibit a marked deficiency of lime; but in my recent visits to those towns I neither saw any symptoms of such deficiency nor heard any complaints from the Medical Officers of Health who were interrogated as to the sanitary condition of the people. Surely "A Physician" of three-score years and ten, who has evidently paid considerable attention to the public health, could have found no difficulty in tracing the dire effects which he attributes to

soft water in the ricketty children and small-boned adults of these towns if his suppositions were correct; but the fact is, that the amount of lime consumed in our food is always in excess of that required for the wants of the system, since a considerable quantity is always expelled in the urine, and there is no such thing as effete lime. This being the case, there would be nothing improbable in the supposition that hard water, instead of being acceptable to nature, may impose an additional burthen upon her by compelling her to get rid of a surplus quantity. And with regard to rickets, that disease, as "A Physician" doubtless knows, has nothing to do with a deficient supply of lime to the system, for Lehmann has shown that it consists in the non-assimilation or abnormal expulsion of lime from the system—the urine of ricketty children containing considerably more lime than that of healthy ones. I am not prepared to dispute the statement that a broken limb will set more quickly under the administration of carbonate of lime to the sufferer; but it would surely be better to administer the proper quantity to the patient alone, rather than to compel a whole community to take the medicine.

"A Physician" objects to the form of my analytical results; but had he followed the modern developments of water analysis he would have been aware that chemists, instead of estimating such saline constituents as chlorides of potassium and sodium and sulphates of potash and soda, the relative quantities of which in any moderately-pure water have no bearing whatever upon its suitability for domestic supply, now prefer to expend their labour upon the determination of those constituents which either constitute organic impurities or disclose the previous history of the water as regards its association with objectionable matters, such as sewage or putrifying animal substances. Unless "A Physician" is a homœopathist he could not possibly have any interest in harmless saline substances which, exclusive of carbonate of lime, are contained in the St Mary's Loch water only in the proportion of at most 1·4 grain to the imperial gallon. I would now only add that the water of St Mary's Loch is well ærated and free from that vapidity which "A Physician" fears.

With regard to the water fleas, I need hardly say that these are perfectly harmless insects. I have rarely found them absent from lake and impounded waters in summer, and they would almost certainly be present in the impounded water of the Talla or Heriot; nevertheless I quite agree with "A Physician" that it is desirable that these insects should be removed from the water before its delivery to consumers. Their removal from my samples was effected, before analysis, by straining through cotton gauze; on the large scale it may be performed by straining through fine wire gauze, or passage through a stratum of gravel, but I greatly prefer filtration through sand, which not only removes all suspended matters, but also diminishes considerably the amount of peaty matter held in solution. In connection with the filtration of such waters, I will here quote the remarks contained in the first report of the Rivers Pollution Commission, published at the commencement of the present year. Speaking of the Manchester water supply, which is very similar both as regards source and quality to that which you would obtain from St. Mary's Loch, they say, "The position of the gathering ground and reservoirs is such as to preclude the possibility of excremental pollution, and consequently the water is not filtered before delivery. Nevertheless, even in the case of a water of such undoubted purity, we would recommend filtration, since the best waters from gathering grounds are liable at times to be turbid, and although turbidity in these cases has not the significance which it possesses when the muddy water is derived from sources exposed to excremental pollution, yet the use, for drinking purposes, of water containing suspended matters is reasonably objected to by consumers, and may even drive them, in some instance, to the use of clear and sparkling water derived from dangerous sources."

In conclusion, then, I can only repeat the opinion to which my analysis and an inspection of the gathering grounds have led me, viz., that, after efficient straining or filtration, the water of St Mary's Loch will, in every respect, be well adapted for the supply of Edinburgh, and will, if so used, constitute one of the best water supplies in the United Kingdom.

I am, Sir, your obedient servant,

E. FRANKLAND.

J. D. MARWICK, Esq., Town-Clerk, Edinburgh.

II.—REPORT BY Dr. ALEXANDER WOOD.

EDINBURGH, 19th *NOVEMBER* 1870.

To J. D. MARWICK, Esq., City-Clerk.

MY DEAR SIR,

I have to acknowledge receipt of your letter of 16th instant, a pamphlet entitled "Interim Report by the Works Committee of the Edinburgh and District Water Trustees," dated 26th October last, a copy of the *Scotsman* newspaper of Friday, November 11th, 1870, containing a letter, occupying two columns and a half of that newspaper, headed "Quality of St Mary's Loch Water," and signed "A Physician;" also copy of the *Scotsman* newspaper of date 14th November, containing another letter with the same heading and signature, and you request me "for the information of the Trustees, to favour you with my views in regard to the water, as reported on by Dr Frankland, and also my opinion on the objections stated to it by 'A Physician.'"

In the remarks I shall make on these documents I shall adopt the familiar style of a letter, rather than the baldness of a mere scientific Report. I do this because it will enable me better to explain to non-scientific readers the various points which I am required to bring before them.

With the attacks on the Town Council, and the calculations as to the probable expense of the St Mary's Loch Water Scheme, with which "A Physician" commences his letter, I have nothing to do, further than to remark that they seem to me to invalidate altogether his assumed character of an impartial observer, actuated only by zeal for the citizens in undertaking the laborious duty which he imposed on himself.

The first remark made by "A Physician," lying within his proper province and mine, is the exception he takes to the testimony of Dr Frankland and Dr Macadam, on the ground "that both of these" (gentlemen) "are merely chemists, and have no title to speak as physicians or physiologists." The report states that the Committee sought the opinion of Dr Frankland as the very highest authority on such a question, by the advice of the Professor of Chemistry in the University of Edinburgh, and I think most people who know anything of the subject will endorse his recommendation.

It is the province of the chemist to ascertain the various purposes to which water is applied in a town, and the various qualities of water which can best subserve these purposes, and then by applying his analytical skill to such specimens of water as may be submitted to his judgment, to determine how far they fulfil the required conditions. This Dr Frankland has very fully and clearly done in the report which you have sent to me; and if the physician who has ventured to impugn his opinion be really entitled by his superior knowledge to do so, it is greatly to be regretted that, instead of writing anonymously, he had not favoured the public with his name, that it might have had due weight in this important enquiry.

"A Physician" speaks of "lake water as mawkish, unaerated, of unstable temperature, and prone to be loaded with rotten vegetable organisms." I presume that all physicians, from the time of Celsus downwards have agreed with that Father of Medicine in the comparison which he thus makes of the different qualities of water. "Aqua levissima pluvialis est; deinde fontana; tum ex flumine; tum ex puteo; post hæc ex nive aut glacie; gravior his ex lacu; gravissima ex palude." It is therefore to be regretted that engineers have not yet devised a satisfactory method of bringing water from springs directly to the mouths of the thirsty inhabitants of large towns,

but are obliged to have recourse to contrivances for storing it. "A Physician" has neglected to show that the artificial lake into which Mr Leslie proposed to receive and detain the Heriot waters would not transmit them to us as mawkish, as unaerated, of as unstable temperature, and as prone to be loaded with rotten vegetable organisms as St Mary's Loch, or any natural loch in the kingdom.

On the superiority of a natural reservoir to an artificial one, all are agreed. Mr Bateman says (p. 18 of Interim Report by the Special Committee of the Corporations of Edinburgh, Leith, and Portobello, dated 24th October 1868):—"St Mary's Loch certainly appears a very desirable source of supply. Any natural lake in which the water is good, and which can be converted by artificial works at its outlet to purposes of water supply, is preferable to artificial reservoirs, which require time to construct, and which may be liable to other objections. Again, the same gentleman states (p. 32 of Report dated 4th February 1869):—"There is this great advantage, however, in the St Mary's Loch scheme over the others, that you have to incur very small outlay to obtain the amount of storage required, and that the natural beach which the Loch itself now possesses will preserve the water from all injury and discoloration which would attend it if stored in large artificial reservoirs with clay or gravelly slopes, rising or falling according to the state of the weather, and exposing fresh surfaces for abrasion by the action of the wind." Mr Gale says (Interim Report, dated 26th October 1870, p. 19), "Water drawn from artificial reservoirs, subject to considerable fluctuations in level, will always be less pure than that drawn from a natural loch." Dr Frankland says (Ibid., p. 27), "The advantages of a natural loch with a pebbly beach over an artificial reservoir are great," &c.

You may remember that in the evidence I gave in 1869 before the Committee of the House of Lords, I spoke of the frequent yellow colour and loaded character of the Edinburgh water—a part of my evidence which was impugned by the other side. However, when Mr Leslie, the engineer of the Water Company, was called on their behalf, he fully corroborated my statement, and explained the frequent pollution of our drinking water by the effect produced by wind or rain in agitating the surface of the artificial ponds, and causing the contained water to act on the puddled banks. This, to a large extent, you will escape, by resorting to a natural rather than an artificial reservoir for your supply. No proper comparison can therefore be made between the *spring* water of the Talla or the Heriot and the *lake* water of St Mary's; indeed, as regards the former, Dr Frankland, in a passage which "A Physician" does not quote, says, "It owes a good deal of its superiority to filtration through a porous stratum which would be submerged in the proposed reservoir, and would then cease to have any effect on the water" (Interim Report, p. 27).

"A Physician," quotes, with approbation, a sentence from Professor Wanklyn, in which he expresses a hope "that he will see the time when our towns, ceasing to be supplied with the waters of rivers or lakes, will derive their drinking-water from deep springs."

Attempts in this direction have not proved so successful as to lead us to set a great value on the advice of Wanklyn, or of the "Physician" who endorses it. Bischof, in his 'Chemical Geology,' says of deep springs, that the temperature of the water is usually high in proportion to the depth of the spring, while "A Physician" very properly highly values cool water. Besides, we learn from the same authority that these deep springs extract more from the rocks and are richer in mineral constituents than those nearer the surface. I doubt if even "A Physician's" love for saline impregnation would cause him to regard this as an advantage. In proof of this statement, I may add that water from some of our deepest sources, as the Artesian Well at Grenelle, is often so charged with saline matters as to be perfectly undrinkable.

In fact the character of the water depends much more on the strata through which it passes than on the depth from where it is taken.

In his second letter "Physician" makes very merry over the "water fleas" which Dr Frankland removed from the St Mary's Loch water before analysing it. Surely it must be assumed ignorance on the part of so learned a "Physician" to conceal that an artificial lake, such as must be made to store the waters of the Heriot or the Talla, is quite as likely to teem with insect life as the natural loch which he condemns.

The great point, however, which a "Physician" seeks to make against the St Mary's scheme in his letter, is the alleged absence in the St Mary's Loch water of certain salts of lime which are essential to the necessary hardness and solidity of the bony framework of our body. It is scarcely possible to deal seriously with this statement, or by argument to convince the old ladies whom it has terrified that there is small risk that the boys of the next generation by drinking the waters of St Mary's Loch will be found lying like jelly fish at the tops of the closes devoid of shape and form, instead of as at present driving their hoops against our legs and their "shinties" into our eyes.

Town water is chiefly required for four purposes

1. Drinking.
2. Cooking.
3. Washing.
4. Manufacturing.

"A Physician" assumes that the first of these is the most important, and in some respects it is. Water, however, may be quite wholesome although not very palatable, and we have it on undoubted evidence, which I do not quote because I do not think it necessary to parade authorities on every occasion, that water so loaded with nitrites as to be almost poisonous was clear, cool, sparkling, and agreeable to the taste so as to be sought after by persons from a distance for these qualities. No one will assert that there is anything necessarily present in soft water to make it injurious; and as to those salts, the absence of which a "Physician" so much deprecates, I shall have something to say immediately. It will be admitted that for cooking, soft water is very superior; and it is surely of some consequence to the poor especially that their food should be presented to them as well cooked and with as little expenditure of time and fuel in the cooking as possible;* and the same holds good in regard to washing, and economy in the use of soap. Soft water is also superior to hard for all manufacturing purposes, except brewing, where the presence of salts of lime, especially the sulphate, is of great importance. In his second letter "A Physician" says—"Am I in error in believing that brewing is the chief manufacturing operation in Edinburgh, and certainly that which uses the largest quantity of water?" I suspect if "A Physician" were to push his inquiries a little further he would find *first*, that there is no water available for Edinburgh supply that would be really useful for brewing pale ale, except that contained in the private wells of the brewers; *second*, that were such water available it would be most unsuitable for either drinking or domestic use; *third*, that it would very soon render useless the self-feeding boilers, now so common, by the copious salts of lime which, when boiled, it would deposit.

The Edinburgh breweries are all situated in one district, or rather in one line, by reason of the hard water which is got there; and the public water, when used by them at all, is employed for washing, not for brewing.

I have thus shown that for most purposes, except drinking, a soft water is superior to hard.

The subject of drinking water must however be considered at greater length by reason of the stress "A Physician" lays on it.

It has been calculated that an adult human being consumes about three-fourths of a ton of water annually. The duty which this water discharges is threefold.

* The evidence of so practical and experienced a "*Chef*" as the late M. Soyer given before the General Board of Health on the saving of food effected by cooking with soft water is very interesting. The general result was that two-thirds of any food cooked in soft water produced as great a result as three-thirds in hard.

1. It removes effete solid matter in solution by the secretions.
2. It maintains equilibrium in the animal temperature by its evaporation.
3. It supplies some of the fluid which enters so largely into the composition of all the tissues of the body, and also introduces, in solution or otherwise, a certain portion of those saline matters which minister to its growth.

For the first of these duties it has to exert a considerable solvent power, and the purer it is the greater its solvent power must be; and it is also plain that the purer the water the more readily will it be vaporised so as to effect the second object of its introduction into the body.

In regard to the third, or the introduction into the body of certain salts, and notably of those of lime, "A Physician" greatly over-estimates its power. Carbonate of lime is only held in solution by water containing an excess of carbonic acid, which makes a very sparkling and clear water, but decidedly hard. On parting with its excess of carbonic acid the lime becomes insoluble, and in that state cannot be so readily conveyed by the blood to those parts where it is needed, as when it is presented held in solution by the albuminous ingredients of our food. Nor, though the water from granite or millstone grit, and hard oolite rocks, is nearly free from lime, do we find that the inhabitants of these regions suffer from any softness of the bony structure.

The idea which forms the main object of "A Physician's" letter is very contemptuously dismissed by the Duke of Richmond's Commission, which reported in 1869. This is all the more remarkable as there existed an evident bias in that Commission to uphold the advantages of hard water, and they would therefore gladly have availed themselves of such an argument, had they thought it contained a single grain of truth.

They report as follows—

"Some eminent chemists" (not physiologists or physicians) "have contended that a moderate quantity of carbonate of lime is not only harmless, but that it is actually useful in supplying material for the bones of man and animals. Considering, however, the much larger quantities of carbonate of lime taken in our solid food, such an additional source of supply would seem to be unnecessary."

This is really the fact, it is not to the water we drink that we are mainly indebted for earthy salts. Except chloride of sodium (common salt), the other salts, including those of lime, occur naturally in sufficient quantity in most of the articles which are used as food. Thus, in the typically perfect foods presented by nature to the young of various animals, and which contain all that is necessary for the growth of the body, as milk and the egg, a due proportion of the lime salts is found; and the casein of milk possesses a power, which water has not, of holding phosphate of lime in solution, which necessarily facilitates its conveyance to those parts of the frame where it is required, especially during the period of growth. It is a curious fact, that the bones of the bird are supplied with the material for their formation, not by water, but by the oxygen passing through the pores of the shell of the egg uniting with the free phosphorus in the yolk, to form phosphoric acid, and this again uniting with the lime of the shell to form phosphate of lime.

It is no use for "Physician" to quote the casual expressions of various authors as to the value of the lime salts in water. It is demonstrable that all which a safe-drinking water contains would go but a small length in furnishing the supply which the system demands, and that it must therefore be obtained from other sources. Thus Dr Smith, Medical-officer to the Poor-law Board, says (*Practical Dietary*, page 29):—

"Phosphorus, in combination with *lime*, magnesia, soda, potash, &c., is found in most animal and vegetable foods."

Dr Golding Bird, an author whose authority no physician will undervalue, says:—"To show how readily the supply of earthy phosphates* is derived from without, I have calculated from the best authorities the quantity of these salts which exist in an ounce of eleven different articles of food:—

* Phosphate of ammonia, ammonio-phosphate of soda, phosphate of lime, ammonio-phosphate of magnesia.

Articles of Food.	Phosphates in one ounce.	Authority.
Pease (Picum Sativum)	9·26 gr.	Braconnot.
Maise (Zea Mais)	7.2 ,,	Gorham.
French Bean (Phaseolus Vulgaris)	4·7 ,,	Braconnot.
Wheat (Triticum Hybernum)	4·7 ,,	Liebig.
Beans (Vicia Faba)	4·7 ,,	Einhoff.
Potatoes (Solanum Tuberosum)	2·35 ,,	Liebig.
Rice (Oryza Sativa)	1·92 ,,	Braconnot.
Milk	1·2 ,,	Liebig.
Artichoke (Helianthus Tuberosus)	0·96 ,,	Payer and Braconnot.
Vetchling (Latherus Tuberosus)	0·756 ,,	Do.
Beef	0·33 ,,	Liebig.

In the blood, phosphate of lime is held in solution by the albuminous fluids; and the softening of the osseous tissue which "A Physician" predicts as the consequence of the use of the pure water with which Glasgow is now, and Edinburgh is about to be, supplied, arises, not from the absence of the lime-salt in the water, but from some pathological cause within the body itself interfering with the nutrition of the bone. In fact, learned as "A Physician" is, he is mistaken as to the nature of the disease which he employs to terrify us.

Mark also the inconsistency of "A Physician." He blames you for being guided by the opinions of "mere chemists" in judging of the suitability of water for domestic use, and yet, ignoring the facts which have been collected by Miescher, Gluge, Muller, Owen-Rees, and a host of physicians and pathologists, he prefers to them, in a question of pure pathology, an array of French chemists, who have not the information essential to constitute them authorities on such a subject. Till I read the letter of "A Physician," I thought that every tyro in medicine was informed that the softening of the bones in this disease he refers to was caused not by deficiency of bone-earth, but by its unnatural absorption and removal by another channel. I have no wish to parade authorities, I shall give but three:—1st. A practical Surgeon; 2d. A learned Physician; 3d. A Chemist who has made the chemistry of the human body his especial study.

Mr Solly, the eminent Surgeon of St Thomas' Hospital, writes,—"In this disease the absorbent vessels are unnaturally excited, and the earthy matter of the bone is absorbed and thrown out by the kidneys in the urine," to such an extent that calculus of lime salts is formed in the kidney.

Dr Copland, the learned author of the Dictionary of Medicine, says, "Several earthy preparations, have been recommended with the view of furnishing the materials for the re-ossification of the bones. But, *as the disease is not so much the result of any deficiency of the elements of bone in the nutriment*, as in the failure of the organic, nervous, or vital energy."

Franz Simon, in his *Animal Chemistry*, says that, in this disease, "The phosphates *exceed* the physiological average. . . . . This extraordinary and morbidly increased capacity of the kidneys, for the removal from the blood of those salts which are so essential for the structure of the osseous tissue, and the consequent tendency to the formation of calculi in Rachitic children, is regarded by Walther as a vicarious act of the kidneys in connection with the formation of bone."

Besides, the water of St Mary's Loch is not destitute of lime salts. Its hardness, according to Dr Frankland, is 2·28; the Talla is the same; the Heriot is 4·29; while Loch Katrine is only ·88.

As "A Physician" seems, to a certain though very limited extent, to use the illogical argument that the salts of lime being in certain waters, they must be there for some use, and therefore for the particular use which he seeks to assign to them the following extract from the work of Bischof, already referred to, may comfort him by showing what they actually do. I earnestly hope, however, he will not use it as the means of a fresh alarm to our citizens. It is possible that if the City sewage water derived from St Mary's Loch and sent into the sea contains less lime than that of Crawley, the oyster scalps—the property of the city—may suffer, and an argument on this head would not be so easily answered, nor a panic arising from it so soon allayed, as that founded on the softened bones of young Edinburgh.

The passage is as follows:—

"It is interesting to observe that the substance (carbonate of lime) present in these rivers in the largest quantity, is precisely that which we know with certainty to be continually abstracted from sea water by shell fish. If it is assumed that the mean quantity of carbonate of lime in the Rhine is 9·46, then according to Hagen's estimate of the quantity of its water flowing at Emmerick, it will be found that the quantity of carbonate of lime annually carried into the sea by this river is sufficient for the formation of the shells of 332,539 millions of oysters of the usual size."

While I believe that no one of the various sources of supply of water which have been proposed for Edinburgh, contain a sufficient quantity of the salts of lime to make them injurious as drinking waters, it would be much easier to demonstrate the bad effects of too much than of too little of that ingredient. Dr Paris, an acknowledged authority on diet, says that hard water "has certainly a tendency to produce disease in the spleen of certain animals, especially sheep," and Dr Cleghorn informs us that this is found to be the case among the flocks on the eastern side of the Island of Minorca. A water sufficiently charged with sulphate of lime to make it available for brewing, is an unwholesome water for drinking, producing, in those unaccustomed to its use, indigestion, constipation, alternating with diarrhœa, and often serious arrest of the urinary secretion, and is utterly unfit for boiling food or for washing.

The Selenitic well waters of Paris very generally injure strangers; and Parent Duchatelet and Pinel have both traced the occurrence of persistent diarrhœa in two of the Parisian asylums for the insane to the use of water containing sulphate of lime.

Every groom knows that a horse's coat will become rough by giving it hard water to drink; and Rossignol (Traité d'Hygienè Militaire) gives facts showing that to the same cause was to be assigned the production of bony exostoses (splints) on horses, which ceased to grow on the substitution of soft water,—perhaps too strong a corroboration of Agassiz's opinion that the drinking of water in limestone regions enlarges the skeleton. Professor Gamgee tells us that sheep are also peculiarly affected with calculus in limestone districts—an effect which many authors contend extends also to man.

The old opinion that goitre and cretenism are produced by drinking snow water is now abandoned; and since the rearches of M. Grange, Dr M'Clellan, and others, the prevalence of the salts of lime and magnesia in the water used for drinking has been suspected as the cause. I am aware that M. St. Lager has combated this opinion, but I do not think he has succeeded in refuting it.

I have given these illustrations, however, not so much because I put faith in them, as to show how easy it would be to get up a case on the other side, as strong, if not stronger, against water containing the salts of lime, as "A Physician" has done against softer water. But the fact is, the whole subject has been so thoroughly investigated very recently by the Royal Commission appointed in April 1867 to enquire into the water supply of the metropolis and other large towns, and which reported on the 9th June 1869, that it seems presumptuous in any "Physician" to advance views which have been completely refuted, although previously to the Report of that Commission they had occasioned a good deal of controversy.

The General Board of Health which at that time assumed authority on all matters of a sanitary nature, issued a Report in May 1850 which contained this remarkable passage, "We advise that Thames water, and other water of like quality, as to hardness, be as early as practicable abandoned." The same year, in consequence of a proposal to bring a fresh supply of water into London from the chalk near Watford, Government appointed three eminent chemists (the late Professors Graham and Miller and Dr Hofmann) to report on the quality of the existing and proposed supply, and also on the dictum of the Board of Health. The report of this chemical Commission was given in June 1851.

While the Board of Health had expressed an opinion "that the presence of lime and other mineral matter deteriorates the

wholesomeness and value of waters for the purposes of drinking," the Chemical Commission says no reasonable doubt can be entertained of its salubrity. The shallow well waters of London vary from thirty-two to eighty degrees of hardness, yet these waters have never been pronounced unwholesome. The Duke of Richmond's Commission (1869) took very full evidence on the subject. Among the witnesses examined—Mr Bateman, Mr Hawksley, Mr Rawlinson, Dr Letheby, Dr Lyon Playfair, M.P., Dr Parkes, Mr Simon, Dr Frankland, Dr Odling, Sir Benjamin Brodie, Dr Miller, Dr Angus Smith—a very general opinion seemed to prevail that the quality of the water as to hardness or softness within moderate limits did not at all affect the health.

When, however, they came to consider their relative advantages for culinary purposes, the preference was given decidedly to soft water. The Chemical Commission reports, "The hardness of water forms an objection to its use both in cooking and washing. Mr Rawlinson considers there would be great economy in household purposes by the use of soft water. Dr Miller speaks of its tendency to cause incrustations in kitchen boilers."

The Chemical Commission are also strong on this. They say, "It is in the more careful washing for the middle and upper classes that the advantages of soft water become fully sensible." Again, in the same report it is observed, "In the washing of the person the saving of soap by the use of soft water is most obvious. For baths soft water is most agreeable and beneficial, and might contribute greatly to their more general use. Its superior efficiency to hard water in washing floors and walls is calculated also to promote a greater cleanliness in the dwellings of all classes." The Report then goes on to show the saving of labour and of soap in using soft water, and the possibility with soft water of dispensing with the use of soda, which is very desirable, from its injurious action on the colours of certain prints, and the permanent yellow tinge and weakness of fibre which it may occasion even in white linens when exposed to heat before the soda is entirely washed out, as in ironing.

The superiority of soft to hard water for all manufacturing purposes except the production of pale ale is so universally admitted as to require no comment. Mr Bateman estimates, on data which he gives, the annual saving in Glasgow by the introduction of Loch Katrine water to be £36,000.

The only other part of the letter of "A Physician" which seems to require notice is his assertion that the present scarcity of water, "while it may have produced a little inconvenience, has been in no way detrimental to health," a position which he attempts to prove by the statement that the mortality for the week ending October 22d, 1870, was less than that of the corresponding week of 1869. A severer logic would have thought it necessary first to enquire if the other conditions were similar. Have the labours of the Improvement Trust, and of other new sanitary agencies, had no effect in improving the health of the town?

Mr Simon, the medical officer to the Privy Council, in a more philosophical spirit, observes that the changes induced by insufficiency or impurity of water must not be expected to display themselves in a sudden and violent manner; the results are often gradual, and may elude ordinary observation, yet be not the less real and appreciable by a close enquiry.

The abundant evidence on this subject furnished to the Health of Towns Commission, 1844-5, clearly shows that a deficient supply of water leads to impurities of all kinds,—the person and clothes imperfectly washed, houses and streets uncleaned, sewers unflushed, until the air becomes poisoned with the emanations escaping into it from so many sources; and yet from the same evidence we learn that all this may exist without producing actual disease, but that it induces a state of the system in which the vital powers are lowered, so that if the person is exposed to infection he is more liable to be attacked, and, if attacked, he is less likely to recover.

It was clearly proved before the Committees of both Houses of Parliament on the Edinburgh Water Bill that, whether from deficient supply or imperfect distribution I will not now stay to argue, the people of Edinburgh, especially the poorer

classes, were lamentably ill supplied with water. If the person signing himself "Physician" read the details of that evidence, or if he has visited any of the poor, and seen their distress, I envy not his feelings if he can characterise these sufferings as "some inconvenience but nothing like actual distress." There is reason to fear that the actual disease which he seems to regard as the only test is already upon us, and that ere long the Registrar-General's tables may be turned against him. And yet he weeps over the imaginary soft bones of a coming generation without a tear to spare for the real miseries of the one in which his lot has been cast!

I would have preferred had the style of the "Physician's" letter allowed me to have restricted myself to a simple report on the medical qualities of the water, but I found it impossible to answer your requisition without going somewhat controversially into the arguments of the letter you laid before me to report on; some parts of that letter it is impossible to discuss seriously—ridicule, though no test of truth, is sometimes the best means of combating a particular kind of error.

I shall now submit the conclusions at which I have arrived, viewing the subject dispassionately from the standpoint of a physician.

1. That the Heriot, the Talla, and St Mary's Loch, all afford water of a quality suitable for all the purposes for which it is required in a town.
2. That the Heriot is a better water for general domestic use than the Talla.
3. That the *spring* water of the Heriot and the Talla is superior to the *lake* water of St Mary's.
4. That the construction of the necessary ponds for storing the produce of these springs would go far to deprive them of any superiority which they at present possess, and would certainly render the water supplied from such ponds inferior in some respects to that obtained from the natural lake.
5. That the analysis of the water of St Mary's Loch shows it to contain a sufficient quantity of the salts of lime to

remove all fear of the danger suggested in the letter of a "Physician," especially when the copiousness of the supply of these salts from other sources is considered.

6. That, under these circumstances, it appears to me that the water procurable from any one of the three sources of supply being suitable, the Trustees should be guided in the selection by the questions of quantity, engineering difficulties, and comparative expense, and not by the opinion of any physician.
7. That the present supply of water in Edinburgh is manifestly insufficient, and that the poorer classes especially are not receiving enough to maintain them in a healthy state.
8. That should any epidemic disease appear among us, they will be less able on this account to resist contagion, or to bear up against disease if attacked.

I remain, my dear Sir, faithfully yours,

ALEX[R]. WOOD, M.D.

III.—REPORT BY DR. LITTLEJOHN.

EDINBURGH, 26th *NOVEMBER* 1870.

J. D. Marwick, Esq.,

Dear Sir,

As requested by you I have read the lengthy communication regarding the quality of the water of St Mary's Loch, signed "A Physician," which appeared in the *Scotsman* of the 11th instant, and I proceed to notice the chief objections the writer has to providing Edinburgh with an increased supply of pure water from St Mary's Loch. In reading the letter in question one is struck with the formidable list of authorities adduced in support of the views advanced—authorities, many of them out of date in questions relating to the water supply of large towns, and nearly all of them labouring under the very disqualifications attaching in "Physician's" opinion to Drs Frankland and Macadam being "merely chemists, and having no title to speak as physicians or physiologists." If the letter were intended for popular perusal, surely authorities nearer home, of later date, and more easy of access, might have been named; and if the letter were more particularly intended for a professional audience, the quotations from the Continental authorities should have been carefully given so as to admit of their being verified. In this there could have been no difficulty, since the "Physician," dating, as he does, from Edinburgh, could have had access to any of our public libraries. But under his concealed name it was all the more necessary for him to advance no statement that was not amply supported by correct references. Had his true name been appended to the letter this might not have been required—his standing in the profession and the character of his previous studies possibly constituting him an authority on the subject. As it is, it is difficult to understand why his name should be withheld. Retired from the profession and occupying, as he intimates, an independent position, "Physician" complains that the water of St Mary's Loch is too pure, and specially that it is dangerously deficient in earthy salts. Now the statement that a pure water is not a wholesome water is opposed to the best and most recent authorities on the subject of water supply. I shall take two which should have been referred to by "Physician." They are easy of access, they are the latest publications in this country on Public Health, and have again and again been appealed to in questions of the kind. I allude to Professor Parkes' work on Hygiene and to Professor Mapother's Lectures on Public Health. Dr Parkes (3d edition 1870, p. 83) says —"Although it is not at present possible to assign to every impurity in water its exact share in the production of disease, or to prove the precise influence on the public health of water which is not extremely impure, it appears certain that the health of a community always improves when *an abundant and pure water is given.*" The first part of this quotation is a sufficient rebuke to the dogmatism of "Physician" on the supposed baneful influence of water containing a small amount of earthy salts, while the concluding portion is an ample justification of the efforts made by our unenlightened Corporation to secure for the inhabitants the advantages of an unlimited supply of such water as that from St Mary's Loch. Similar objections, on the score of excessive purity, were urged to the introduction of the Loch Katrine water into Glasgow, and the direst results were prophesied. The objections were successfully and convincingly met by the Glasgow authorities, and the anticipated deterioration of the public health has never been detected. Yet "Physician" speaks in magniloquent language of "Glasgow with its lake water of almost nullity of impregnation having still *(sic)* to await the lesson it has to learn and the experience it has to record, with as yet no great

encouragement to cheer it on in the vastest and boldest physiological experiment on the health of a population that any place or time has heretofore witnessed." If "Physician" had referred to the works in our own language which I have already mentioned, he would have found evidence that the Loch Katrine water had already produced good results; and, as yet, neither the Registrar General nor any of the numerous medical men in the metropolis of the west have notified the appearance of the formidable diseases put down by "Physician" as, in his opinion, dependent on the use of water of great purity. There has been ample time for their occurrence, for, since 1859, this pure water has been supplied in the greatest abundance to the citizens of Glasgow, and, certainly by this time, had such diseases as rickets, &c., been the invariable result of the use of pure water, their appearance in an unusual degree must have attracted attention. But is "Physician" too far removed from the sources of ordinary, not to speak of professional information, not to have been aware that Glasgow, since the introduction of the Loch Katrine water, has passed unscathed through an epidemic visitation of cholera, while Edinburgh, under the care of the late water company, has had to acknowledge a large mortality?

In Dr Parkes' work, there is a chapter devoted to a consideration of the diseases which have been traced to the use of impure water, but there is no mention of diseases caused by the use of pure water. Surely, if any reliable facts could have been appealed to on the subject of the use of pure water as a source of disease, they would have been found duly chronicled in this, the standard work on Hygiene.

"Of late years," says Dr Parkes, p. 65, "an opinion has been expressed that the amount of the mineral substances is of little consequence. This can be true only in a limited sense; there are some mineral substances, such as sodium, chloride, or carbonate, or calcium carbonate, which, *within certain limits, appear to do no harm.* But in the case of other minerals, such as calcium and magnesium sulphates, and chlorides, and calcium nitrate, there can be little doubt that their use is injurious to many persons."

The list of the diseases due to the use of impure water as given by Dr Parkes, is truly a formidable one, and it contains several that have been traced to the saline constituents which "Physician" so much desiderates in the water from St Mary's Loch, and the absence of which from the Loch Katrine water has already produced the best results in Glasgow.

Dr Parkes, p. 66, writes, "Dr Sutherland found the hard water of the red sandstone rocks which was formerly much used in Liverpool to have a decided effect in producing constipation, lessening the secretions, and causing visceral obstructions; *and in Glasgow, the substitution of soft for hard water lessened, according to Dr Leech, the prevalence of dyspeptic complaints*," and Dr Mapother, (second edition, 1867), pp. 111, 112,—"The effects of calcareous salts in water are difficult to recognise, as they are insidious, and take a long period for their development; but a peculiar form of dyspepsia is often assignable to this cause, as well as diarrhœa and subsequent dysentry. *These diseases have become much less frequent in Glasgow since the very pure water supply from Loch Katrine.*"

In all scrophulous diseases, the digestive organs are affected, and while nothing can be stated with absolute certainty on the subject, with such facts before us, there is as much reason to dread the free use of water with a large per centage of saline impregnation, as of water, which, from its purity, cannot be regarded as hard. "Physician" apparently forgets that the water we drink is one of the least important means of supplying the system with saline materials, and that it is in our food that the largest supply reaches the blood, and by that channel all the tissues of the body which demand such constituents. Luckily, the food of our poor, and of our labouring population is singularly rich in these, and not the slightest fear need be felt in the event of the introduction of the water from St Mary's Loch, that scrophulous diseases, well described by "Physician" as "the scourge in our dense populations," will undergo an appreciable increase. Much rather may we not confidently expect that with the introduction of an *abundant* supply of *pure* water, the general health of the community will undergo a marked improvement, and that more especially, with an increased water supply, taken in connec-

tion with the great scheme of city improvement so successfully commenced, the prevalence of scrophulous diseases among the children of our poor population may be effectually checked. Our "Physician" makes merry on the subject of the economical benefit arising from the use of pure water; but in the case of a poor population, this becomes a matter of great importance. Dr Mapother says, p. 99, that "the water with which Dublin will be supplied from the Vartry will be so much softer than that now used, that the daily quantity distributed to the inhabitants will contain ten tons less of lime salts. This will lead to a great economy of soap, for it is calculated that the interest of the cost of the Glasgow water works is repaid by the saving in this particular, and each Dublin citizen will save one penny per week in washing, and something more in the economy of tea, when the supply of soft Vartry water is accomplished." He adds (p. 95), "The advantages of a soft water are briefly, that it is more economical, by the saving of water and soap in ablution and washing of clothes, and it saves fuel by boiling at a lower temperature, and by forming no crust, which must weaken the heating power of the fire. Much labour is required for removing this incrustation. Soft water is more suited for most culinary purposes." This last point is one of much importance to our Scotch populations who live so much on broth, and boiled meat, and who, in their standard diet of porridge, use of course a large proportion of water.

"Physician" allows that the water supply for the present and previous years has been deficient, and admits that "some inconvenience" may have been felt in consequence, but he denies that there has been "actual distress," and certainly any "increased disease and mortality." It is no doubt an easy and a pleasant thing for him in his study chair to give us such comforting assurance, but one naturally asks on what kind of evidence has he arrived at these conclusions? Has he satisfied himself by visitation among our sick poor, or by conference with our Parochial Medical Officers whose duties lead them into the poorest districts of our city? or incapacitated perhaps by age and infirmity from leaving the precincts of his study, has he perused the evidence tendered on these very points by some of the most eminent members of his profession and possibly of "Physician's" own College, should his position in the profession have justified his election as a Fellow? The late Sir J. Y. Simpson, Dr Moir, Vice-President of the College of Physicians, and Dr Alex. Wood, member of the General Medical Council of Education, visited the poorest districts of the city, and assured themselves, by actual inspection and interrogation, not only of the existence of a deficient water supply, but of the sufferings, not to speak of the "inconvenience," felt by the inhabitants in consequence. Surely it was the duty of any one prepared to write so dogmatically on the subject of our water supply to have read the medical and other evidence adduced by the Corporation before the Committee of the House of Lords, and which told so convincingly on the Committee that the question of the deficient water supply was at once settled in favour of the promoters of the Bill to acquire the works, &c., of the late Water Company. But "Physician" says the mortality is actually less this year than what it was last year, and as the scarcity of water has been greater this summer than last, it was to have been expected that there should have been a corresponding increase in the mortality. This apparent discrepancy admits, however, of an easy explanation. In 1869, in addition to a very large and unusual mortality from consumption and other diseases of the chest owing to atmospheric variations, scarlet fever and hooping cough were epidemic, and there can be little doubt that the mortality from scarlet fever was largely increased in consequence of the scarcity of water. Ablutions and baths are of importance in the treatment of this disease, and to secure others from the risk of infection it is of great consequence that the supply of water be abundant so as to admit of the washing and renewal of the body and bed clothes. This year, on the other hand, neither scarlatina nor hooping cough has raged in an epidemic form, and the mortality from chest diseases has fallen to its usual standard.

Throughout his letter "Physician" refers only to the drinking properties of water, and apparently forgets the other and important ones it subserves for domestic purposes. Now the scarcity in Edinburgh during the last two summers has never

amounted to a famine so that the thirst of the inhabitants could not be assuaged, but it has, I maintain, been dangerously deficient for other and necessary purposes, and I cannot do better in bringing these comments to a close than by citing the testimony of Professor Parkes as to the consequences of an *insufficient supply of water*. "The consequences either of a short supply for domestic purposes, or of difficulty in removing water which has been used, are very similar. On this point, much valuable information was collected by the Health of Town's Commission in this invaluable Report. It was then shown that want of water leads to impurities of all kinds; the person and clothes are not washed, or are washed repeatedly in the same water; cooking water is used scantily or more than once; habitations become dirty, streets are not cleaned, sewers become clogged; and in these various ways a want of water produces uncleanness of the very air itself."

"The result of such a state of things is a general lowered state of health among the population; it has been thought also that some skin diseases, scabies, and the epiphytic affections especially—and ophthalmia in some cases, are thus propagated. It has also appeared to me that the remarkable cessation of spotted typhus among the civilised and cleanly nations is in part owing, not merely to better ventilation, but to more frequent and thorough washing of clothes. The deficiency of water leading to insufficient cleaning of sewers has a great effect on the spread of typhid and of choleraic diarrhœa; and cases have been known in which outbreaks of the latter disease have been averted by a heavy fall of rain." (p. 63).)

The water of St Mary's Loch is a very pure, and, in my opinion, a wholesome water. It is remarkably free from organic contamination—the importance of which in the production of disease has only been satisfactorily established of late years, and while on a par in this respect with the Loch Katrine water, it possesses this advantage that its proportion of saline ingredients is larger, and therefore that it is still less likely to act injuriously on the leaden pipes used in its transmission, or on the cisterns in which it must be stored by the inhabitants. Its waters are not stagnant. They present a large surface to the pure air of a strictly pastoral region, and while several streamlets enter it, the Yarrow leaves it. As engineers and chemists have again and again pointed out, it possesses as a natural reservoir, great advantages over all artificial collections of water secured by earthy embankments, such as are contemplated in all the other proposed schemes, and from the slight variations in the level of its surface in the dryest seasons, it also contrasts favourably with natural reservoirs such as those of our present water supply, which have been empty for months, and exposed to the disintegrating action of the air and sun.

In the foregoing comments I have carefully abstained from following "Physician" in his purely technical discussions which are only of professional interest, and which like many of his other statements are open to the grave objection that they do not represent the science of the present day. I have appealed to ordinary sources of information which are to be found in the library of every intelligent physician who lays claim to the character of a sanitary reformer. Stronger facts might have been adduced, and more convincing arguments quoted from our Parliamentary Blue-books and other volumes which could only be consulted with some difficulty by the non-professional reader. My aim has been to make such quotations from undoubted authorities, as must carry conviction to any unprejudiced mind.

HENRY D. LITTLEJOHN, M.D.,
Medical Officer of Health.

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PLAN AND RESULTS

OF THE

FIELD EXPERIMENTS

CONDUCTED ON THE

FARM OF JOHN BENNET LAWES, Esq.,

AT

ROTHAMSTED, HERTS.

MAY, 1869.

EXPERIMENTS WITH DIFFERENT MANURES ON **PERMANENT MEADOW LAND.**

THE PARK.

The Land has probably been laid down with Grass for some centuries. No fresh seed has been artificially sown within the last 30 years certainly; nor is there record of any having been sown since the Grass was first laid down. The experiments commenced in 1856, at which time the character of the herbage appeared uniform over all the Plots. Excepting as explained in the Table, and in the foot-notes, the same description of Manure has been applied year after year to the same Plot.

(Area under experiment, about 6½ acres.)

1 acre .. .. .. = (about) 0·40 Hectare .. .. .. or 1·59 Prussian Morgen.
1 lb. (pound avoir.) .. = (about) 0·45 Kilogramme .. .. .. or 0·91 Zollverein Pfund.
1 cwt. (hundredweight) = (about) 51·0 Kilogrammes .. .. .. or 1·02 Centner.
1 ton .. .. .. = (about) 1016·0 Kilogrammes .. .. .. or 20·33 Centner.
1 lb. per acre .. .. = (about) 1·12 Kilogramme per Hectare or 0·57 Zollv. Pfd. per Pr. Morgen.
1 cwt. per acre .. .. = (about) 125·5 Kilogrammes per Hectare or 0·64 Centner per Pr. Morgen.

Plots.	Manures, per acre; fourteenth season—1869.	Produce per Acre, weighed as Hay. Average per Annum; 15 Years 1856-1870.	13th Season; 1868.	14th Season; 1869.	15th Season; 1870.
		Cwts.	Cwts.	Cwts.	Cwts.
1	200 lbs. Ammonia-salts (1) [also, for the first 8 years, 1856-1863, 14 tons Farmyard Manure per acre per annum]	46¾	41¾	61	16¾
2	Unmanured, 1864 and since [for the first 8 years, 1856-1863, 14 tons Farmyard Manure per acre per annum]	41¼	36¾	55¼	15¼
3	Unmanured, continuously	22¾	17¾	38	5¾
4 {a	Superphosphate of Lime (2)	24¼	19¾	40¼	7¼
4 {b	ditto ; and 400 lbs. "Ammonia-salts"	36¾	29¼	45¼	8¼
5	400 lbs. "Ammonia-salts"	28¾	24	35¼	5¾
(3) 6	Sulphates of Potass, Soda, and Magnesia (4); and "Superphosphate of Lime" (previously, 1856-1868 inclusive, Ammonia-salts as Plot 5)	31¼	27¼	36¼	10¼
7	Sulphates of Potass, Soda, and Magnesia (4); ditto	35¾	38	54¾	17¼
(3) 8	Sulphates of Soda and Magnesia (5); ditto	33¾	27¼	46¾	12¾
9	Sulphates of Potass, Soda, and Magnesia (4); ditto; and 400 lbs. "Ammonia-salts"	53¾	39¾	68¾	29¾
(3) 10	Sulphates of Soda and Magnesia (5); ditto; 400 lbs. "Ammonia-salts"	49¼	44¼	57¾	21¼
11	Sulphates of Potass, Soda, and Magnesia (4); ditto; 800 lbs. (6) "Ammonia-salts"	61¼	63¾	75¼	42¾
11a	Sulphates of Potass, Soda, and Magnesia (4); ditto; 800 lbs. (6) "Ammonia-salts; and 200 lbs. each, Silicate of Soda and Silicate of Lime (7)	[illegible]	72¼	78¾	49¾
12	Unmanured, continuously	25¼	24	38¾	11¼
13	Sulphates of Potass, Soda, and Magnesia (4); "Superphosphate of Lime"; 400 lbs. "Ammonia-salts;" and 2000 lbs. Cut Wheat-straw	53¾	55¼	74¾	42¾
14	Sulphates of Potass, Soda, and Magnesia (4); "Superphosphate of Lime"; and 550 lbs. Nitrate of Soda (8)	57¾	69	76¾	56¼
15	none; none; 550 lbs. ditto	36¾	32	53¾	15¼
16	Sulphates of Potass, Soda, and Magnesia (4); "Superphosphate of Lime"; and 275 lbs. ditto	48	51¼	74¼	33¾
17	none; none; 275 lbs. ditto	35¾	29	54¾	19¼
18	Mixture supplying the quantity of Potass, Soda, Lime, Magnesia, Phosphoric Acid, Silica, and Nitrogen contained in 1 ton of hay (commencing in 1865)	33¾	27¼	55¾	14¼

(1) Equal parts Sulphate and Muriate of Ammonia of Commerce.
(2) 200 lbs. Bone-ash, 150 lbs. Sulphuric Acid (Sp. gr. 1·7).
(3) Plots 6, 8, and 10, had, besides the Manures specified, 2000 lbs. Sawdust per acre per annum for the first 7 years, 1856-1862, but without effect.
(4) 300 lbs. Sulphate of Potass, 100 lbs. Sulphate of Soda (200 lbs. 1856-1863), and 100 lbs. Sulphate of Magnesia.
(5) 250 lbs. Sulphate of Soda (500 lbs. in 1862 and 1863), and 100 lbs. Sulphate of Magnesia (Sulphate of Potass also, as on Plots 7, &c., 1856-1861).
(6) 800 lbs. in 1856-7-8; only 400 lbs. in 1859-60-61; and 800 lbs. since.
(7) The application of Silicates did not commence until 1862.
(8) 550 lbs. Nitrate of Soda is reckoned to contain the same amount of Nitrogen as 400 lbs. of "Ammonia-salts."
(9) Average of 10 years only, as the manures specified were first applied in 1859 (previously, 1856-1868 inclusive, Sawdust only).
(10) Average of 11 years only, as these experiments did not commence until 1858.
(11) Average of 4 years only, as the experiment only commenced in 1865.

EXPERIMENTS ON THE GROWTH OF **BARLEY** YEAR AFTER YEAR ON THE SAME LAND, WITHOUT MANURE, AND WITH DIFFERENT KINDS OF MANURE.

HOOS FIELD.

Previous Cropping—1847, Swedish Turnips, with Dung and Superphosphate of Lime, the Roots carted off; 1848, Barley; 1849, Clover; 1850, Wheat; 1851, Barley manured with Ammonia-salts.

First Experimental Barley Crop in 1852. Barley every year since; and, unless stated to the contrary in the foot-notes, the same Manure has been applied year after year to the same Plot.

(Area under experiment, about 4½ acres.)

1 acre .. .. .. = (about) 0·40 Hectare .. .. .. or 1·59 Prussian Morgen.
1 bushel .. .. .. = (about) 0·36 Hectolitre .. .. .. or 0·66 Prussian Scheffel.
1 lb. (pound avoir.) .. = (about) 0·45 Kilogramme .. .. .. or 0·91 Zollverein Pfund.
1 cwt. (hundredweight) = (about) 51·0 Kilogrammes .. .. .. or 1·02 Centner.
1 bushel per acre .. = (about) 0·9 Hectolitre per Hectare .. or 0·42 Pr. Scheffel per Pr. Morgen.
1 lb. per acre .. .. = (about) 1·12 Kilogramme per Hectare or 0·57 Zollv. Pfd. per Pr. Morgen.
1 cwt. per acre .. = (about) 125·5 Kilogrammes per Hectare or 0·64 Centner per Pr. Morgen.

Plots.	Manures, per acre; eighteenth Season—1869.	Produce per Acre. Average per Annum, over 17 Years, 1852-1868. Dressed Corn. Quantity.	Dressed Corn. Weight per Bushel.	Total Straw.	Dressed Corn. 17th Season; 1868.	18th Season; 1869.	19th Season; 1870.
		Bushels.	lbs.	cwts.	Bushels.	Bushels.	Bushels.
1 O.	Unmanured continuously	20¾	52½	12¾	10¾	15¼	13¾
2 O.	Superphosphate of Lime (1)	26¾	53	14	18¼	18¾	18
3 O.	Mixed Alkalies (2)	23¼	52¾	12¾	14¼	18¾	16¾
4 O.	Ditto ; and "Superphosphate of Lime"	28¾	53	14¾	17¾	22½	18¼
1 A.	200 lbs. Ammonia-salts (3)	32¾	51½	18¾	20¼	27¾	27¾
2 A.	200 lbs. ditto ; and "Superphosphate of Lime"	47¼	53	28	37¾	48	41¼
3 A.	200 lbs. ditto ; none ; and "Mixed Alkalies"	35¼	52¼	20¾	25	34¾	36¾
4 A.	200 lbs. ditto ; "Superphosphate of Lime" ; and ditto	46¾	53¼	28¾	34¾	49¾	38
(4) {1 AA.	275 lbs. Nitrate of Soda	37¾	51¼	22	27	32¾	29¾
{2 AA.	275 lbs. ditto ; and "Superphosphate of Lime"	49¾	52¾	30¼	44	48¾	46¾
{3 AA.	275 lbs. ditto ; none ; and "Mixed Alkalies"	38	52	24¼	27¼	33¾	32¾
{4 AA.	275 lbs. ditto ; "Superphosphate of Lime" ; and ditto	50¼	52¾	33	45¼	49¾	44¼
(5) {1 AAS.	275 lbs. ditto ; and 400 lbs. Silicate of Soda (5)	38¾	51¾	23	29¼	34¾	35
{2 AAS.	275 lbs. ditto ; and "Superphosphate of Lime" ; and 400 lbs. Silicate of Soda	49¾	52¾	31	45	49¾	44¾
{3 AAS.	275 lbs. ditto ; none ; "Mixed Alkalies" ; and 400 lbs. Silicate of Soda	40¼	52¾	25¾	36¼	40¼	42¾
{4 AAS.	275 lbs. ditto ; "Superphosphate of Lime" ; ditto ; and 400 lbs. Silicate of Soda	50¼	52¾	33¼	46¾	51¾	47¾
(6) {1 C.	1000 lbs. Rape-cake	45¾	53¾	27¾	37	42¾	41¾
{2 C.	1000 lbs. ditto ; and "Superphosphate of Lime"	47¼	53¾	28¾	35¼	48¼	41¾
{3 C.	1000 lbs. ditto ; none ; and "Mixed Alkalies"	43¾	53¼	27¾	35¾	43¾	38¾
{4 C.	1000 lbs. ditto ; "Superphosphate of Lime" ; and ditto	47¾	53	29¾	36¾	50¾	43¾
(7) {1 N.	275 lbs. Nitrate of Soda	37¼ (10)	52¼ (10)	23¼ (10)	25¾	35¼	38¾
{2 N.	275 lbs. (8) ditto	41¼	52¼	26¼	25¼	38¾	40¾
5 O.	200 lbs. (9) Sulphate of Potass ; and "Superphosphate of Lime" (10)	23¼ (12)	53 (12)	12¾ (12)	15	23¾	14¾
5 A.	200 lbs. (9) ditto ; ditto ; and 200 lbs. "Ammonia-salts"	44¼	53¼	28	36¼	49¾	41¾
M.	100 lbs. each, Sulph. Soda and Sulph. Magnesia ; and ditto	22¼ (13)	53 (13)	12¼ (13)	14¾	16¾	16¾
6 {1	Unmanured continuously	23	52¾	12¾	15¼	14¾	13¾
6 {2	Ashes (burnt soil, turf, and weeds)	23¾	52¼	12¾	16	15¾	15¼
7	Farm-yard dung (14 tons every year)	48	54	28¾	43¾	46¾	47¼

(1) 200 lbs. Bone-ash, 150 lbs. Sulphuric acid (sp. gr. 1·7).

(2) 200 lbs. Sulphate of Potass, 100 lbs. Sulphate of Soda, and 100 lbs. Sulphate of Magnesia (for the first six years, 300 lbs., 200 lbs., and 100 lbs., respectively).

(3) Equal parts Sulphate and Muriate of Ammonia of Commerce.

(4) First 6 years 1852-7, 400 lbs. Ammonia-salts per annum; next 10 years 1858-67, 200 lbs Ammonia-salts per annum; Nitrate of Soda commenced in 1868. 275 lbs. Nitrate of Soda is reckoned to contain the same amount of Nitrogen as 200 lbs. "Ammonia-salts."

(5) The application of Silicates did not commence until 1864; in 1864-5-6 and 7, 200 lbs. Silicate of Soda and 200 lbs. Silicate of Lime were applied per acre, but in 1868, and since, 400 lbs. Silicate of Soda, and no Silicate of Lime; the plots ("AAS") comprise, respectively, one half of the original "AA" plots, and, excepting the addition of the Silicates, have been, and are, in other respects, manured in the same way as the remaining halves; and, for the sake of comparison with the latter, the *average produce* is given for the whole period of 17 years, 1852-1868.

(6) 2000 lbs. Rape-cake per annum for the first six years, and 1000 lbs. only, each year since.

(7) 300 lbs. Sulphate of Potass, 200 lbs. Bone-ash, and 150 lbs. Sulphuric acid (sp. gr. 1·7), without Nitrate of Soda, the first year (1852); Nitrate alone each year since.

(8) 550 lbs. Nitrate of Soda for 1853-4-5-6, and 7; and 275 lbs. only each year since.

(9) 300 lbs. per annum for the first six years, and 200 lbs. each year since.

(10) Ammonia-salts also the first year, but not since.

(11) Average of 16 years only.

(12) Average of 16 years only.

(13) Average of 14 years only.

EXPERIMENTS ON THE GROWTH OF **WHEAT** YEAR AFTER YEAR ON THE SAME LAND; WITHOUT MANURE, AND WITH DIFFERENT KINDS OF MANURE.

BROADBALK FIELD.

Previous Cropping—1839, Turnips, with Farmyard Manure; 1840, Barley; 1841, Peas; 1842, Wheat; 1843, Oats; the last four Crops Unmanured.
First Experimental Wheat Crop in 1844. Wheat every year since; and, with some exceptions, nearly the same description of Manure on the same Plots each year—especially during the last 17 years.

(Area under experiment, about 13 acres.)

1 acre = (about) 0·40 Hectare, or 1·59 Prussian Morgen.
1 bushel = (about) 0·36 Hectolitre, or 0·66 Prussian Scheffel.
1 lb. (pound avoir.) = (about) 0·45 Kilogramme, or 0·91 Zollverein Pfund.
1 cwt. (hundredweight) = (about) 51·0 Kilogrammes, or 1·02 Centner.
1 bushel per acre = (about) 0·9 Hectolitre per Hectare, or 0·42 Pr. Scheffel per Pr. Morgen.
1 lb. per acre = (about) 1·12 Kilogramme per Hectare, or 0·57 Zollv. Pfd. per Pr. Morgen.
1 cwt. per acre = (about) 125·5 Kilogrammes per Hectare, or 0·64 Centner per Pr. Morgen.

Plots.	Manures, per acre; twenty-sixth season—1868-9.	Produce per Acre. Average per Annum, over 17 Years, 1852–1868. Dressed Corn. Quantity.	Weight per Bushel.	Total Straw.	Dressed Corn. 25th Season; 1868.	26th Season; 1869.	27th Season; 1870.
		Bushels.	lbs.	cwts.	Bushels.	Bushels.	Bushels.
0	Superphosphate of Lime (three times as much as on No. 5 and succeeding Plots)	17¼	58¾	15¾	22¼	15½	21¾
1	Mixed Alkalies (twice as much as on No. 5 and succeeding Plots)	15¼	58	14¼	20¼	12¾	16¾
2	Farmyard dung (14 tons every year)	35¼	60	34	41¾	38¾	36¼
3	Unmanured continuously	14¼	57½	13½	16¾	14¼	14¾
4	Unmanured for Crop of 1852, and since; previously Superphosphate (with Muriatic Acid) and Sulphate Ammonia	16½	58¾	14¼	17¼	14¾	15¾
5 (a and b)	Mixed Alkalies (1); and Superphosphate of Lime (2)	17¾	58¾	15¼	17¼	15¾	18¾
6 (a and b)	ditto; ditto; and 200 lbs. Ammonia-salts (3)	27¾	59¼	25¼	28¾	21¾	30¾
7 (a and b)	ditto; ditto; and 400 lbs. ditto	36¾	59¼	36¼	39¾	28¾	40¼
8 (a and b)	ditto; ditto; and 600 lbs. ditto	38¾	59	42¾	46¼	34¼	45¼
9 a	ditto; ditto; and 550 lbs. Nitrate of Soda (4)	36¼	58¼	41¼	47¾	39	45¼
9 b	none; none; 550 lbs. ditto	26¾	56¾	29	27¾	24¼	26¾
10 a	none since 1844; none since 1844; 400 lbs. "Ammonia-salts"	23¾	57¼	22¾	21¾	20¼	21¾
10 b	none (except 1844, '48 and '50); none (except 1844, '48, & '50); 400 lbs. ditto	27¼	58¼	26¾	27¾	19¾	23¾
11 (a and b)	none; "Superphosphate of Lime"; and 400 lbs. ditto	29¾	57¼	28	33¼	22¾	23¾
12 (a and b)	366¼ lbs. (5) Sulphate of Soda; ditto; and 400 lbs. ditto	35	59¼	34	39¾	27¼	35¾
13 (a and b)	200 lbs. (5) Sulphate of Potass; ditto; and 400 lbs. ditto	34¼	59¾	34¾	39¼	27¼	37
14 (a and b)	280 lbs. (5) Sulphate of Magnesia; ditto; and 400 lbs. ditto	34¾	59¼	34	41¾	27¾	35¾
15 a	"Mixed Alkalies"; ditto (6); and 400 lbs. Sulphate Ammonia	33¾	59¼	33	44¾	26¼	38¼
15 b	ditto; ditto (6); 300 lbs. ditto; and 500 lbs. Rape-cake	34¼	59¾	34¾	41¼	27¾	38¾
16 (a and b)	Unmanured in 1865, and since; previously, 1852-64 Mixed Alkalies, Superphosphate, and 800 lbs. Ammonia-salts	39¼ (7)	58 (7)	46¼ (7)	22¾	16⅝	18¼
(8) 17 (a and b)	"Mixed Alkalies"; and "Superphosphate of Lime"	32¾ (9)	59¼ (9)	32¾ (9)	37¼ (11)	16⅝	34¼
(8) 18 (a and b)	none; none; and 400 lbs. "Ammonia-Salts"	17¾ (10)	58¾ (9)	16¼ (10)	18¾ (12)	22¾	19
19	none; Superphosphate of Lime (2); 300 lbs. Sulphate Ammonia; and 500 lbs. Rape-cake	31¾	58¾	30¼	37	23¼	32¾
20	Unmanured continuously	14¾ (13)	57¾ (13)	14¼ (13)	..	13¾	14¼
21	"Mixed Alkalies"; "Superphosphate of Lime"; and 100 lbs. Muriate Ammonia	21¾	58¼	19¾	26¼	20¾	25¾
22	ditto; ditto; and 100 lbs. Sulphate Ammonia	21¾	58¾	19¼	25	15¾	26¼

(1) Since 1858, 200 lbs. Sulphate of Potass, 100 lbs. Sulphate of Soda, and 100 lbs. Sulphate of Magnesia; for Crop of 1857-8, and previously, 300 lbs., 200 lbs., and 100 lbs., respectively.
(2) 200 lbs. Bone-ash, 150 lbs. Sulphuric acid (sp. gr. 1·7).
(3) Equal parts Sulphate and Muriate of Ammonia of Commerce.
(4) 550 lbs. Nitrate of Soda is reckoned to contain the same amount of Nitrogen as 400 lbs. "Ammonia-salts."
(5) For 1858, and previously 1½ time as much.
(6) With Muriatic instead of Sulphuric Acid.
(7) The Manures of Plots 17 and 18 are, respectively, year by year transposed.
(8) Average whilst manured, 13 years, 1852–1864.
(9) Average of 17 years' Ammonia-salts, alternated with Mineral Manures.
(10) Average of 17 years' Mineral Manures, alternated with Ammonia-salts.
(11) Plots 17 had the Ammonia-salts for the Crop of 1868.
(12) Plots 18 had the Mineral Manures for the Crop of 1868.
(13) Average of 16 years, 1852–1867; in 1868, owing to a mistake at the time of carting, the produce could not be ascertained.

The Plots marked "(a and b)" are divided into duplicate portions, "a" and "b," respectively, which are manured alike; excepting that, for the crops of 1864-5-6 and 7, the "a" portions of plots 5, 6, 7, 8, 9, 16, and 17 (or 18), received a mixture of soluble Silicates in addition to the other Manures, but, hitherto, without any material effect; and for the crops of 1868, and since, cut straw (that produced in the previous season) has been applied (instead of Silicates) on the "a" portions of plots 5, 6, 7, 8, 11, 12, 13, 14, and 17 (or 18).

EXPERIMENTS ON THE GROWTH OF **OATS** YEAR AFTER YEAR ON THE SAME LAND; WITHOUT MANURE, AND WITH DIFFERENT KINDS OF MANURE.

GEESCROFT FIELD.

Previous Cropping—1847 and 1848, Clover, Experimental Manures; 1849—1859, Beans, Experimental Manures; 1860, Fallow; 1861 and 1862, Wheat, Unmanured; 1863, Fallow; 1864, Beans, Dunged; 1865, Wheat, Unmanured; 1866, Beans, Unmanured; 1867 and 1868, Wheat, Unmanured.
First Experimental Oat Crop in 1869.

(Area under Experiment, ⅞ acre).

1 acre = (about) 0·40 Hectare, or 1·59 Prussian Morgen.
1 bushel = (about) 0·36 Hectolitre, or 0·66 Prussian Scheffel.
1 lb. (pound avoir.) = (about) 0·45 Kilogramme, or 0·91 Zollverein Pfund.
1 cwt. (hundredweight) = (about) 51·0 Kilogrammes, or 1·02 Centner.
1 bushel per acre = (about) 0·9 Hectolitre per Hectare, or 0·42 Pr. Scheffel per Pr. Morgen.
1 lb. per acre = (about) 1·12 Kilogramme per Hectare, or 0·57 Zollv. Pfd. per Pr. Morgen.
1 cwt. per acre = (about) 125·5 Kilogrammes per Hectare, or 0·64 Centner per Pr. Morgen.

Plots.	Manures per Acre, First Season—1869.	Produce per Acre. 1st Season, 1869. Dressed Corn. Quantity.	Weight per Bushel.	Total Straw.	2nd Season, 1870. Dressed Corn. Quantity.	Weight per Bushel.	Total Straw.
		Bushels.	lbs.	cwts.	Bushels.	lbs.	cwts.
1	Unmanured	36¾	36¾	19¼	16¼	35	9¼
2	Mixed Alkalies (1); and Superphosphate of Lime (2)	45	38¼	24¼	19⅝	35¼	9⅝
3	400 lbs. Ammonia-salts (3)	56¾	37¼	36¾	30	34¼	17¼
4	400 lbs. Ammonia-salts; "Mixed Alkalies"; and "Superphosphate of Lime"	75¼	39¼	54	50¼	36	28¼
5	550 lbs. Nitrate of Soda (4)	62¼	38¼	42¾	36¼	35¼	23
6	550 lbs. Nitrate of Soda; "Mixed Alkalies"; and "Superphosphate of Lime"	69¾	38¼	49¾	50	35¾	28¾

(1) 200 lbs. Sulphate of Potass, 100 lbs. Sulphate of Soda, and 100 lbs. Sulphate of Magnesia.
(2) 200 lbs. Bone-ash, 150 lbs. Sulphuric Acid (sp. gr. 1·7).
(3) Equal parts Sulphate and Muriate of Ammonia of Commerce.
(4) 550 lbs. Nitrate of Soda is reckoned to contain the same amount of Nitrogen as 400 lbs. "Ammonia-salts."

EXPERIMENTS ON THE GROWTH OF LEGUMINOUS CROPS.

I.—Beans, Peas, and Tares.

Experiments on the growth of Leguminous corn-crops, with different descriptions of manure, were commenced in 1847, about nine acres being devoted to the purpose.

Experiments with Beans were continued for thirteen consecutive seasons, to 1859 inclusive; but, during the later years, the crop fell off very much, and the land became very foul.

In 1860 the land was fallowed.

In 1861 a crop of wheat, without manure, was taken.

In 1862 beans were again sown, but with some variation in the manuring.

In 1863 the land was fallowed.

In 1864, and since, beans have been grown, with much the same manures on the same plots, each year, as in 1862.

The general result of the experiments with Beans has been, that mineral constituents added as manure (more particularly potass, and, to some extent, phosphoric acid also), increased the crop very much during the early years; and, to a certain extent, afterwards, whenever the season was favourable for the crop. Ammonia-salts, on the other hand, produced very little effect; notwithstanding that a Leguminous crop contains two, three, or more times as much nitrogen as a Graminaceous one grown under parallel circumstances. Nitrate of soda, however, has produced very striking effects. But Leguminous crops grown too frequently on the same land seem to be peculiarly subject to disease, which no combination of manuring that we have hitherto tried seems to obviate.

Experiments with Peas were soon abandoned, owing to the difficulty of keeping the land free from weeds, and an alternation of Beans and Wheat was substituted; the beans being manured much as in the experiments with the same crop above described.

In alternating Wheat with Beans, the remarkable result has been obtained, that nearly as much wheat, and nearly as much nitrogen, were yielded in eight crops of wheat in alternation with the highly nitrogenous beans, as in sixteen crops of wheat grown consecutively without manure in another field, and also nearly as much as were obtained in a third field in eight crops alternated with bare fallow.

Experiments with Tares were also soon abandoned, for the same reason; beans being at first substituted, with some variation in the description of the manures employed; but of late this experiment has likewise been abandoned.

II.—Red Clover (*Trifolium pratense*).

Experiments on the growth of Clover, with different descriptions of manure, were commenced in 1849, and, with the occasional interposition of a corn-crop, or fallow, have been continued up to the present time. As with beans, the result was, that mineral constituents applied as manures (particularly potass, and, more or less, phosphoric acid also), considerably increased the early crops; whereas ammonia-salts had little or no effect. But since the first few years all attempts to grow Clover year after year on this land have failed to give anything like a fair crop, or a plant that would stand the usual time on the ground, notwithstanding that fresh seed has been sown again and again.

In one year a portion of the land was trenched 2 feet deep; one-third of the manure being applied at a depth of 16 inches, one-third at a depth of 8 inches, and the remainder on the surface.

The general result of the experiments is, that neither ammonia-salts, nor nitrate of soda, nor organic matter rich in carbon as well as other constituents, nor mineral manures, nor a complex mixture, has availed to restore the clover-yielding capabilities of the land.

It is, however, worthy of remark that, in 1854, Red Clover was sown in a kitchen-garden only a few hundred yards distant from the experimental field, on soil which has been under ordinary garden cultivation for, probably, two or three centuries, and it has every year since shown very luxuriant growth; and, after re-sowing three times during the period (in 1860, 1865, and 1868), there is, at the present time, little or no indication of failure.

Lastly, in the winter of 1867-8, small portions of the experimental land were dug, some to the depth of 9 inches, some to the depth of 18, some to the depth of 27, and some to the depth of 36 inches, and sown to the respective depths with different manurial mixtures. From other similarly sized plots the soil was removed to the depths of 9, 18, and 27 inches respectively, and replaced by soil from the same kitchen-garden border, on a portion of which Clover has been successfully grown since 1854, as above referred to. Clover was sown in April, 1868, over the whole of these, and some other portions not so treated; but the plant has, for the most part, died off during the winter, and Clover has been again sown (April, 1869).

EXPERIMENTS ON THE GROWTH OF ROOT-CROPS.

Experiments with Turnips were commenced in 1843. Eight acres, divided into numerous plots, were set apart for the purpose; and the crop was grown for ten consecutive years on the same land ("Norfolk Whites" 1843-1848, and "Swedes" 1849-1852); on some plots without manure, and on others with different descriptions of manure. Barley was then grown for three consecutive seasons (1853-1855) without manure, in order to test the comparative corn-growing condition of the different plots, and also to equalize their condition, as far as possible, by the exhaustion of some of the most active and immediately available constituents supplied by the previous manuring. A new series of experiments with Swedes was then arranged, having regard to the character of the manures previously applied on the different plots, and to the results previously obtained. This second series was commenced in 1856, and is still in progress.

It is impossible adequately to state the bearing of the results in a few words, but the following are some of the most characteristic indications:—

1. Without manure of any kind, the produce of roots was reduced in a few years to a few cwts. per acre; but the diminutive plants (both root and leaf) contained a very unusually high percentage of nitrogen.

2. Of "mineral" constituents, phosphoric acid (in the form of superphosphate of lime) was by far the most effective manure; but, when this manure is used alone, the immediately available nitrogen of the soil is rapidly exhausted.

3. Really large crops of turnips can only be obtained when the soil supplies a liberal amount of both carbonaceous and nitrogenous matter (as well as mineral constituents); and when they are already available within the soil, or are supplied in the form of farmyard manure, rape-cake, Peruvian guano, ammonia-salts, &c., the rapidity of growth and the amount of the crop are greatly increased by the use of superphosphate of lime applied near to the seed.

Experiments on an actual Course of Rotation—Turnips, Barley, Leguminous Crop (or Fallow), and Wheat.

AGDELL FIELD.

These Experiments were commenced in 1848; so that the present crop (1869) is the 22nd experimental one, or the second crop of the Sixth Course

One-third of the land has been continuously unmanured; one-third manured with Superphosphate of Lime alone once every four years, that is for the turnip-crop commencing each course; and one-third manured (also for the turnip-crop only) with a complex manure, as described in the foot-note, No. 2.

In the Second, Third, Fourth, and Fifth Courses, instead of clover, half of each plot was sown with beans, and the other half left fallow.

From half of each of the three plots the whole turnip-crop (roots and leaves) was removed; and on the other half the roots were eaten on the land by sheep, and the uneaten leaves were spread and ploughed in. In the case of all the other crops, the total produce was removed from the land.

The abstract of results given below relates to the portions of each plot from which the turnip-crops were entirely removed; and on which, in the later courses, beans (not fallow) replaced the clover.

(Area under experiment, about 2¼ acres.)

1 lb. (pound avoir.) per acre .. = (about) 1·12 Kilogrammes per Hectare, or 0·57 Zollverein Pfund. per Prussian Morgen.
1 cwt. (hundredweight) per acre = (about) 125·5 Kilogrammes per Hectare, or 0·64 Centner per Pr. Morgen.

Years.	Description of Crop.	Produce per Acre.								
		Plot 1. Unmanured continuously.			Plot 2. Superphosphate of Lime (1), alone, for the Turnip Crops only.			Plot 3. Complex Manure (2), for the Turnip Crops only.		
		Corn (3) (or Roots).	Straw (or Leaf).	Total Produce (4).	Corn (3) (or Roots).	Straw (or Leaf).	Total Produce (4).	Corn (3) (or Roots).	Straw (or Leaf).	Total Produce (4).
	1st Course, 1848-51.									
1848	Norfolk White Turnips	65¼ cwts.	45¾ cwts.	111¼ cwts.	225½ cwts.	107¾ cwts.	332 cwts.	218 cwts.	151¼ cwts.	369¾ cwts.
1849	Barley	44¾ bush.	2983 lbs.	5636 lbs.	29¾ bush.	2111 lbs.	3941 lbs.	29¾ bush.	2088 lbs.	3794 lbs.
1850	Clover (calcd. as hay)	..	..	54 cwts.	..	..	57¾ cwts.	..	..	63 cwts.
1851	Wheat	28¼ bush.	3431 lbs.	5369 lbs.	28 bush.	3371 lbs.	5350 lbs.	29¾ bush.	3582 lbs.	5509 lbs.
	2nd Course, 1852-55.									
1852	Swedish Turnips	26 cwts.	4½ cwts.	30½ cwts.	223¼ cwts.	20¼ cwts.	243½ cwts.	396¼ cwts.	36¾ cwts.	433 cwts.
1853	Barley	34¼ bush.	2430 lbs.	4465 lbs.	29¼ bush.	1873 lbs.	3360 lbs.	39¼ bush.	2664 lbs.	4873 lbs.
1854	Beans	5¾ bush.	1055 lbs.	1443 lbs.	5¾ bush.	1103 lbs.	1534 lbs.	5¾ bush.	1335 lbs.	2065 lbs.
1855	Wheat	30¼ bush.	3619 lbs.	5859 lbs.	30¼ bush.	3585 lbs.	5789 lbs.	37¾ bush.	3947 lbs.	6271 lbs.
	3rd Course, 1856-59.									
1856	Swedish Turnips	32 cwts.	2¼ cwts.	34¼ cwts.	136 cwts.	7¾ cwts.	143¾ cwts.	332¼ cwts.	17¼ cwts.	349½ cwts.
1857	Barley	48¾ bush.	2600 lbs.	5337 lbs.	29¾ bush.	1475 lbs.	2976 lbs.	48 bush.	2430 lbs.	5168 lbs.
1858	Beans	6¾ bush.	1100 lbs.	1518 lbs.	6¾ bush.	1155 lbs.	1595 lbs.	27¾ bush.	1509 lbs.	2387 lbs.
1859	Wheat	33¾ bush.	4030 lbs.	6262 lbs.	31¾ bush.	3930 lbs.	6129 lbs.	30¾ bush.	4510 lbs.	7134 lbs.
	4th Course, 1860-63.									
1860	Swedish Turnips	1 cwt.	(64 lbs.)	1 cwt.	29¾ cwts.	1¼ cwts.	30¾ cwts.	87¼ cwts.	3¼ cwts.	90¾ cwts.
1861	Barley	30¾ bush.	2372 lbs.	4718 lbs.	30¾ bush.	2060 lbs.	3772 lbs.	60¼ bush.	2049 lbs.	7391 lbs.
1862	Beans	29 bush.	1840 lbs.	3661 lbs.	29¾ bush.	2150 lbs.	4049 lbs.	42¾ bush.	3289 lbs.	5990 lbs.
1863	Wheat	44¾ bush.	3467 lbs.	6350 lbs.	34¾ bush.	3090 lbs.	5619 lbs.	46¼ bush.	4097 lbs.	7636 lbs.
	5th Course, 1864-67.									
1864	Swedish Turnips	8¾ cwts.	6¼ cwts.	9¾ cwts.	68 cwts.	4¼ cwts.	72¾ cwts.	176¼ cwts.	8¼ cwts.	185 cwts.
1865	Barley	39 bush.	2134 lbs.	4182 lbs.	33¼ bush.	1613 lbs.	3094 lbs.	47¼ bush.	2595 lbs.	5148 lbs.
1866	Beans	10¼ bush.	1013 lbs.	1599 lbs.	7¾ bush.	978 lbs.	1463 lbs.	20¾ bush.	1999 lbs.	3343 lbs.
1867	Wheat	21 bush.	2143 lbs.	3473 lbs.	19¾ bush.	1966 lbs.	3032 lbs.	23¾ bush.	3003 lbs.	4367 lbs.
	Summary—Average of the 5 Courses, 1848-1867.									
1848, '52, '56, '60, '64	Swedish Turnips	36¼ cwts.	10¾ cwts.	37¼ cwts.	136¾ cwts.	28 cwts.	164¾ cwts.	242¾ cwts.	42¾ cwts.	285 cwts.
1849, '53, '57, '61, '65	Barley	41½ bush.	2538 lbs.	4872 lbs.	30¼ bush.	1815 lbs.	3029 lbs.	44¾ bush.	2732 lbs.	5275 lbs.
1850, '54, '58, '62, '66	Clover, 1850 (calcd. as hay)	..	..	54 cwts.	..	..	57¾ cwts.	..	..	63 cwts.
	Beans	12¾ bush.	1252 lbs.	2078 lbs.	17¾ bush.	1347 lbs.	2161 lbs.	21¼ bush.	2036 lbs.	3439 lbs.
1851, '55, '59, '63, '67	Wheat	33 bush.	3338 lbs.	5467 lbs.	30¼ bush.	3236 lbs.	5200 lbs.	30¾ bush.	3961 lbs.	6344 lbs.

(1) First Course—100 lbs. Bone-ash, and 100 lbs. Sulphuric Acid (sp. gr. 1·7); Second Course—160 lbs. Bone-ash, 120 lbs. Sulphuric Acid; Third, Fourth, Fifth, and Sixth Courses—200 lbs. Bone-ash, and 150 lbs. Sulphuric Acid, *per acre*.

(2) First Course—100 lbs. Pearl-ash, 100 lbs. Bone-ash, 100 lbs. Sulphuric Acid, 100 lbs. Sulphate of Ammonia, 100 lbs. Muriate of Ammonia, and 1000 lbs. Rape-Cake; Second Course—300 lbs. Sulphate of Potass, 100 lbs. Sulphate of Soda, 100 lbs. Sulphate of Magnesia, 160 lbs. Bone-ash, 120 lbs. Sulphuric Acid, 100 lbs. Sulphate of Ammonia, 100 lbs. Muriate of Ammonia, and 2000 lbs. Rape-cake; Third, Fourth, Fifth, and Sixth Courses—300 lbs. Sulphate of Potass, 200 lbs. Sulphate of Soda, 100 lbs. Sulphate of Magnesia, 200 lbs. Bone-ash, 150 lbs. Sulphuric Acid, 100 lbs. Sulphate of Ammonia, 100 lbs. Muriate of Ammonia, and 2000 lbs. Rape-cake, *per acre*.

(3) The quantities given in *Bushels* represent the *Dressed Corn* only.

(4) The "Total Produce" of the Corn-crops includes Dressed Corn, Offal Corn, and Total Straw.

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LETTERS

ON THE

QUALITY OF THE ST MARY'S LOCH WATER;

BY

A PHYSICIAN.

Reprinted, by request, from the Scotsman, with Additions.

Ego pœnitere tanti non emo.
Aulus Gellius.

EDINBURGH:
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1871.

of cholera, than any that have been since published.

LETTERS

ON THE

QUALITY OF ST MARY'S LOCH WATER.

LETTER I.

(From the Scotsman of November 11, 1870.)

Edinburgh, November 7, 1870.

SIR,—In your publication of the 27th October, I observed a report of the proceedings of the Edinburgh Water Trust, a designation, of course, to be accepted as merely another name for our City Council, making the announcement that it had been finally resolved to carry into effect the proposal of resorting to St Mary's Loch for an addition to our water supply. Though opposed to this determination, on every ground of knowledge and reason that lay within the reach of my capacity, I did not at first incline to make it the subject of any comments; chiefly because, as it seemed to me to have been irrationally arrived at by men who, whatever their merits otherwise, were not competent from their training to decide on such questions, I did not see by what means it was to be rationally encountered, so that to engage in the contest appeared to me likely to be about as hopeful a proceeding as it would be to assail Arthur's Seat with buffets. On second thoughts, however, as my conclusion on the subject was a deliberate one, I considered that I ought not to be ruled by this impression; and should be willing to believe that, if I could state truths, and support them with authority above my own, there might still be those ready to yield to them an attention which might be hoped to be unfettered by prejudice or predetermination. But still, though the accumulation of the kind of facts relating to such a question, with the habit of estimating them, was but a part of past customary labours, and though I might only be able to attract towards them in your columns the notice of a day, it was necessarily more than the work of a day to digest them as I best could into that order and clearness that was required for my purpose. Hence the delay that has taken place in sending you my remarks.

Making no pretensions to engineering knowledge, it is neither the relative practicability, nor the cost of the opposing schemes which have been suggested, that I pretend to discuss. My sole consideration will

of cholera, than any that have been since published.

refer to the wholesomeness of the water which it is designed to introduce. Of course, thus far it is known to every one, that rarely indeed is the first estimate of an engineer not greatly exceeded by the results; and it would show little prescience in our citizens were they not prepared to expect the future levying of a water-rate, should the St Mary's Loch scheme be carried out, approaching to twice that at which they have hitherto been assessed. But the question ought not to be so regarded. If the St Mary's Loch Water be the worst in quality, even though its introduction were the least costly, it should not be selected, while any healthier resource, however temporary, remained possible elsewhere; just as the Heriot water scheme, or that of any other of our hill streams, even though it were the dearest, should be preferred were its water the more salubrious. The public health must stand above all meaner considerations, for it is the sole ultimate test of the public welfare. Now, as to the wholesomeness of the St Mary's Loch water, the report of the Trustees gives us but scanty testimony, to set in opposition to the repugnance with which a lake water has been from time immemorial regarded, as mawkish, unaërated, of unstable temperature, and prone to be loaded with rotten vegetable and animal organisms. Mr Bateman's testimony here I set aside, on the same score as that I make no assumptions to a knowledge of engineering. And after Mr Bateman, there remain only Dr Frankland, the Professor of Chemistry at the School of Mines, and Dr Macadam; but both of these are merely chemists, and have no title to speak as physicians or physiologists. Nor do we know to what extent they have so spoken, their Reports having not hitherto been published in full; the Trustees being apparently more ready to announce their decisions than careful to display to the utmost the grounds on which they were founded. Of Dr Frankland's judgment, however, this at least we have distinctly learned: that he considers the Heriot water as for "drinking purposes decidedly superior to those of St Mary's Loch and the Talla;" while St Mary's he pronounces good for "domestic" uses. From the mode of introduction of these terms, we may surmise that by "domestic" uses, the Professor means washing and scouring, and the boiling of vegetables; and that it is in a kind of contrast to this that he applies the marked epithet of "decidedly superior" to the potable, and thereby far the more important and vital, qualities of the water of the Heriot. In the face of this, it seems to me an assumption of the most unexpected character, to cite Dr Frankland as assigning a preference to the St Mary's water. If it be, as may be more reasonably judged, the Heriot water rather which he designs to recommend, let us see how in this he is likely to be supported, or, in an opposite view, antagonised by another London Professor. "My colleagues," says recently Mr J. A. Wanklyn, Professor of Chemistry at the London Institution, "Messrs Chapman and Smith, and myself, have already shown that Loch Katrine and Bala Lake furnish waters which (although very soft), are, in point of putrescible matters contained in them, considerably worse than the average London supply in the state in which it actually reaches the consumers:" while he states, at another part of the same

publication, that "we hope to see the time when our towns, ceasing to be supplied with the waters of rivers or lakes, will derive their drinking water from deep springs." Thus much, meanwhile, to neutralise the scientific testimony adduced by the Trustees for the salubrity of the St Mary's Loch water. Let us now see, on the other hand, to what further objections that water may be held liable.

It is trite to tell, that the bones form the frame-work of the animal structure, without the existence of which, in due hardness and solidity, it could have neither strength, proportion, nor uses. It is almost alike familiar, that more than a half of the substance of the bones, conferring upon them this necessary stability, consists of earthy salts, of which phosphate of lime constitutes the larger portion; the bulk of the remainder, and still a very considerable portion, being carbonate of lime. But it is not a fact so generally or so easily cognoscible, that, according to many proofs and observations advanced, especially by Valentin, a highly distinguished physiologist, the phosphate of lime itself is frequently first formed within the body, being derived from the lime which has been introduced as carbonate of lime, and which joins itself to phosphoric acid set free from other combinations. In newly formed bones, besides, and in the fresh junctions of fractured bones, he found the relative proportion of the carbonate of lime to the phosphate greatly increased, affording another proof of the essential importance of the existence of the carbonate within the body; while Fletscher and Beneke, in confirmation, have remarked the more rapid union of fractures under its use. Analogous observations have been made by Seegen. By an opposite set of proofs, Boussingault, Chossat, and Seegen have shown the injury to the solidity of the bones, in instances where carbonate of lime has been incidentally or designedly withheld. In the disease of rickets in children, also, a well-known scourge in dense populations, there is an interruption in the process of ossification, the proportion of earthy salts in the bones being sometimes reduced by two-thirds; and it must here be manifest how much further damage may be effected in increasing the number, or in aggravating the affliction, of the puny and distorted victims of the disease, by an arbitrary abstraction of the supply of the carbonate of lime provided by nature. Scrophulous diseases, moreover, so wide in their range, and so destructive in their direct as well as indirect influence, owe much to the moderate and habitual use of lime for their prevention, as at one time, before the discovery of iodine, they depended extensively upon it for their cure. Taken from our drinking-waters, besides, lime takes along with it their freshness and sapidity, and leaves them destitute of a stimulus to the stomach and its functions more truly efficient, and in the end more wholesome, than the most relished of condiments. And it is precisely through our best, and in all ages most esteemed, class of drinking-waters that this salutary agent is, though existing in other articles of aliment, presented to us the most regularly, the most habitually, under the most varied forms of diet, and in that state of solution the most adapted for its reception and assimilation within the system. But even if water could be proved to afford only the half of

of cholera, than any that have been since published.

our supply of this wholesome agency, necessary and advantageous at all times, and in the periods of growth a special mainstay; and if reckless or sanguine minds were bold enough to assume, what, to say the least, has never yet, either in proper extent, or adequate duration of time, been anywhere even duly tested, and were to maintain that the part to be procured from other sources was sufficient for all our wants; would it be wise in us, on such grounds, so laxly established, to reject the half of the wealth offered us by nature, that we might make the experiment of subsisting on the remainder?

The epithet of purity, with regard to water, is one that has been grossly misapplied. Apart from chemistry, and with reference to its salubrious uses, a water is no more entitled to be termed pure because it is destitute of a moderate impregnation of earthy salts, than a beer is pure in the proportion that it is little imbued with the extract of malt, or a soup with animal juices. Its purity consists in its commensurate endowment with those properties that fit it for promoting the growth, and sustaining the functions, of the living system. Were our aims for a community to be lowered from the consideration of how we might raise to the highest perfection its health and its vigour, and so conduce to its happiness and well-being, to that of how we best might aid it in economising its soap, then the application of the term purity to a water unimpregnated with earthy salts would conceal under it less of a mischievous fallacy. But, even here, what is wholly true, in a chemical sense, of the effect of the hardness of water in decomposing soap, is only partially true in a practical application. Not the most ignorant of housewives removes the hardness of the water she uses in washing, by wasting upon it her soap; but she produces first the degree of softness she requires by the addition of the common carbonate of soda, known thence in the shops as washing soda, which she purchases at a penny a pound, and of which, for our present city water, but a small quantity suffices, at a cost which thus need give no alarm to the most far-seeing parsimony. In this way, satisfying all other purposes, she preserves for the healthful uses of her family, to refresh the strong, or to cool the parched lips of fever, that sapid, sparkling, and aerated water which she may have been hitherto lucky enough to procure from springs, or from mountain streams that springs have newly fed. Thus we gather little in favour of the St Mary's Loch, on the score of that purity for which it has mainly been recommended to us, not by the chemists, but by the Town Council, as a quality for which no amount of a first outlay could be held too extravagant. This for them is unfortunate; yet, as a result, it is in no wise surprising. Where neither the observing nor the logical faculty has ever necessarily been specially or habitually exercised, men's minds see only that which lies most prominently before them, and, deciding upon it, ignore the rest, because it does not lie within the sphere of their inquiry, and because of the difficulty they have in penetrating to the principal quality when it is not the most apparent. Thus on purity, in this matter, as bearing on soap, they have a glimmer of light: but on purity, as affecting the laws of life and organisation, which they have made no object of study,

they are in darkness. Nor in this ignorance is there anything discreditable. It is merely, where we can suppose an equal amount of intelligence otherwise, the effect of the mental training having been in one direction, and not in another. But it becomes discreditable, when pretence is made to a knowledge and capacity of judging that is not possessed; just as it is disastrous where duties and responsibilities are assumed, or chance to be imposed, on those who cannot evince that capacity. It is proceeding far to say, with a recent quarterly reviewer, that, "notwithstanding the eulogies so often pronounced on 'the glorious principles of local self-government,' those principles, when reduced to practice, will usually be found exhibited in jobbing, waste, maladministration, and local disorder." But we may speak of the Water Trustees as we have just spoken, while desiring, as is but fitting, to leave wholly unimpeached their motives, their respectability, or their general intelligence; for the decision here depends on none of these things, but on a tutored sagacity, habitually directed towards the special kind of questions involved, or to the weighing of facts and testimonies in questions of kindred quality, constituting a form of capability which is little likely to be nurtured among what I fear must be termed the shrill platitudes of a Town Council's debates.

What I have hitherto said of the true criterion of the wholesomeness of a water, I have shown to depend, not only on facts and observations of scientific exactness, but on the demonstration of a general sense of fitness and probability, which has attracted to it the spontaneous assent of the world in all ages, with a degree of unanimity that has remained unbroken till very recent times, and even yet in singularly few examples. Of the higher kind of assent, that of physicians and chemists of eminence, including those who have made the subject of the preservation of health a peculiar study, I could offer almost countless further instances; and proceed to add a few, selecting from among only the more recent, and naming none unless of high merit and distinction, as well as of special authority on the question. Johnston, speaking in his "Agricultural Chemistry" of the presence of earthy salts in water, says that "it is not without a purpose that all water we meet with is thus impure." "The fluids of the animal body contain nearly the same saline substances as are present in the water we drink, and from this source it is no doubt intended that a certain portion of those saline substances should be obtained upon which the preservation and health of our bodies depend." "Thus, there is an obvious design and adaptation in the impurity of our spring and river waters, for *by that impurity they are better fitted to minister to the wants of living beings.*" (The *italics* are in the original.) "We see," remarks Boussingault, in his "Memoirs on Agricultural Chemistry and Physiology," "by what precedes that the saline substances dissolved in water have their place in alimentation, which, without their assistance, would have been insufficient." Dupasquier points out the usefulness of bicarbonate of lime in potable water; urging that "the quality of potable waters stands in no relation with their degree of purity; that the purest waters, with regard to the quantity of matters dissolved,

of cholera, than any that have been since published.

are not on that account the best; and that it is by a truly providential provision of nature that waters contain more or less of foreign matters in solution." Riche, Professor at the School of Pharmacy at Paris, says, in a recent lecture, that spring water is the best of all waters; and for this he adds to his own authority that of the eminent chemist, Dumas. He notifies further that carbonate of lime, if in small proportion, gives an agreeable taste, and is salubrious; and that it is by carbonic acid that the lime necessary for our organism is dissolved, by which water is converted into a veritable aliment. Tardieu, the great authority on public health in France, states that most writers hold the presence of one part of carbonate of lime in two thousand of water as advantageous. Oesterlen, who is to Germany, in many respects, what Tardieu is to France, says that a chemically pure water is by no means the pleasantest water to drink; that the sparkling clearness, and agreeable and refreshing qualities to be desired, depend much on moderate impregnation with salts of lime; and that these salts of lime, so presented to us, are required for our bones and other tissues. In a report made to the Academy of Medicine in Paris, by MM. Poggiale, Boudet, and Tardieu, and adopted by that distinguished body, it is remarked that all communities seek spring water, even at great sacrifices of cost; and, among others, nine principal places of France are specified, with some foreign examples, as Rome, Brussels, and (let us be thankful in the meantime) Edinburgh. They point out as an error to be combated, that the chemically purest waters are the best; the saline matters being necessary to the support of life, becoming absorbed like alimentary substances, forming thus part of our organisation, and requiring renewal like its other portions. Lefort considers that a drinking water should contain enough of mineral salts to contribute to the process of ossification; and he has the further statement, that a water which holds dissolved the greatest possible proportion of carbonic acid, oxygen, and nitrogen, and which contains carbonate of lime in quantity below that which curdles soap (such as our present Edinburgh water,) leaves nothing to be desired, whether for drinking or for household economy. Professor Bouchardat recognises the usefulness of carbonate of lime in waters for a variety of services to the living system, and amongst these for aiding the nutrition of the young, by supplying an indispensable aliment to the bones; and it is on this ground that he considers rain-water too pure, the absence of the salts of lime being prejudicial to "nurses, children in early life, and young animals." Hugueny sets the chief value on the existence of lime in waters, from its being required in the period of man's growth; and Agassiz, to possess whom amongst them is a pride to the Americans, has remarked that the drinking of water in limestone regions enlarges the skeleton. And let it be remembered, as another important element for consideration in the choice of a drinking water, that it has been found that the original temperature of a source becomes little affected by transmission through even covered conduits. Many observations of this have been made abroad; and, among others, by MM. Commaille and Lambert at Rome, who

have ascertained that the waters from springs, which supply that city so profusely, are uniformly fresh and cool in summer as in winter; while the water brought to supply a fountain from the lakes Bracciano and Martignano, though it arrives in such mass as to drive mills, and therefore more than equals the anticipated capabilities of our St Mary's, is variable in temperature, warm in summer and cold in winter. Thus the *aqua virgo*, esteemed the best in Rome, arising from springs, and flowing under ground in an ancient conduit, was found to show a heat of 57° F.; while the more modern supply from the lakes, introduced in the seventeenth century by Pope Paul V., and flowing also through a subterranean conduit, both being of considerable length, stood as high as 73° F., the temperature of the air at the hour of the experiment being about a degree lower. In a word, it was unfit to refresh or quench thirst. Surely there is enough of testimony here to neutralise the opinions of our Water Trustees, even by their own admission, as well in point of number as in quality of judgment. If the authorities are chiefly foreign, it is because the subject has been infinitely more and better considered in foreign countries than in our own.

But it may be asked whether all the authorities are thus on one side, and whether there be no conflicting testimony to adduce, and which assuredly ought not to be concealed. It is merely the truth, however, to state that such testimony occurs only with extreme rarity, is presented by far less eminent authorities, and occurs with an incompleteness, and often with an inaccuracy, that tallies with the difficulties that were to be encountered where the probabilities it had to contend against were so strong and so manifold. Thus, I have seen it stated that the Dutch drink waters destitute of carbonate of lime, and yet are a healthy and robust people. But my knowledge of Dutch medical literature is neither so indirect nor so scanty as not to have made me aware how far such a statement is from being supported by the facts. A journal, under the leading editorship of the singularly able Professor Donders, of Utrecht, speaks of the "duinwater," with which Amsterdam is chiefly supplied, as containing more than double the quantity of carbonate of lime (no less than 11½ grains to the gallon) that is dissolved in the "schuitwater," a source of supply which has been partially introduced; and assigns this as one of the qualities which confer upon the former a decided superiority as a drinking water. A writer in another Dutch medical journal speaks of a well at Amsterdam, the water of which contains even a larger proportion of carbonate of lime, on the score of which he claims for it a decided preference over rain and river water; adding the remark that no instructed person can dispute the great value of a certain proportion of this substance in drinking waters, for the uses of the organisation. In a paper read before the Academy of Sciences at Amsterdam, the wells at Utrecht, The Hague, Middelburg, Leyden, Zwolle, and Bois-le-duc, are severally instanced as containing very considerable portions of carbonate of lime. And so this alleged fact falls to the ground. Another instance which has been cited is that of Aberdeen. But

of cholera, than any that have been since published.

Aberdeen drinks at least a river water, with its adequate aeration, and not the vapid overflowings of a lake ; and this river water, though it has not a sufficiency of calcareous carbonate, has the not wholly unimportant difference of a fourth more than that of the St Mary's Loch, with a corresponding degree of hardness, that rises to occasionally a somewhat higher proportion. And who shall say, after the facts and opinions which have been adduced in an earlier portion of this letter, that there is no relation between this inferior quality of the water at Aberdeen, and the appreciably higher mortality there than in Edinburgh, as shewn in the mean of the ten years terminating in 1866 by the returns of the Registrar-General; proving that there had been more than a neutralisation, from some cause, of all the advantages that ought otherwise to have been derived from the presence of a far less numerous and less dense population ? Some instances cited, again, are marked by no true analogy : the so-called pure water having been used only incidentally, and for a short time, or at a period of life where its deficiences were less essential ; or the concomitant has been a squat and stunted growth of the population, like that of the Laps and Esquimaux, such as is not desirable for our city ratepayers. As to Glasgow, with its lake water of almost absolute nullity of impregnation, it has still to await the lesson it has to learn, and the experience it has to record ; with as yet no encouragement to cheer it on in the vastest and boldest physiological experiment on the health of a population that any place or time has heretofore witnessed. Nor must it be forgotten that, while every legitimate experiment proceeds upon a hypothesis resting on previous experience, often of the slowest growth, this experiment starts up to us as a sudden excrescence, not founded on any induction of experience, but assuming rather what is in the face of all experience, handed down to us from time immemorial as the common faith of the world. Doubtless, while the tendency continues, often a very unfortunate one, for communities to huddle together in enormous masses, occasions may arise where, from local position or otherwise, a sufficiency of water, of a justly termed wholesome quality, cannot be attained ; and then necessity will intervene, with an approach to the same hard law that makes the traveller in the desert stoop among the camel's feet, to filter through his lips his mouthful of turbid pollution. But to this kind of choice we need never turn voluntarily, where Providence has kindly supplied us with the better alternative. No one has dared to assert that a moderate quantity of carbonate of lime in water is prejudicial to health. Surely, then, if there be a doubt on the one side, and no doubt on the other, prudent men, recognising the weight of their responsibilities, would take the water containing the lime, if within their power ; and resort to that of doubtful, or disputed, or unproved salubriousness, only when compelled by that necessity which offered them no other resources.

Has, then, the St Mary's Loch water nothing to recommend it ? It seems it has quantity ; though not even that is admitted by Mr Hawksley. But if the quality be bad, or doubtful, then the greater quantity can but increase the evil or the doubt ; and the mischief will

only be relieved in so far as the lake-water is mixed with the better water hitherto and now enjoyed by us. A leading man among our Town Councillors speaks of a proposed addition of nine millions of gallons daily as but an intolerable proceeding by driblets ; though the Heriot Water, pronounced by Mr Bateman, their own engineer, to be capable of producing this quantity, is also characterised by Dr Frankland, their own chemist, as a drinking water "decidedly superior" to that of St Mary's Loch. We must acknowledge here a mind of very gigantic ideas, but we may fairly hesitate before allowing them to be of equal exactness, and must rather surmise that their hugeness has made them unwieldy. He must have daily twelve millions of gallons from St Mary's Loch, a quantity that, with our existing supply of usually above eight millions, would, were the difficulty overcome of an equable distribution without which no profusion of supply can rightly avail, give nearly two hogsheads of water daily to every man, woman, and child of a population of 200,000 ; the amount required for an adult man for food and drink each day being estimated by physiologists at three pints and a-half, thus leaving him an overplus, beyond all further legitimate uses, in which he and his wife and children may splash like tritons. It is difficult to regard with seriousness the manner in which this preposterous demand is made for an excess of water. In the "Statement of the Corporations" of February 1869, when upwards of eight millions of gallons were pouring daily into the city, we are told that reports had been "received from the medical officers of the corporations, to the effect that the continuance of such a state of matters was attended with the most serious risks to the public health." If the "gravest apprehensions" were then entertained, what must have been the anxieties when, the water being still delivered at the same rate at the date of the initiation of the new management in May of this year, the authorities saw it first dwindle in June to 750 cubic feet per minute, then to 600 feet before the end of August, and finally to 375 feet, or less than three and a quarter millions of gallons a day, at the beginning of September ; with no improvement thence till the middle of October, and even then in a very limited degree. The gravest apprehensions reasonably entertained for the public health with a supply of over eight millions of gallons daily, must have inferred utter ruin with a provision of nearly five millions less. But, let us comfort ourselves by taking the actual result from the dry, yet here more satisfying, returns of the Registrar-General. In the week ending on October 22nd, the mortality in the city was not above, but 23 under, that of the corresponding week of last year ; and 37 under the general weekly average of that year. Including the four previous weeks, the average weekly mortality was 33 under that of the whole preceding year.* The truth, then, manifestly is that, with certainly some inconvenience, there has been nothing like actual distress, nor the continuance even of a faint echo of the clamour that

* March 10, 1871. With still a curtailment in the supply of water, this materially improved state of the health of the community has continued persistently till now.

of cholera, than any that have been since published.

feigned it, while there has been anything but an increased disease and mortality; and so the dark forebodings, with the exaggeration of our real wants they set forth, have been more than thrown to the winds.

It is thus by no means easy to determine the process, by which the Town Council have reached the conclusion which they desire to force upon a too supine community; and it becomes even painful, in as far as it might lapse into a consideration of possible motives, to pursue the inquiry. It may be speculated whether, after having made a continuity of remarkable efforts, and caused a waste in expenditure of £20,000 in obtaining from the late Water Company, in the end, terms that would have been conceded at the beginning without cost or labour, there may not remain naturally with them a strong temptation to do something different from what the Water Company had promised, had it remained in possession, with a cheapness of rate which we are little likely ever to know again; and yet a little praiseworthy magnanimity, such as might have been expected from them, ought easily to have overcome the tendency to such weakness. That they should have a prescient care for posterity, by providing at our present cost for a problematical increase of the city during half-a-century to come, may to some appear generous, but to others simply prodigal. Certain at least it is, that had the Water Company demanded, half-a-century back, the right to expend a capital then, in anticipation of the 200,000 of our present population, and to lay the great resulting charges on the ratepayers of that day, no shout could have been loud enough to express the derision with which the proposal would have been encountered by the then types of that class who are the noisiest in their professions now. But let them take heed, at all events, that, under the pretext of generosity, they do not bequeath a legacy of evil to their descendants, in the shape of a comparatively distasteful and unwholesome water, provided at the no slight cost of those now living, but in reality at a greater cost in vigour and refreshment to those who are to follow, while better, and still ample, resources are more easily within reach. Under the provision of a leading necessary of life made by our late Water Company, the city has risen to much of its present extent and magnificence. But its growth, instead of advancing, may be checked, and its prosperity lowered, if those who now flock from a distance to the numerous villas surrounding it, and who are the best additions to its population, inasmuch as they bring everything to it and take nothing from it, should be henceforward repelled by the prospect of a costly water-rate, for an unrefreshing and, in a strict sense, innutritious water.

And thus I have concluded what I have been quixotic enough to consider a duty. Possibly I have been concerning myself with those whom Arnauld has characterised as troubling themselves little with considering proofs, paying slight regard to the reasons of others, and desiring to sway all by mere authority, because they never distinguish between authority and reason, and who believe there is less discredit in persisting in error than in confessing it. To me personally the

matter is of no moment whatever. Retired from the practice of my profession, I have no private end to serve. Approaching three score years and ten, I shall suffer nothing by any change. Leaving no descendant in the city, I am solicitous for no relative. With a simple regard for the true welfare of our beautiful metropolis and its inhabitants, now and to come, "*liberavi animam meam*."—I am, &c.,

A. Physician.

Letter II.

(From the Scotsman of November 16, 1870.*)*

Edinburgh, November 12, 1870.

Sir,—Since writing my letter to you of the 7th November, on the faulty nature of the St Mary's Loch water, I have had an opportunity of seeing three several "Results of Analysis" by Dr Frankland, to whose views I was only able to refer in part previously, having been further aided by a still more limited access to those of Dr Macadam.

What I have now learned from this more complete information confirms more than all that I had hitherto pronounced of the injudiciousness of the selection of the St Mary's water, yet not in any greater degree than I had anticipated. The Heriot water is declared by Dr Frankland to be "the best river water" he had ever examined. For "drinking purposes" it is "decidely superior" to the waters of St Marys Loch and the Talla. It is "a soft water." It is "entirely free from living organisms, and from suspended matter of any description, and it exhibits no evidence of previous sewage or animal contamination;" while, best of all, it is "derived chiefly, if not entirely, from deep-seated springs." The "gathering ground appears to be of unexceptionable character." So far, and very far indeed, for positive statements. It is a small matter that he brings forward nothing to counterpoise these, unless the insinuation of a fear, which may be taken for what it is worth, that the gathering ground may not always remain as good as it now is; and which, moreover, is more than balanced by a similarly hinted fear for the St Mary's and Talla waters, that wet weather samples of them "might not prove quite so favourable."

On the other hand, when Dr Frankland proceeds to compare the St Mary's Loch water with that of the Talla, the latter is declared to be "undoubtedly the best;" thus reducing the unfortunate St Mary's to the lowest rank of the three, of which the Heriot stands first. Nevertheless, the positive good qualities of the St Mary's water are, that it is "very soft, and hence well adapted for washing and manufacturing operations, except brewing." Except brewing! Am I in error in believing that brewing is the chief "manufacturing operation" in Edinburgh, and certainly that which uses the largest quantity of water? But there remains yet another degradation for the St Mary's

of cholera, than any that have been since published.

Loch water. "It is," says Dr Frankland, "nearly, but not quite equal to Loch Katrine in quality, when the water fleas are strained off the St Mary's water." And thrice again we have these "water fleas" or "water insects" introduced, of which the Heriot water is pronounced to be "clear." Now, there has been apparently a delicate consideration here for the city, with reference to these fleas, for which the Town Council or its Water Trustees should have due credit. They are not once alluded to in their report, as published in your paper; and this must clearly be in order that the public might be benevolently saved, in the meantime, from dwelling in anticipation on that disgust which it would be soon enough for them to manifest, when they discovered this more lively than agreeable kind of vermin marauding and disporting in their drink. And this is the water which is to be brought to us, and for which we are to be made chargeable in the Act with an "unlimited water rate." It is, however, once more creditable to the Water Trustees that they are consistent. They do not strain at fleas, and, I fear, they do swallow camels.

As to the more strictly chemical part of Dr Frankland's analysis, I make no pretensions to impeach their ability or accuracy, in as far as they go; but I should have been glad, if it was intended that they should instruct the public duly, that they had been less of mere outlines, and had been more complete in their details, and with more ample collateral explanations. They are neither fully qualitative nor fully quantitative; they give no direct indications whatever of the saline bases; and they make no attempt at that synthesis, or reunion and rearrangement of the various elements, so uniformly expected in such documents, which is designed to show the proportions and forms of combination in which the constituents appear to exist in the several waters.—I am, &c.,

A PHYSICIAN.

LETTER III.

(From the Scotsman of November 30, 1870.*)*

Edinburgh, November 29, 1870.

SIR,—I was well aware that my previous letters would be replied to, but I was not the less assured that they could not be answered, for the obvious reason that all opposite views were of too recent origin to be as yet duly based on a matured experience, which, with men of a truly philosophic spirit of inquiry, is ever of slow and hesitating progress. Hence, the authorities I adduced are met with assertions enough, but repelled by no corresponding proofs.

As an additional authority in support of those whom I have cited, I desire none better than Dr Frankland, when he makes once more the positive statement that the water of the Talla and the Heriot was, at

the time the samples were taken (he could speak of no other), "of markedly superior quality." That, in spite of this, he had been enabled to arrive at the conclusion that the St Mary's Loch Scheme was that which was to be preferred, nowhere appears in his previous reports; for the general tenor of which, with full appreciation of the surmise that I had not "followed the modern developments of water analysis," I must still desire far more copious particulars and fuller explanations.

With the evidence before the Royal Commission on Water Supply I was already acquainted, and would have been surprised at the vagueness of much of it, had I not been aware how few and imperfectly determined were the facts on which a large portion of it was grounded, owing to the short time for investigation that had elapsed since the promulgation of the newer of the doctrines involved. As to the discussion relating to the comparative wholesomeness of hard and soft waters, it has no very striking connection with our present topic, because Dr Frankland has himself pronounced the Heriot to be a "soft" water; and no one has ventured to recommend, or could even procure in this district, the waters exceeding 16 or 20 degs. of hardness which Dr Parkes thinks would be prejudicial. What is furnished for the navy in the shape of distilled water has nothing to do with the question, as neither the age of those using it, nor the duration of time in which it is used, affords the proper test; and certainly the distilled water will be discarded whenever the crews are within reach of a better supply. In conclusion, would it not have been as well if Dr Frankland had stated that the decision of the Royal Commission was against the introduction of lake water into London?

Where there is a more direct attempt to instruct me than I have hitherto encountered, I ought to be duly grateful, even though it should prove a failure. Thus, when Dr Frankland gives me the credit of knowing that Lehmann has shown that in rickety children there is an abnormal expulsion of lime from the system, I remain at a loss to understand whether he considers that a reason why less lime should be taken in. If so, I demur to either the consistency or the reasonableness of so absolute an assertion. On the other hand, where I am recommended by another monitor that I should have consulted the writings of Professor Parkes and Professor Mapother, there is again a claim upon my gratitude; which, however, would probably have been here also somewhat more lively, had I not been already acquainted with both works, and with the disappointment of not finding the subject discussed in either with anything like adequate fulness.

Lastly, Dr Frankland kindly calls my too wandering attention home, to the salubriousness of certain Scottish towns drinking comparatively soft waters. Glasgow and Aberdeen I have discussed already; Selkirk and Peebles, besides drinking a water twice as hard as that of St Mary's, are places too small to come duly within the category. But why does he refer me to Perth and Greenock, the latter with a less hard water than St Mary's? I have only means of present reference to

of cholera, than any that have been since published.

three years' returns of our Scottish Registrar-General, but from these I find that while Edinburgh had an average general mortality of 2·44 per cent., that of Perth was 2·54, and that of Greenock 3·18 per cent.; while the mortality from epidemic disease was in Edinburgh, 0·52 : in Perth, 0·65 ; and in Greenock, 0·96 per cent., the last town maintaining the same sad supremacy with reference to the mortality of children. I know that further inquiries would show generally a maintenance of a similar proportion ; and thus may fairly stop to wonder at the prudence of this challenge.

What advantage is to be derived from the mere annexation of my individual name to my remarks, I cannot imagine. Were my reputation a great one, I should wish no one to be blinded by it. If it be mean, surely no one ought to complain that my facts and my arguments are thus left merely to their naked force. But the demand for my name is, in truth, a ludicrous evasion from a severer task. Names and authorities are given, and given in profusion, such as are stated by me to be "above my own," and these, and not myself, are to be answered and confuted. That any one who reads them should not know where to seek for them, is no fault of mine. All of them are recent. Some are within this year, and eight-tenths of them within these ten years.

As I have, at the least, shown that there is anything but unanimity on this question, if I have not shown something of far more intrinsic consequence, I need only add that no rational individual can expect to see in the results of an insalubrious water anything like a striking and immediate effect. It will be by slow degrees, through generations leaving their inheritance to generations, that the deterioration will evince itself; and even then more by a general discomfort and want of vigorous growth and health, cumulating at last in an aggregation of broad results, than in startling instances, or any more tangible manifestations. Surely, then I am entitled to repeat the question contained in my first letter :—

Whether, even if water could be proved to afford only the half of our supply of a wholesome agency, necessary and advantageous at all times, and in the periods of growth a special mainstay ; and if reckless or sanguine minds were bold enough to assume, what, to say the least, has never yet, either in proper extent, or adequate duration of time, been anywhere even duly tested, and were to maintain that the part to be procured from other sources was sufficient for all our wants ; would it be wise in us, on such grounds, so laxly established, to reject the half of the wealth offered us by nature, that we might make the experiment of subsisting on the remainder?

And to this I would add another extract :—

Doubtless, while the tendency continues, often a very unfortunate one, for communities to huddle together in enormous masses, occasions may arise where, from local position or otherwise, a sufficiency of water, of a justly termed wholesome quality, cannot be attained ; and then necessity will intervene, with an approach to the same hard law that makes the traveller in the desert stoop among the camel's feet, to filter through his lips his mouthful of turbid pollution. But to this kind of choice we need never turn voluntarily, where Providence has kindly supplied us with the better alternative. No one has dared to assert that a moderate quantity of carbonate of lime in water is prejudicial to health. Surely, then, if there be a doubt on the one side, and no doubt on the other, prudent men, recognising the weight of their responsibilities, would take the water containing the lime, if within their

power ; and resort to that of doubtful, or disputed, or unproved salubriousness, only when compelled by that necessity which offered them no other resources.

To persist in defiance of this, I hold would be not only a fault, but a cruelty.—I am, &c.,

A Physician.

Letter IV.

(From the Scotsman of December 27, 1870.)

Edinburgh, December 23, 1870.

Sir,—In the report of the Works Committee of the Edinburgh and District Water Trust, published in your paper of the 29th November last, there was the following astounding statement :—"If anything further were necessary to show how completely the opinions of 'Physician' and those who hold his views are opposed to the science of the present day, the report of the Royal Commissioners on the Water Supply of the Metropolis and other Large Towns, presided over by the Duke of Richmond, and presented to both Houses of Parliament in 1869, should be conclusive." In a letter, which appeared in your publication of the following day, I put at once the pertinent question, whether it would not have been as well if Dr Frankland, whose quotations had possibly led the Trustees to fall with blind precipitation into this startling announcement, "had stated that the decision of the Royal Commission was against the introduction of lake water into London."

For an answer to this question, with that display of confidence towards the public by which it might have been hoped to be accompanied, the city has now waited in vain for almost a month. It thus seems now to fall in some measure upon me, whose opinions were specially said to be overthrown by the report of the Royal Commission in a manner so conclusive, to bring that report, with its deliverances, in a more full and candid way before the consideration of the public, in order that they may judge towards which direction its authority leans. In my last letter, I pointed out the absurdity of quoting authority after authority, vague and uncertain, and largely qualified, as they necessarily were, to prove the superiority of soft to hard water, when a hard water had actually never been in question ; for the Heriot Water was declared to be a soft water by Dr Frankland himself, and to the Talla he gives a permanent hardness lower even than that of St Mary's, and lower, therefore, than many sound thinkers consider advisable, though it would still possess to a large extent those other advantages that render the water of a river superior to that of a lake. It does not seem necessary, therefore, to dwell upon the evidence of one witness, who says that there is no difference in his opinion between hard and soft water as to effect upon health,

of cholera, than any that have been since published.

though that witness be Dr Frankland (Report 6259); from some of whose more peculiar chemical views, by the way, nearly every other chemist examined (5422, 6459, 7074, app., p. 78) differs widely and directly: but I would ask if there be any candour or fairness in citing Dr Parkes as to waters of from ten to twenty degrees of hardness, which were, and could be, in no wise under discussion here, and witholding his statements when, in classifying the various qualities of water (3147,) he names first, as "*the purest and most wholesome water*," that "which is free from suspended matters, and contains very little dissolved organic matters, say under one grain per gallon, and that, probably vegetable, and of dissolved *mineral matters under seven grains per gallon*." Nor is this statement weakened by his description of his next best "pure and wholesome water, to which no objection could be taken, I believe, in a sanitary point of view" (3148), and which would contain under two grains of similar organic matter, and under twelve grains dissolved mineral matters per gallon, the latter principally carbonate of lime. But we know (in spite of the incomplete form of Dr Frankland's analyses, which was formerly adverted to, and owing to which they resemble none of the others given in the report of the Royal Commission, and are as little, I am glad to say, like those in any other works by chemists of authority) that the St Mary's water has more, besides its vivacious fleas, than this proportion of organic matter per gallon; while the mineral salts in the Heriot water, instead of reaching seven grains to the gallon, are actually under four and a-half grains, and its organic matter is set down at little more than a fourth of that in the lake water. The Heriot water, then, is even a better water than that which Professor Parkes describes as his chosen type of the purest and most wholesome water; and we now ascertain, from his own language, what was the real scope and tendency of his evidence.

But there is another witness, Dr Letheby, whose testimony might have been expected to have attracted, in a special manner, the attention of the Water Trustees; not because this gentleman is Professor of Chemistry in the Medical School of the London Hospital, for they might not have held that enough to save him from being fantastic and a dreamer, but because he is the Medical Officer of Health to the Corporation of the City of London, and thus peculiarly entitled to all appreciative sympathies from the corporation of a sister capital. Yet they have passed over Dr Letheby's evidence with a neglectful silence, which, on the part of those who desire to stand with a more open confidence towards the public, it may be as well to remedy. A moderately hard water, Dr Letheby tells us (3931), is the best for drinking. His evidence for this, he continues (3932), is derived from his own inquiries and experience, and from the evidence of the Continental Commissions which have been appointed to inquire into the best supply for the large cities of the Continent. Taking the case of Paris, he states that the French Commission concluded, after very careful investigation, that the facts went to show that the men were finer grown, and that fewer conscripts were refused on the score of insufficient

stature, and the like, in hard water than in soft water districts; and after much consideration of the subject, they recommended the municipality of Paris to take a water which is almost identically the same as the water of London, and has therefore fully thrice the hardness of that of the Heriot. "Very recently," Dr Letheby proceeds, "I have received from the Government of Vienna the report of a Commission appointed to investigate the distribution of water in Vienna, and there again they have abandoned soft water and have taken to a moderately hard water for two reasons: first, that the evidence goes to show that the men are better formed, that the bones are stronger and better formed, and that there is better health in moderately hard water districts than there is in soft water districts; and, secondly, that the public prejudices against water tainted with peaty matter are so great that a large city should never draw its supply from a soft water district." It cannot be said, he continues (3933), that lime is not necessary in water to supply bone; and, in addition to that, the water containing carbonate of lime "is almost always colourless, bright, sparkling, and agreeable to the palate." No wonder, then, that after all this various testimony, the able and well-informed witness adds (3934, 5), that if a soft water were at the door, and it were necessary to go to a distance for a carbonate of lime water of from 10 to 15 or 16 degrees of hardness, (from 14 to nearly 23, by the scale of Dr Frankland), he would recommend the hard water to be taken, even though it would cost more. Surely, the Water Trustees will not allege that a Medical officer of Health of the Corporation of the Queen of Cities is, too, behind the science of the day; or, if they do, will they tell us where science is to take refuge, if it have no tenable place, either within a Corporation or without one? For myself, I have been admonished to keep nearer home in my lucubrations, though not upon any very accurate notions as to what have been the limits of my inquiries. But is it not to fling myself into the very bosom of the Water Trust, when I have this latest testimony to add to so many others, and speak it thus in the words of the chief health official of the chief of municipalities?

And, now, let us turn to the conclusions of this report, whose decisions the Water Trustees, doubtless as proudly conscious of the value of their warranty as of the terrors of their denunciations, have pronounced to be *conclusive*. "The evidence before us," says the report of the Royal Commission (p. ci., 213,) "leads us to the conclusion that the Thames water has many good qualities, which render it peculiarly suitable for the supply of the metropolis, and which give it in some respects a superiority over the soft waters usually obtained from high gathering grounds. When properly filtered, it is clear, bright, colourless, agreeable, and palatable, and the amount and nature of its saline constituents are considered by many to contribute to its general acceptability for drinking. It is well aerated, has good keeping qualities, and is unusually safe as regards action on lead and iron." Their idea of the nature of these saline constituents is shown (p. lxi. 159,) by their stating that the chalk in

of cholera, than any that have been since published.

Thames water usually gives from 12 to 15 degrees of hardness (17 to 21 of Frankland,) which may be diminished, however, by thorough boiling to from 3½ to 7 degrees. In summing up their conclusions and recommendations (p. cxxvi., 258,) they repeat that there are points dependent on the softness and colour of the hill water, which might render it less suitable for the supply of the metropolis than the harder water at present used; that the same general remarks apply to lake water; that for drinking purposes (cxxvii., 260,) the weight of evidence is in favour of hard water; that for cooking, no important objection to the Thames water has been clearly proved; and that for washing, and for manufacturing purposes generally, they cannot see any advantage that is of sufficient importance to render it necessary to go to a great distance for soft water. As to quantity of supply, they do not consider it necessary to provide for more (cvii., 229,) than at the rate of 40 gallons per head of the population, the present provision being 33½ gallons. They add to this the important intimation, that they are of opinion (cxxviii., 264,) "that no town or district should be allowed to appropriate a source of supply which naturally and geographically belongs to a town or district nearer to such source, unless under special circumstances which justify the appropriation;" an axiom to which the community of Selkirk will be careful to advert, when, in terms of a recent unanimous resolution, they come to resist the spoliation attempted to be inflicted upon them, the result of which would be to check and injure the present state, and, still more, the future growth of their manufactures, in which they have a right to emulate, as it is for the common weal that they should emulate, the similarly situated towns of Galashiels and Hawick. Nor will their plea against this merciless clutch be weakened, when they can point to that "embarrassment of wealth" which Dr Frankland tells the Water Trustees they possess far nearer their own bounds, in the sources of the Heriot, if not of the Talla; the first, according to his original reports, being "decidedly superior" as a drinking water to the second, and yet still a soft water, and the second being, not the less, "undoubtedly the best" when compared with the water of St Mary's.

Let us pause for an instant here to consider what all this may mean. Have the Water Trustees, then, really ever read and weighed this Report of the Royal Commission? and, if they have, can their scrutiny have actually left them so blind to its import as to imagine that it lends support to their views, and confutes the views of those whom they have designated as their opponents? Or, if they have not been thus blind and careless, have they ventured to withhold their convictions from the public, with the hope that the contents of the report, so unfavourable to their prospects before the tribunal of Parliament, would remain unregarded by the mass until it was too late to intervene? I do not like the dilemma, and must leave to others its decision. Even the title of their bill has already become a misnomer; for that is scarcely a district trust from which Musselburgh and Dalkeith, whose co-operation was expected, have already seceded, choosing to seek else-

where for less doubtful and costly draughts than the mawkish waters of St Mary's. Meanwhile, there is something sad in the predicament that, whether they succeed or fail in their enterprise, the result will be disaster to the city. If they fail, in addition to the mischief of delay, they will have lavished in vain a large amount of the common funds, to be flung down beside all that they wasted in their fruitless efforts of last year, an account of which has been frequently demanded, but, doubtless, for adequate reasons, never shown. If they succeed, it will be to introduce at once discontent and discomfort, with a looking back repiningly to the better things that have been wilfully cast away, and a looking forward doubtfully, amid new and heavy pecuniary burdens, to the risks of a gradually impaired condition of haleness and vigour in our community. The only course that will bring them honour, will be to retrace their steps while there is yet time: for all men then will be ready with sympathy and respect for an open and honest confession of error; but there will be none for an obduracy which, let its issues shape themselves as they will, can thus end only in evils that could so easily have been avoided.

And this reminds me that there is yet another lesson which the Trustees would do well to take from the Report of the Royal Commission. Adverting to the consumers being at the mercy of the parties undertaking the supply of water, it proceeds (lix., 149) to state that "the health, often even the life, of the inhabitants is in the hands of these parties, and it is therefore a matter of paramount public interest that the manner in which they exercise this immense power should be jealously watched, and efficiently controlled." The lesson ought not to have been needed; but the Trustees may learn from it that they are not to wrap themselves up in a pretence of infallibility, which no one is, or ought to be, simple enough to concede to them. For my own part, the very names of nearly all the Trustees are unknown to me, and I am not aware that I have the slightest personal acquaintance with one of them. To me, therefore, they exist as but mere abstractions, and it is with their measures alone that I concern myself. It is needless, then, for them to imagine that, if any differ from them as to the wisdom, and thence as to the propriety, of their proceedings in relation to a matter of public interest, they have thus acquired any title to regard them as what they are pleased to term their opponents; even though it may have been proceeded so far as to question whether their scientific training can have been such as to render them the fittest to gather and appreciate scientific facts, or to adjudicate between scientific opinions. There is nothing in this inconsistent with the search for truth, or the desire to see it established; and if they themselves are solicitous for the truth, they will encourage, rather than stifle, discussion. But, whether or not, they cannot, at least, fail to know, that there are men in the city who can, do, and will think, and who will hold it to be their duty to state their thoughts also, on a topic so weighty as this, where a larger expenditure, with an inevitably heavier pressure of permanent taxation, is involved, and the public welfare is more deeply implicated, than on any previous occasion in the city's history. Nor,

of cholera, than any that have been since published.

while such men state nothing dogmatically, but argue with the caution and reserve proper to those who labour to lay a just foundation for their opinions, will they have the slightest fear that they will be led to mistake a few depreciatory phrases on the other side as a refutation, or a few hardy assertions as decisive proof, and arguments.

I now conclude this letter, though my topics, which would require the space of a dissertation, are very far from being exhausted. In as far as the true purport of the report of the Royal Commission is concerned, I may have said enough. With your permission, however, I shall, if it appear necessary, revert to the discussion again shortly, when I shall make some additional statements, which I have already prepared, with regard to the insalubriousness of lake or similar waters, and on the subject of the physiological actions of the calcareous salts. It may be proper to animadvert, then, also on the extraordinary statement of Dr Frankland, that the medical science of the Continent, with its literature on the subject of water, and its components, so infinitely beyond ours in copiousness and variety, is yet, on these points, inferior to it in value. Alas, even for British chemistry! it has now no names to place beside the distinguished workers in the science abroad, whose genius and intellect shed a lustre on the age, and on the countries that have produced them.—I am, &c.

A PHYSICIAN.

LETTER V.

(From the Scotsman of January 10, 1871.*)*

Edinburgh, January 9, 1871.

SIR,—The easy duty of my last letter was to demonstrate the impropriety of adducing the Report of the recent Royal Commission on Water Supply as proving the superiority of a soft to a hard water for general uses, while the purport of that document, and its conclusions, were of a precisely opposite description; and I left it to the public to determine what possible motives or circumstances could have led to so extraordinary a travesty. I then expressed an intention of entering further, with your permission, in a future letter, into the consideration of the insalubriousness of a lake or similar water, should there still appear to be a necessity for continuing the discussion. That necessity could only be obviated by a timely withdrawal, on the part of the Water Trustees, of their obnoxious scheme: a course which many thought they might have preferred to abiding that rejection by Parliament which may be anticipated to await them, and which will then bring with it still more surely those reproaches that a sensitively honourable mind is always the first to address to itself, in the shape of an imaging forth of shaken confidence, lost time, and wasted finances.

As this withdrawal, in every sense so desirable, has not yet been announced, I ask leave to return to my topic; in my consideration of which, while confining myself for the present to the relations of a too soft water to salubriousness as shown by statistics, I would fain carry along with me, on the part of the public, that grave attention which the question deserves, and which the Water Trustees have afforded so scant opportunity of exercising.

In the appendix to the report of the Royal Commission, there is to be found (p. 77) a table, presented by Drs Letheby and Odling, and Professor Abel, from which much weighty instruction is to be derived. Its design is to show the relation between the quality of water, more especially as to its degrees of hardness per gallon, and the prevalent rates of mortality in twenty-seven cities and towns, twenty-four of which are in England, and three in Scotland. But as the towns are grouped into only two divisions, according as the waters are above or below 10 degrees of hardness, and as the mortality seems to be founded on that of only a single year, I have thought it better, for the sake of greater security, to re-cast, and add to, the materials, founding the rates of mortality, with a very few casual exceptions, on averages of ten years, while increasing by thirteen the number of the towns and cities, five of these being in Scotland. The forty towns thus collated, I now place under four several categories as to their degrees of hardness: the first group embracing those above 10 degrees, the second those of from 10 to 6 degrees, the third those of from 6 to 2 degrees, and the fourth those of 2 degrees and under. Within the first group are embraced eighteen towns, among which is London; their average population being above 230,000, and the mean hardness of the waters 15·8 degrees per gallon. In the second group seven towns are included, having an average population of above 137,000, and an average hardness of 7·8 degrees per gallon. In the third group there are also seven towns, the average population being above 120,000, and the hardness 3·3 degrees; while in the fourth group the towns are eight in number, the average population amounting to above 88,000, and the hardness to 1·3 degrees per gallon.* In all statistical inquiries, we know that we approach the nearer the truth the more carefully arranged are our categories, and the more widely they are based on extent of numbers and time; and we have here enough to reach a tolerably safe approximation. The facts elicited are striking and important.

In the first group, with its largest masses of population, and its highest degrees of hardness of the waters, we elicit a yearly mean mortality of 21·9 per thousand of the living. In the second group, in which Edinburgh and Leith take their place, in spite of what should have been the advantage of the smaller masses of the population, the diminished hardness of the waters connects itself with a mortality increased to 24·9 per thousand. Passing to the third group, in which Dundee and Paisley are situated, with what should have been the yet

* See Appendix, Table A.

of cholera, than any that have been since published.

greater advantage of the masses of the population having undergone a further reduction, we find, with the still decreasing hardness, a still increasing mortality, the amount now rising to 26·3 per thousand. And in the fourth and last group, in which Glasgow, Greenock, Aberdeen, and Perth find their position, and to which would have fallen to be referred the waters of St Mary's (1·8 degree of hardness per gallon), again the yet further diminished average of the masses of population forgoes its advantage, and the lowest proportion of hardness is marked by the highest proportion of mortality, which stands now at a yearly average of 28·5 per thousand of the living. This result, by its near correspondence, confirms that arrived at by Dr Letheby and his associates from their less ample details, which, with an average hardness of 14·9, give a mortality of 22·2, and with a hardness averaging 4·9, a mortality of 26·1 ; the mean of the three last groups, as detailed above, being, when taken together, 4·2 of hardness, and 26·6 of mortality. If this so uniform descending in the hardness of the water, and climbing of the rate of mortality, could be viewed as but a coincidence, it carries with it, at least, a terrible consistency that may well arrest attention. But to regard it as a mere coincidence, would be manifestly to reject unreasonably the best description of evidence which such a subject is capable of admitting. It is true that the condition of health of a community is contingent, not on any single natural influence, but on the mutual actions and reactions of a variety of agencies in relation to regimen, diet, atmosphere, &c., on the presence of which, in greater or less purity or cogency, the general issue depends. But it is the province of statistics, should the result, in any individual example, appear to be affected by the failure or predominance of any individual agency, to spread the inquiry over an enlarged field, so that the disturbing element in one direction becomes neutralised by the existence and adduction of a compensating element in another. And, above all, it is to be noted whether, however other elements may waver, there be one element that is uniformly present, and in a ratio that is is ever capable of being nearly determined. Such an element, so capable of being known and measured, we have here, along with the water itself, in the varying quality of its hardness, as denoting the degree of its impregnation with carbonate of lime ; beside which must be placed the other superior properties of a water with which that impregnation is customarily associated.

The preceding numerical results, nevertheless, are not avouched as positive statements. They are offered merely as approximations, but approximations founded on such strong probabilities that only the foolhardy would disregard them ; for they speak from and of a past experience, and to await the lesson of one's own experience, where we may already profit by that of others, we know to be the wide distinction between the methods of folly and wisdom. Admitting then, the value of the results, it is possible to exhibit their import in a yet more striking and tangible shape. Thus, let us suppose that we were to reduce Edinburgh from its place in our second table, showing an average hardness of 7·8, with an average mortality of 24·9, to a place

in the third table, with an average hardness of 3·3, and an average mortality of 26·3, the result would be that with our present computed population of 180,000, the number of our funerals would be increased by 252 yearly. If we were to carry it into the fourth table, having an average hardness of 1·3, and an average mortality of 28·5, the number of our funerals would be increased by 648 yearly. With regard to Leith, with a present computed population of 38,000, its transference from the second to the third class, similarly calculated, would cause an increase of 53 funerals yearly, and to the fourth class of 136 ; or, taking Edinburgh and Leith together, there would be an aggregate increase, in the one case, of 305, and in the other, of 784 funerals yearly. These conclusions may certainly surprise many, and none more than those who have decided on this subject without having first deigned to consider it ; but it is not the less impossible to escape them.

But there may be an unwillingness to calculate upon futurity, for that is in the Highest hands. We may revert our speculations, then, and judge no further here than of the lesson which seems derivable from the past. Let us assume, with that view, that those in authority half-a-century back, when the proceedings of the late Water Company began to shape themselves into an efficiency that seemed adapted to the time, had shown themselves as supremely provident as certain of our city Councillors now, who desire to forestall a supply for the half century that is to come. The joint population of Edinburgh and Leith being then somewhat above 130,000, it would have been required of these far-sighted men to look forward prophetically towards the future increase of numbers, by which it may now be estimated to have reached about 220,000 ; and they would at once have gone, with a sagacity as practical then as now, to the St Mary's Loch as that certain and inexhaustible store that was to meet unstintingly all possible demands. This would have been a charming picture of prescience : but it has unhappily its reverse ; and the very least of its evils would have been the load of premature taxation that would necessarily have been imposed. The conjunct population, during the ensuing fifty years, would have been placed as to the criterion of hardness (1·8 per gallon), in the lowest of our tables, instead of ranking as now in the second ; and its rate of mortality would have been to be computed as 28·5, or the mean of the former, instead of 24·9, or the mean of the latter, the difference thus denoting an enhanced mortality of 3·6 for every thousand living. Now, reckoning the mean of the joint population during the lapse of this half-century to have been 175,000, the excess of deaths would have been 630 yearly above what has actually occurred ; and this, multiplied by 50, the numbers of years, would have given an aggregate of 31,500 deaths and funerals as the total sacrifice of the community to a foresight which would thus have trafficked in destruction at, in every sense, a costly price ; and to the failure of which that of the prudent Else, in the German popular tale, whose admirers alleged of her that she could see the wind and hear the flies (not fleas) cough, affords no parallel in range or extravagance, and, least of all, in fatality of issue.

of cholera, than any that have been since published.

We may take yet another illustration. If it were a real, it was doubtless a grave cause of regret for Leith to know that, by the publicly cited testimony of one of its inhabitants, water was during the year before last so scarce in the town that, on the interesting occasion of a baptism, there was not enough in the house to suffice for the ceremony, though it may be presumed to have been that of sprinkling. Yet even the pain of an infliction so peculiarly portentous as this, which was, doubtless, in some shape overcome, may appear somewhat light to the inhabitants of Glasgow, when they reflect that, with their vaunted superabundance of a soft water, they have suffered from a mean mortality during ten recent years of 30·5 per 1000 of the living; while Leith, with this bemoaned scarcity of a harder, yet not a hard water, had a mean mortality during the same period of only 23·5 per 1000. In other words, if Glasgow, with its present computed population of 430,000, had enjoyed the blessing to its inhabitants of an existence as little harassed as that of Leith, no less than 3010 fewer funerals in the year would have traversed its streets. For Greenock, associating a soft water with a mean mortality of 32·4 per thousand, the comparison is still more cheerless. The analogous relations to Edinburgh, with its mean mortality of 25·4 per 1000, are equally obvious, and, for the towns with soft water, only a little less painful. We may as yet rejoice, then, that the conditions which weighed upon others have hitherto not been ours; and, least of all, have been ours in the year which has just closed, when a vastly enhanced, and before unexampled, curtailment of the supply of water has been attended by a more than usually prosperous state of the public health; proving that even then, with so great an intensity of drought, there had been, as was previously pointed out, no real exigency of want, in any adequate sense of the term.

So much for the mortality which might have ensued, and which a happier policy has heretofore avoided. And even though, beyond all fairly reasonable expectation, the future mortality were found not to be increased through the accomplishment of the scheme now devised by the Town Council, still we could not be secure on that score that no actual and grievous mischief had been inflicted, because the effect of the inauguration of other beneficial agencies or improvements, ever in agitation or progress, might be counteracted and annulled. Conversely, too, there may be influences depressing a population, which a wholesome water might resist or retrieve. But it is peculiarly the intervention of considerations of this nature that enforces the necessity of averages, rather than lowers their value. Nor is it alone by the greater proportion of deaths that the action of an insalubrious influence is denoted. There are a thousand petty ailings and discomforts besides, which, with as subtile a force as that produced by a prolongation of east winds, render existence uneasy without making it immediately unsafe. It is not mere life for a population that is to be desired, but a consciousness in it of tone and vigour, and of mental as well as bodily elasticity. There are those who, when they defend the

qualities of a water that has been challenged, surprise us by seeming to speak as if it was to be expected that, when an unwholesome water is introduced in one year into a city, in the very next ensuing years we should see already rickety children crouching at the thresholds, and the streets thronged with men with sallow faces, craving for mental and physical stimulants to relieve life of its irksomeness, and to quicken it into feverish excitement while they hasten on its close. There are others who fancy themselves endowed with a prescience so miraculous, that, having but once sipped the St Mary's water in winter, they can not only foretell what will be its qualities in the heats of summer, but what will be its effects, for weal or woe, on the human frame in all time coming. But as reason revolts at such inconsiderate flippancies, so nature works in no such sudden and extravagant moods or fashions. When she tends towards perfection, it is by slow degrees; and we mark the completed stages of the progress, or its final result, rather than the progress itself. And it is not otherwise when the course imposed upon her is one of deterioration. Each generation bears with it a downward tendency, which it bequeaths to a succeeding generation, to be again intensified and again transmitted; more frequently adding, yet sometimes subtracting, a modicum till the measure is complete. It is by statistics that we strive to mark the stages; but in all statistics a main element is time.

It was the object of my previous letters to show that very many eminent men, physicians, physiologists, medical writers on public health, and chemists, held the belief that a water which was unduly impregnated with calcareous salts was not only unpleasant, but unwholesome; and the grounds were detailed on which this belief was founded. The proofs are now furnished, derived from our own experience, and applied to our own instruction, which demonstrate that their belief was correct. On the other hand, it has been shown how laxly held, and how feebly sustained, have been the opposing opinions, and by how few they have been entertained. There must be many men who would willingly avoid the task of investigating these things for themselves, and there may be a few rash enough, when the results are shown, to despise them, and proceed in defiance of them; although the inferences rest upon neither assumption nor theory, but evolve themselves simply from the facts of which they become the expression. But to all earnest men the truths they disclose are of a kind that ought to be held as sacred to humanity; and he who can slight them, without disproving them, exhibits neither moral nor intellectual quality that the meanest spirit need envy.—I am, &c.

A Physician.

March 10. To the preceding details, I am now able to add the results obtained from the collation of 25 other towns, chiefly seated in the Mersey and Ribble basins, the necessary materials having been mainly gathered from the recent First Report on Rivers Pollution,

of cholera, than any that have been since published.

and the returns of the English Registrar-General. These towns I have divided into groups as nearly identical in constitution as possible with those in the preceding letter: the previously formed fourth group, or that with a hardness of water of two degrees per gallon and under, being, however, unavoidably wanting, this extreme of softness of water-supply happily existing still in too great rarity to have afforded any additional examples; while the lowest of the third group descends, not now to nearly 2 degrees, but to only 3·42 degrees of hardness per gallon.* The results, in as far as they thus extend, evolve themselves in singular harmony with those elicited before: unless it be considered as an exception, that it is now the towns of the smallest average population which have the greatest average hardness of water-supply, so that whatever gain may be held to accrue from the lower mortality usually concurrent with the less dense agglomerations, must be here taken into the account; though it may be as fair to consider a persistence of the rule, under such varying conditions, as an element of its stability. In the first group, then, having a hardness of above 10 degrees per gallon, we have seven towns, with an average population of 35,129, an average hardness of 16·1, and a mortality, taken at a mean of ten years, of 22 per thousand of the living. In the second group, having a hardness of from 10 to 6 degrees, the towns are ten in number, the mean of their population being 67,909, and the mean hardness fallen to 8·1, but the mortality increased to 25·1. In the third group, with a hardness ranging from 6 to near 3 and a-half degrees, the towns are seven, the average population is 81,880, and the average hardness reduced to 4·2, while the mortality is now advanced to 26·2; or the fatality is about a fifth more than that in the group with the hardest water, the fourth and deadliest group in the former category remaining here unrepresented. Thus, from the averages of the whole sixty-five towns we seem to derive a consistent lesson, which, even if its foundations be but the first forecast towards a larger and more perfect induction, it would be a reproach to slight; unless henceforward a new fanaticism of opinion, maintained by however few, is to be permitted to inflict overweeningly a new martyrdom on our communities.

And yet more, while these sheets are passing through the press, my attention has been directed to two additional tables, copied from a recent report by Dr Letheby. In these he states the mean mortality per 1000, in one to five years, of twenty towns having water above ten degrees of hardness, and of thirteen towns having water under ten degrees of hardness. In the former division are nine towns which do not appear in my Tables: Portsmouth, Wolverhampton, Nottingham, Hull, Southampton, Reading, Ely, Chelmsford, and Gloucester; and these I find have death-rates assigned to them giving an average of 21·9 per 1000 of the living. For the towns in the latter division, there is given an average death-rate of slightly above 28 per 1000; but of these two only, Exeter and Bradford, have not already been included in the Tables drawn up by myself. We thus possess now an

* See Appendix, Table B.

aggregation of proofs, proceeding on different occasions from different sources, relating to a total of seventy-six unselected towns. The averages deduced from all of these alike evince, in nearly identical numbers, the greater salubriousness associated with hard when compared with soft waters; and thus the evidence, while passing uniformly further and further beyond the sphere of coincidence into that of demonstration, gathers itself into a force of conviction, such as surely, where circumstances permit a choice of waters, the extremes only of stolidity or recklessness can continue to neglect or withstand.

PH.

Letter VI.

(From the Scotsman of January 24, 1871.)

Edinburgh, January 23, 1871.

SIR,—I perceive, from a document published in your paper of this morning, that the Town Council and the Water Trustees still retain some painful anxieties, which I can easily believe, and possibly some lingering hopes, for which I have more difficulty in giving them credit, towards being able to repel the host of arguments and authorities which I have laid before the public, in proof of the more than imprudence of passing by the sources of the Heriot, in order to seek a more costly and a less agreeable and wholesome water in the twice as far removed depths of the stagnant St Mary's. The author of this document is Dr Stevenson Macadam; a gentleman whom I feel desirous to regard with every possible respect that is consistent with the wide difference in our opinions.

We may pass over briefly what Dr Macadam says of the fleas, whom he seems to treat as gently as if he loved them. That they may be found to exist in limited numbers in all impounded waters, no one will pretend to deny; though all may safely demur to that as a reason for their being introduced in unlimited numbers, along with a water notoriously fitted to favour their propagation. That they are innocuous is not probable, and at least is not proved. An example of an opposite view occurs to me as adduced by Moleschott, a writer on dietetics of the highest eminence, who states that the frequent diarrhœas produced by the still-running river waters of the Netherlands, among which he specially instances the Maas, are commonly attributed to the organisms these contain. To cite the insects adhering to fruit as analogous, is inept; for these are organisms so wholly differing in their nature and conditions of life from the water-fleas, that a reference from the one to the other would appear more reasonable as a contrast than as a comparison. There would, in truth, be no great distrust or alarm, were the St Mary's Loch water merely "liable to contain a few

of cholera, than any that have been since published.

minute atoms of animal life," to borrow the palliating phrase of Dr Macadam; but change the imaginary few into the real myriads, and the repugnance excited becomes sufficiently natural. Imagine here this picture of the Manchester water supply, one of the vaunted schemes of Mr Bateman, as given by Mr Homersham, the civil engineer (Rep. of Royal Com., 6286.): "If," he says, "you take a globular glass vessel, such as the globes in which gold and silver fish are kept, and which assists in magnifying those insects, you can see them jumping about in all directions." And he adds: "In spring water from the chalk you never find organisms, or an insect of any kind." Nor would the expensive process of filtration, which, judging from experience elsewhere, would cost the city about £1300 yearly, besides interest on capital, suffice to exclude the noisome swarms. Where an insect so diminutive in size, and living on other animalcules still smaller, contains so many as forty or fifty eggs at a time, the progeny must be too minute to be easily arrested by even the best constructed filter.

As to the argument relating to the salts of lime, I should have been glad that it had been more fairly stated. It is quite true that the main earthy constituent of bones is phosphate of lime; but it is not the less true that the supply of this particular salt, with its sources, has never been once in question. Neither is it true that the carbonate of lime, the salt really and continuously under discussion, exists in the bones in what can be termed a small proportion. Berzelius, the very highest of authorities, states it at about one-fifth of the whole amount of bone earth; and I know of no authority who computes it at less than about a sixth. But, whatever its relative proportion, there can be no doubt that it must have its essential uses and adaptations in the wonderful living structure: and there seems reason to infer that one of these uses is to bind together the particles of the more friable phosphate, and to harden them into firmness and solidity, just as the mortar in a building confers stability; or as the carbonate of lime, infiltrating through the sand under the earth, concretes it into stone. Now, even while excepting milk, which is but a scant article of a city diet unless in infancy (and it is not till between the twenty-seventh and thirtieth year that our osseous system is held to reach full maturity), it is nearly exclusively the phosphate of lime that is supplied to us in our ordinary food, and for the carbonate of lime other sources must be looked to. It is no sufficient answer to this that there may be here a double play of chemical affinities within the living system, and that other salts of lime may be decomposed, and a carbonate of lime eliminated; for this is but to subject the vital processes to a needless task, while their energies may sometimes not suffice for more than a fulfilment of indispensable necessities, and the result, therefore, is merely an arbitrary outrage on physiological law. Neither Lehmann, (Phys. Chem. I., 421) nor with him any other physiological chemist of eminence (Drs Frankland and Macadam are not physiological chemists), questions that water holding carbonate of lime in solution

is an adequate means of supplying this salt for the wants of our structure. All that he alleges is, that it may be supplemented "in a great measure," from the salt being secondarily formed within the organism through the decomposition of other salts. But this is not to assert, and still less to demonstrate, a complete and entire substitution, with a correspondingly perfect result; for there are other proofs which show that such a substitution is not consistent otherwise with our highest structural developments. And even here, the first succeeding step would be the solution of the newly formed carbonate in the water that permeates the whole living system, for thus only can it be conveyed to its several destinations; in this way, attempting in the end what nature, through an appropriate drinking-water, had accomplished from the first. Nor could we otherwise be sure of the event. The hen, deprived of carbonate of lime, lays eggs without solidity of shell; although, were there uniformly this play of double affinities, the phosphate of lime abounding in the grain she eats ought to have supplied all her wants. But even this lime in the grain must be derived from that held first in solution in the water with which the soil is imbued. The benefactor to mankind, who, along with an improved quality, makes two ears of corn grow where one only grew before, produces his effect by adding to the soil a larger proportion of the essential constituents of the plant, among which lime holds ever an indispensable place; and these, becoming dissolved in the water, enter the plant as sap, and finally are matured into its fruit, or for its structural uses otherwise. In short, both for the animal and the vegetable organization, the same primary fountain is open; and to reject it, and turn wholly to secondary sources, would be as reasonable as to forego the light of the sun, or the refreshment of a pure air, because our factitious lives have shown to us that much of our business, and the main part of our pleasures, can be pursued with artificial lights, or in close chambers. Yet neither can mere secondary resources be resorted to continuously with impunity. The stronger organizations are never benefitted by them, while the weaker organizations uniformly suffer; the deterioration, often slow, yet ultimately certain, being only mitigated or delayed where the better primary source has its periods to intervene. Why, then, would any reasonable man wilfully throw aside the carbonate of lime where it is presented to him in its most primitive, pure, simple, and natural state, and in that condition of solution under which it can be most easily received within the system, and elaborated for its uses; and that the more especially, when its presence in the water we drink is the sure concomitant of its greater pleasantness and sapidity, and its higher powers of invigorating and refreshing?

Yet who that has not thus appreciated the baseless nature of the arguments customarily adduced by the advocates for a chemically pure water would be otherwise than surprised to find Dr Macadam himself, as a conclusion to his discussion, "fully admitting" the desirability of having five or six grains of saline matter in the gallon, as probably the best proportion for "supplying those saline compounds which are so

of cholera, than any that have been since published.

much required in the building up and sustenance of the animal frame?" But so it is that reason will assert itself; and the water is still to be asked to supply what the oatmeal and the potatoes had been previously alleged to have rendered a needless superfluity. Now, the quantity of carbonate of lime in the Heriot is only about $4\frac{1}{4}$ grains per gallon; while that in the St Mary's water is under 2 grains according to Dr Frankland (hardness, 2.63 in 100,000 parts), and is only $1\frac{1}{2}$ grain, according to a previous analysis by Dr Macadam; who appears thus to misquote the 4·74 of "total solid impurity," which unfortunately includes the peat, for the 2·63 of hardness, representing the carbonate, each in 100,000 parts, and not in a gallon, or only 70,000 parts. As to the addition which the St Mary's water is to obtain by destroying the mortar of the conduit, that reminds me that one of the theoretical objections to a chemically pure water is that it may, in the same fashion, take up and remove from the living system matters that ought to remain. With regard to the indubitable origin of all waters in rain, it may serve as some comfort to those who have not the privilege to sit at municipal feasts, to know that the champagne imbibed there has almost wholly the same nature and origin; if they do not care for considering beyond that it may have acquired some further, and more pleasing, properties while percolating through the tissues of the vine, just as the water we drink becomes salubriously impregnated while draining through the interstices of the rocks. On the other hand, water in a pond or lake loses much of its pleasant carbonic acid, and with it a portion of the carbonate of lime its feeding streams had acquired, and we have, primarily or secondarily, deposits of marl. As to the bursting of boilers from incrustations, that seems utterly unknown in Edinburgh; where, still more singular to state, the like catastrophe does not appear to occur, even from frost.

Dr Macadam is of course a scholar, and it may interest him to observe with me how old, as well as how universal, has been this repugnance to the use of a lake water. Frontinus, who was curator of the Roman aqueducts in the first century of our era, when speaking of the now no longer flowing Aqua Alsietina as drawn from the lake of Martignano, wonders how Augustus could determine to bring into Rome "a water of so little agreeableness and salubrity, and which had never come into popular use and favour," and accounts for it by his having required it for his shows of mimic sea-fights. And, to leap thence to the close of the seventeenth century, it is in like terms that Fabretti, also a great authority on aqueducts, complains of the deterioration of the original waters of the Trajan aqueduct, through their admixture with those of Lake Bracciano by Pope Paul V.; a combination which still exists as the least prized and used for domestic purposes of the famous aqueducts of Rome.

On the whole, then, it seems to me matter of great regret that, on grounds so indefinite as these, and so incapable, for that very reason, of being duly supported, Dr Macadam should find himself in such nearly solitary opposition to the decisions of the Government Com-

missions at Paris, Vienna, and London, and to that of the Academy of Medicine of Paris; as well as to the individual opinions of the very many, and very high, authorities on public health, medicine, physiology, and chemistry whom I have quoted in my former letters. Between him and them I leave it to the public to decide in which way the balance of authority leans, claiming for myself no place upon the scales. —I am, &c.,

A PHYSICIAN.

LETTER VII.

(From the Scotsman of February 16, 1871.*)*

Edinburgh, February 15, 1871.

SIR,—Before closing my humble efforts to stimulate inquiry and reflection with regard to the subject of an additional water-supply to the city, by an exposition of what seemed to me the actual state of science on the question, and finally leaving it to the public, if it so please them, to go the farthest, that they may buy the water that is the worst, at the rate that is the dearest, I have still one or two points on which I formerly pledged myself to offer some remarks, that no statement might be left neglected among those which have been advanced in opposition to the views I have advocated. This done, I shall ask for no further space in your columns, which have been heretofore so liberally opened to me: unless, indeed, I find anything introduced into them in the future, on the authority of the Water Trustees, that seems directly to call upon me to sustain a former argument or allegation; or even, should it so appear to me to be just, to make acknowledgement of any error, an example which I should be glad to see the opposite side have the magnanimity to follow.

Dr Frankland, in his letter of the 21st November last, published a few days afterwards by the Water Trustees, adventures the statement that, "in respect of that section of sanitary science which is devoted to town drainage and water supply, our Continental brethren are at least a quarter of a century behind us." This was said with the evident intention of depreciating a part of the eminent authorities whom I had quoted in my first letter: and yet what is it but merely a repetition of the old fallacy, that asserts what is the truth of one thing which has not been denied, and is not even in question, in order to have it accepted, by minds little practised in logic, as true of another thing which is denied, and is the real matter at issue? It is this setting of a partial and non-essential truth, in the place of the main and vital truth, that most easily misleads the undisciplined reasoner, and thus forms the pest of controversy. No one has disputed that, in the actual practical details of what may be termed municipal and domestic hydraulics, in as far as the mere distribution of water is concerned, the mechanism employed by us, as well in extent as in quality, is generally superior, not to what is known among men of science, but to what has been commonly adopted among the habits of life of the Continent. But, for anything beyond this, to assume a superiority for this country seems to me the most unwarranted of pretences. When we turn to the whole broad science of hydrology, having for its object the study of the qualities of water, whether fresh or sea, pure or mineralised, including the nature and variety of its constituents, with their physiological and sanatory, as well as sanitary, properties and actions, while our own home literature is either an utter void, or shows little beyond a few treatises of neither repute nor originality, we find that of the Continent rich with an array of special and general disquisitions; and these proceeding for the most part from men who, with the basis of an ordinary medical education, have received a direct appointment from Government to superintend and officiate at the various watering places, and who have thus been constrained to make the subject of water, with its actions on the human frame, and those of its various modes of impregnation, the peculiar study of their lives. Let any one refer to a work so well known among the better educated of medical men as Ploucquet's Digest of Medical Literature, and let him run his eye down the long columns containing the catalogue of dissertations on the topic of water, in all its forms and varieties, published up to a little beyond the close of the last century, and he will soon discern how very minute is the proportion among these of the works of British authors. Nor has this proportion changed since. We have no names to place in rivalry, just or unjust, with those of Osann, Helfft, Simon, Ewich, Vetter, James, Posner, Seegen, Durand-Fardel, Lersch, Graefe, Kalisch, Spengler, and many others, besides those I have formerly quoted, all very recent or still living authors.

Doubtless there have been fluctuations of opinion; for a credulity that believes anything, and a scepticism that tests everything, have been ever the bane and the antidote in scientific progress, the fate of which is still to advance among the ruins of temporary reputations: though the actual state of our knowledge, on this topic of water, is best evinced by the recent decisions of the Government Commissions of the three chief centres of European civilisation, who have obviously possessed, and have fully used, the best means of gathering proofs and eliciting or eliminating opinions. It is besides characteristic of the zeal with which the study of this subject is pursued abroad, that there are at least four periodicals specially devoted there to its investigation, while in this country it would be a futile attempt to produce one. It was the salubriousness of the quality of water, in a main, but not exclusive, relation to the presence of carbonate of lime, and not the dexterity and profusion of its manner of distribution, that was discussed in my letter; and there is no ingredient of water whether for health or for bane, including the carbonate of lime, and not excepting the matter of sewage, churchyard exudations, and the like,

that has not been considered by foreign writers, the proofs of which I have now before me. My first acquaintance, indeed, with a lecture on the water of London, delivered by Dr Frankland at the Royal Institution, was through a French translation of it published in the *Revue des Cours Scientifiques;* a circumstance which I trust will satisfy that gentleman that the slumbers of his neighbours are not so sound as he has imagined. He may also easily convince himself that, when the Commission at Vienna determined in favour of waters, the average hardness of which far exceeds that of the Heriot, the question of sewage contamination had not escaped its consideration. They might not be willing to go as far as Dr Frankland, when he stated (Rep. of Royal Com., 6246,) that water that had passed over or through any cultivated district "is not safe for human consumption afterwards;" and if they were not, our rural parishes in the broad Lothians, as in countless other districts, who drink only such waters, would smile also at a danger which left them with an average mortality of from under 9 to rarely exceeding 15 per 1000 of the living, while that of the soft water drinking communities in Scotland was in some instances more than double of the highest of these proportions. And who, with these exaggerated notions of the perils of sewage, would dare to touch a potato that had soaked in the midst of such foulness; or regard as otherwise than a delusive snare the fragrance of our strawberries, that had rested on a soil reeking with its exhalations? But extravagant things have been uttered of water before now, and in, perhaps, still more unexpected quarters. Yet a lesson may be derived from the Koran, where it is said that one of the punishments in the next world is to be the compulsion to drink stagnant waters; for the association of ideas on this score may yet prove an additional reason for the citizens of Edinburgh to congratulate themselves that they are not Mahometans.

Let us turn now to the same letter by Dr Frankland for a reference to the authority of Dr Angus Smith, and let us flash a little light on that also, that we may see what it is really worth. Dr Smith (Rep. of Roy. Com., 7261,) says, as Dr Frankland quotes, with the intention of proving the wholesomeness of soft water:—"I should think that the tallest people in Great Britain are to be met with in soft water districts; for instance, in Cumberland, and probably in Aberdeen. I may say that the tallest people I have seen in Great Britain are in Aberdeen, which is a very soft water district." Now, as this gives but a surmise, and pretends to proceed upon no direct investigation, I might be content to meet it with a retort, on my part, which is at least something better than a surmise. It has always been impressed upon me as a fact, which has been confirmed to me by a late friend and patient who was for long surgeon to the Life Guards, and still more recently by an intelligent non-commissioned officer retired from the same body, that its best and tallest recruits, or, in other words, the finest men in the realm, were obtained from the border districts of England and Scotland, descending southwards through a considerable part of Yorkshire. But, fortunately, there is no need of making this

of cholera, than any that have been since published.

altogether a matter of surmise, or of conjecture on imperfect evidence, in any form. Every district may not be imbued with equal military ardour, nor may it always be a safe criterion to judge of the mass of a population from a gleaning of its more remarkable examples. It is only possible to arrive at a proper estimate by taking promiscuously the population as it presents itself; and this has been recently done in a wide series of measurements, taken in all parts of the country, by Dr Beddoe, with the aid of many coadjutors, and recorded by him in the last published volume of the Memoirs of the Anthropological Society, making us indebted to him for by far the completest and most authentic document of the kind that has been hitherto published. From it we learn that, in Scotland, the Borderers in general (p. 542) equal or surpass the average of the country both in height and weight; while the Borderers of the English side have a still more unequivocal superiority over their own countrymen. These men live, not on the granite or gneiss, nor the slates, giving origin to soft water, but over the lime of the coal formation, with the shales and marls and magnesian limestones of the sandstone formations; and the waters they drink are hard, those of Berwickshire and Roxburghshire thickly incrusting the housewives' kettles. Over the abundant limestones of Yorkshire, the men are among the tallest in England; and Dr Beddoe specially instances here the inhabitants around Richmond and in Swaledale. The average for Berwickshire is actually an inch and a-half above that of Aberdeen; while, as to Cumberland, Dr Beddoe gives to it, excluding Carlisle, an average of an inch below Berwickshire, and half-an inch above Aberdeen. This result seems naturally explained, if we consider that the level parts of that county are of the new red sandstone formation, or the coal formation, and that a broad belt of limestone surrounds the mass of slaty hills in the interior, so that the greater portion of the population lives beyond the soft-water limits. "In the old red sandstone district," says Professor Ramsay, in his Lectures on Physical Geology, "where the marls are somewhat calcareous and interstratified with impure concretionary limestone, called cornstone, the waters are hard. Again, the waters which flow from the Pennine chain, that extends from the southern borders of Scotland into Northumberland, are all hard, because they drain areas composed chiefly of the carboniferous limestone; and all the rivers that run east from this range, over the new red sandstone and lias, and the oolitic and cretaceous rocks, are of necessity exceedingly hard." And such waters supply the homes of those shown by Dr Beddoe to be the generally most stately and vigorous part of our population. Thus Dr Smith's statements, relied on by Dr Frankland, appear only as the product of vague impressions, reached without special inquiry, and hazarded without comparison or consideration; and such have ever intruded themselves into science as its opprobrium, compelling it rather to engage itself with the wearying task of the extirpation of error than with the cheerful labour of the discovery and establishment of truth. But Dr Smith makes yet another statement, which Dr Frankland does not quote, in the form of an assertion that the smallest number of

deaths occurs in the western district of Scotland, where there is soft water; and which I now introduce here in order to controvert it also. Let us select two typical counties, Argyleshire and Berwickshire, each with thirty-one parishes; and we find from the returns of the Registrar-General that, on an average of the ten years ending in 1864, while Argyleshire had only eleven parishes with a mortality smaller than 15 in the thousand, Berwickshire had as many as nineteen, the two lowest rates in the former being 11·4 and 12·8, but in the latter only 5·6 and 7·4. Roxburghshire, too, out of the like number of parishes, shows the same proportion of nineteen which have a death-rate under 15 per thousand.

What is true of man, in this country, in relation to the connection between his vigour of growth and the existence of carbonate of lime in his drinking water, is true of him also in other countries, and is not less true with regard to the lower animals. To content ourselves with the most remarkable of all examples, we may refer to the Patagonian savages, still universally acknowledged, after the removal of somewhat of former exaggeration, to be the most gigantic race known, whose home is on the tertiary plains of their country; while their immediate neighbours to the west, living among the extreme hills of the range of the Andes, are of far less stature, but still greater savages. In my former letters, reference was made to the testimony of Agassiz on this point of stature, as well as to the conclusions regarding it arrived at by the Ministerial Commissions of France and Austria. M. Durand de Gros, quoted by Dr Beddoe, has lately, in an important paper on the influence of mediums in the Aveyron, claimed a higher stature for the natives, human as well as bovine, of the calcareous districts in that department. As to cattle in this country, it will be remembered that all our finest breeds have originated in the hard-water tracts of the shires of Lincoln, Leicester, York, Durham, and Northumberland; and that these breeds have been everywhere largely used to improve the races in other districts, and must still be frequently reverted to by the latter for the maintenance of what are only acquired, rather than inherent, excellences. The same may be said of our sheep, all our best flocks being indebted for many of their highest qualities to the breed of Leicesters, reared on the lias, with its marl and limestones, and the sandstones of their native county. And so again of our horses, whose best and stateliest forms are developed among the carboniferous limestones of Lower Clydesdale, of Northumberland, and of Yorkshire, or, if we look abroad, upon the tertiary strata of Holstein and Flanders; while among the gneiss of Shetland and the Grampians, and the slates of Wales and Cumberland, we have the diminutive Welsh and Highland sheep and the Herdwicks, the kylo, and the pony, as the characteristic indigenous races. Clearly, then, if the Water Trustees design to continue to ride their high horse over the city, they must seek to maintain the lordly stateliness of their stud, not from among the soft, but from among the hard-water districts.

But why not seek for an illustration from a lower order of vitality still, and which may be introduced here as the best fitted to typify the

of cholera, than any that have been since published.

dumbness to which the veiled oracles in the Water Trust desire to reduce the ratepayers? "The process of percolation through the rocks," says Professor Ramsay, "gives rise to springs charged with lime, in the form of what chemists call a soluble bi-carbonate, which is carried into the rivers, and thus finding its way to the sea affords the material to shell-fish and other marine animals, and thus through their nutriment resolves itself into their bones and tissues; and thus it happens, that by little and little, lime is abstracted from the sea-water to form parts of animals which dying, frequently produce, by their skeletons or shells, immense strata of nearly pure limestone." Bischof, in his able work on Chemical and Physical Geology, introduces the curious calculation that, assuming the mean quantity of carbonate of lime in the waters of the Rhine to be 9·46 in 100,000 parts, the total flow, as estimated at Emmerich, would suffice annually to form shell for 332,539 millions of oysters of usual size. If this vast result be conceded as true with regard to the denizens of the sea, who have, indeed, no other conceivable source for the carbonate of lime they require, why should an analogous result be questioned with regard to man, and the imagination stretched to devise other sources for him, more doubtful, or at least more indirect, and less easy? Or why may we not reject, in turn, any of the other sources from which the carbonate of lime may be alleged to be procurable, with equal right to that by which we throw aside arbitrarily its source in water? And yet there are those who affect to sit enthroned above nature, and dictate to us what, according to their sovereign will, we are to accept from our solid food, but to reject from our drinks, of a material that they know to be a necessity for our structure. Why should we not be invited to aid nature, rather than compelled to thwart her? It would have been an overweening confidence in the archers at Agincourt, had they entered the contest with only one string to the bow; and in the battle of life there are none of our resources that we ought either to waste or destroy.

It is impossible to dwell long on the subject of a lake water, without adverting to the fleas, those illustrious strangers on whom the Town Council desire to confer the freedom of the city; though it seems to have been privily arranged that their visit to us was to have been at first incognito, as a kindred secret with that of the expenditure on last year's water bill, and of much information also in relation to the present bill, obtained at the ratepayers' cost, but kept beyond their reach, regarding a water which, too, they are expected to pay for, and, worst of all, to drink. Though these fleas have, owing to their imputed gentleness and innocence, become the pets of the Water Trust and of their advisers, they are not held in equal esteem everywhere; their harmlessness, with, in my opinion, that also of dissolved vegetable matters, having been somewhat too hastily assumed.* The city of Boston, in

* It is to be feared that it is not merely peat that we have to look for among the remains in the St Mary's water. When we consider the way in which the hill pastures are drained, it is manifest that the sheep droppings laid on the turf, and remaining there unchanged, will be swept with the first heavy rains into the surface cuttings, and thence carried directly into the Loch, adding their own special piquancy, with thoughts of eggs of flukes and hydatids, to the other qualities of the water.

the United States, is one of the few communities that imprudence has hitherto led, or necessity driven, into the use of a lake water, their supply being from Lake Cochituate, situated at some distance. In a paper published in *Silliman's American Journal of the Sciences* (vol. xix.), Dr Hayes, the Assayer to the State of Massachusetts, describes the "numerous animalcules and infusoria, fresh water sponges, and abundance of ochry matter, resulting from the chemical action of the water on the iron pipes," to be met with in this water. It is only consistent with this, that he finds in it an affinity to "putrid waters;" and states that "there are periods in every season, during which it closely approaches to these in character." In one October, "the general supply of water had become very offensive." The water-fleas increased in quantity and in size (from 1-8th to 1-16th of an inch), and "the cotton filters were soon closed by their bodies;" and in these fleas was detected the chief cause of the offensiveness, consisting in an oily matter in their bodies. The peculiar flavour of the water continued as bad in January as it had been in autumn. Those who, after this, descry purity in vermin and peat, and deny it in carbonate of lime, must really be considered as placing themselves beyond the pale of reason on this question. It may be noted here, as a suggestion by the way, that if I have not dwelt upon the action of a soft water on iron pipes and on lead cisterns, with the poisonous impregnations which have been occasionally observed to follow upon the latter, it was not because I had allowed the facts to pass unobserved, or considered them undeserving of attention, but because it appeared to me that there were points which had been less discussed, and which were of yet greater importance, with which I could better occupy the space at my disposal. Nor have I alluded to the possible effect of the introduction of a soft water upon the temperance of the community; though probably the idea prevalent in my youth has not yet been outrooted, that a lake water in summer, to be drunk with satisfaction, or even with impunity, required to be qualified with an addition of spirits. Beliefs of this kind cling closely to the toper, for they harmonise with his inclination; but I am willing to leave their consideration to the zealous circumspection of the abstainers in the Town Council, as some compensation for questioning their capacity in other matters which were less within their sphere.

Upon the whole, I think few who have followed this inquiry with attention, and will weigh what has been demonstrated for a hard water and against a soft water, with an unchanging consistency throughout of direct as well as collateral evidence and argument in regard to both, will wonder that the Commissions at Paris and Vienna have finally decided in favour of a hard water. Of the new waters designed for Paris, the copious sources of St Philbert and of Armentières, to be brought from a distance of about eighty miles, have an average hardness of 12·46 per gallon, or almost thrice that of the Heriot; while of those chosen for Vienna, the still more copious source of the Kaiserbrunnen, of ancient fame, and forty miles from the city, and those of the Stixenstein and the Antonioquello, show an average hardness per

of cholera, than any that have been since published.

gallon of 8·82, or a little more than double the hardness of the Heriot. With our Water Trust here, and still more, from recent tokens, with our Town Council, I cannot even now resign the hope that an appeal to reason may yet prove also congenial. It had, however, all the aspect of the following out of a foregone conclusion, when these bodies first, with their known proclivities, consulted Dr Frankland on the quality of the various accessible waters, and flung themselves prostrate in unquestioning confidence before him; for that gentleman was previously conspicuous as the advocate of an extreme of soft waters. Yet it is from him that, in his letter so often under discussion, we have the declaration that the Heriot Water "is excellent for every domestic purpose, including drinking;" while in his previous report and analysis he had pronounced it to be for drinking purposes the best, and to be without fleas and without peat. The Water Trustees may avouch the St Mary's Loch water to be a fit water, if they can. But even if they succeeded in this, how far would they be advanced towards their ultimate object, when it becomes necessary for them to show that, in order to reach the loch, they must go twice as far as for another water which exists in abundance, and which they dare not, and do not, declare to be unfit? Will Parliament sanction an outlay so extravagantly needless, merely because the Water Trustees have attempted the double oppression of forcing upon many of the citizens a water to which they have a repugnance, and of ungenerously fighting the opposition to it with the citizens' money, when a water could be obtained at a cheaper rate to which none have objected? The levy of any tax beyond what is necessary, is not a mere arbitrary encroachment on the means of the ratepayer. It has an import beyond this. Precisely in the proportion of the amount, the excess exacted is a tax upon his charity, for it diminishes the surplus of his income which he designed to be thus devoted. Or it takes in the same way from his comforts, or from his means of cultivating letters, or of fostering the arts, or promoting public or political enterprise; and to an extent which, though possibly and comparatively little for the individual, is not little in the aggregate.

With this I conclude. In the course of my remarks, I trust that I have not assumed too prominent a position for myself; my ambition having been, as I have already stated, merely to stimulate inquiry, while leaving it to others to institute and direct opposition as they shall judge fit. To no allegation or argument that had the slightest title to notice have I failed to give attention. Thus, in close-linked succession, I have seen, to my own entire conviction, many random assumptions crumble down on the one side, that carefully gathered proofs and authorities might accumulate into solid strength on the other. Perhaps never, certainly rarely, has any kindred scientific demonstration, positive as well as negative, been more complete, in any reasonable sense of what is possible with our finite means of knowledge. Even the Water Trustees may have been convinced, among the multitude of details presented to them, that the subject admits of a larger assemblage of facts, a closer testing of opinions, a more abstruse

train of argument, and a wider comprehensiveness of collateral illustration than they at first conceived to pertain to it; and that, however blindly they were willing to leap, there might be others who reasonably desired to use greater circumspection before they chose to follow. Those who can appreciate the pearls of truth I have strewn will gather them up. Others, through carelessness and indifference, may pass them unnoticed. Others still will throw them aside, and turn to seek for garbage.—I am, &c. A Physician.

P.S.—As this series of letters has now been continued over a considerable space of time, it may be proper to recapitulate here, as a consecutive whole, the conclusions which I hold to have been substantiated during their progress, and to solicit once more for these the patient attention of the reader:—

1. The human body needs for its structure and maintenance the supply of certain salts, among which are the carbonate and phosphate of lime, these being in a special manner required to give stability to the bones, but having also their further uses in the living economy.
2. The phosphate of lime is supplied to us in our ordinary animal and vegetable food, but is not presented to us in water.
3. The carbonate of lime, on the contrary, is not primarily presented to us in sufficient quantity in our solid food, but is contained in variable and more fitting proportions in spring and river waters.
4. It is from the carbonate of lime brought down by rivers into the sea that all marine animals derive the denser parts of their construction, the remains of which, during the progress of geological periods, have been, and continue to be, aggregated into huge expanses of limestone rock.
5. What has sufficed for the wants of these lower animals has sufficed also for those of the higher organisations of which man is the head.
6. Positively, this is proved and confirmed by the fact that it is in the limestone districts, where the waters are more or less hard, that man has been shown to have reached his most vigorous average physical development.
7. Negatively, this is proved also by its having been found that the mortality of our principal towns increases, on a calculation of averages, in the proportion that the hardness of the waters is diminished.
8. A water containing about six grains of carbonate of lime in a gallon is nowhere held to be a hard water, but is fitted for every use of domestic economy or manufacture.
9. Such a water, whether as a drink, or as combined with our food, presents to us in the most regular and constant of forms, and in its most simple, natural, and easily appropriated state, the carbonate of lime required for the healthy maintenance of the living system; while it is otherwise naturally preferable, because imbued with more agreeable qualities, and with higher refreshing and invigorating powers.
10. A lake water, independent of the consideration of its low impregnation with carbonate of lime, is further objectionable from its

of cholera, than any that have been since published.

deficiency of air and carbonic acid, its extreme coldness in winter and tepidness in summer, its combination with peaty and other matters, the abundant presence of living animal and vegetable organisms, and its general want of sapidity and agreeableness, and consequently its lower refreshing powers.

11. These views, so obviously concordant in fact and reason, are consistent with the natural tastes and instincts of all peoples in all ages, have been maintained by the mass of scientific men in all countries, and have been publicly ratified through the results of repeated Government inquiries.

12. Therefore, wherever a community has a choice between a water immediately derived from springs, and thus moderately impregnated with carbonate of lime, the excellence of which no one questions, and a lake water, the defective qualities of which are denounced by many, it ought unquestionably to prefer the former, on every probable consideration of comfort, health, convenience, and, in the end, were it on no other grounds than these, of the truest economy. PR.

APPENDIX TO LETTER V.

Table A.

I.

TOWNS.	Population 1861.	Hardness per Gall. over 10.	Deaths per 1000 of living 1851-60.
1. Birmingham...	212,621	15·5	27
2. Leamington ...	18,768	18·5	20
3. Guildford ...	29,330	18·5	19
4. Sunderland & South Shields	135,553	12·6	24·5
5. Southport ...	14,647	19·5	19
6. Newcastle and Gateshead ...	170,377	19·5	26·5
7. Wakefield ...	55,049	16	23
8. Dover	31,575	17	20
9. Canterbury ...	16,643	18	23
10. Norwich ...	74,440	14·5	25
11. Croydon ..	46,474	16·4	19
12. York	59,909	14·3	23
13. Derby	51,049	14·4	24
14. Lincoln... ...	47,063	11	20
15. Cheltenham ...	49,972	12	19
16. Worthing ...	18,921	17·3	18
17. Banbury ...	30,171	16·9	21
18. London (1867)	3,082,372	13	23·6
Mean	230,274	15·8	21·9

II.

TOWNS.	Population 1861.	Hardness per Gall. 10 to 6.	Deaths per 1000 of living 1851-60.
1. Liverpool and West Derby...	269,742 / 225,845	9·6	28
2. Durham	70,274	7·5	23
3. Leeds	117,566	7·5	28
4. Edinburgh* ...	168,121	7	25·4
5. Carlisle	44,820	6·1	23
6. Leith	33,628	7	23·5
7. Worcester ...	30,969	10	23
Mean	137,290	7·8	24·9

* The mean Deaths for the Scottish Towns are for the ten years 1857,-66.

III.

Towns.	Population 1861.	Hardness per Gall. 6 to 2.	Deaths per 1000 of living 1851-60.
1. Preston, ...	110,523	5·5	27·
2. Dundee, ...	91,664	4·3	28·7
3. Plymouth, ...	62,599	3	24·
4. Manchester & Salford, ...	349,323	2·5	28·5
5. Chorlton, ...	169,579	2·5	24·
6. Maryport, ...	13,707	2·3	24·3
7. Paisley, ...	47,406	2·9	27·8
Mean, ...	120,685	3·3	26·3

IV.

Towns.	Population 1861.	Hardness per Gall. 2 & under	Deaths per 1000 of living 1851-60.
1. Sheffield, ...	128,951	2	28·
2. Glasgow, ...	394,864	0·6	30·5
3. Lancaster, ...	18,347	0·6	30·
4. Cockermouth,	10,546	1·5	32·7
5. Whitehaven,	14,064	1	22·4
6. Greenock, ...	42,098	1·3	32·4
7. Aberdeen, ...	73,900	1·4	25·7
8. Perth, ...	25,250	2	26·3
Mean, ...	88,502	1·3	28·5

of cholera, than any that have been since published.

APPENDIX TO LETTER V.

Table B.

I.

Towns.	Population 1861.	Hardness per Gall. over 10.	Deaths per 1000 of living 1851-60.
1. Runcorn, ...	26,792	17·73	21
2. Warrington,	43,875	12·66	24
3. Congleton, ...	34,596	11·94	23
4. Rugby, ...	24,436	11·12	19
5. Bedford, ...	38,072	24·25	19
6. Bristol, ...	66,027	17·12	28
7. Deal, ...	12,105	18·41	20
Mean, ...	35,129	16.17	22

II.

Towns.	Population 1861.	Hardness per Gall. 10 to 6.	Deaths per 1000 of living 1851-60.
1. St Helen's ...	37,961	8·22	26
2. Northwich...	33,070	9·63	22
3. Wigan ...	94,561	8·44	27
4. Northampton	41,152	7·23	24
5. Leicester ...	68,056	9·38	25
6. Macclesfield	61,543	5·88	25
7. Accrington	17,638	6·91	29
8. Preston ...	110,523	6·25	27
9. Birkenhead & Wirral ...	79,840	8·29	19
10. Ashton-under-Lyne ...	134,753	9·94	27
Mean, ...	67,909	8·10	25·1

III.

TOWNS.	Population 1861.	Hardness per Gall. 6 to 2.	Deaths per 1000 of living 1851-60.
1. Bolton, ...	130,269	3·42	27
2. Rochdale, ...	91,754	3·58	24
3. Bury and Radcliffe, ...	101,135	3·80	23
4. Oldham, ...	111,276	4·87	25
5. Stockport, ...	94,360	5·80	26
6. Blackburn,...	63,126	4·08	29
7. Chorley, ...	41,678	3·78	22
8. Over Darwen,	21,447	4·35	34
Mean, ...	81,880	4·22	26·2

SUMMARY ABSTRACT.

Degrees of Hardness.	Mean Deaths per 1000.	
	Table A.	Table B.
Over 10 ...	21·9	22
10 to 6 ...	24·9	25·1
6 to 2 ...	26·3	26·2
2 & under	28·5	

ON THE CONDITION OF THE BLOOD, HEART AND LUNGS IN CHOLERA.

The several theories which assume that the symptoms of cholera are due to a primary blood poison are all believed to be supported chiefly by anatomical facts. It is of supreme pathological importance to ascertain the correctness of these, and of no less therapeutical moment, for treatment is often directed against the supposed blood-poison.

The post-mortem appearances of the blood, lungs and heart are among the facts to which allusion is made. Different observers differ widely in their descriptions of these, and some even candidly state that their observations are contradictory, and consequently insusceptible of any induction.

The importance of the subject renders it desirable that the supposed contradictions should be reconciled, for it cannot be that the contradictions are real. The same cause must be followed by like effects, provided all attendant circumstances be taken into consideration, and that the post-mortem appearances seen in cholera form no exception to this rule will be found, if only the cases that are similarly circumstanced be compared. Unfortunately the published cases susceptible of comparison are not numerous, for in most reports, the time after death at which examinations were made is not stated.

Few authors seem to have realised the rapidity and extent of the changes which take place in the blood, the heart and the lungs within even a very short period after death, and information as to the period of time which elapsed between death and examination has generally been omitted. Dr. Parkes in detailing cases never omits to give this essential information, he lays great stress on the importance of examining cases before post-mortem changes have occurred, and in consequence, his book, although it was written more than twenty years ago, contains more precise and trustworthy information regarding the pathological anatomy of cholera, than any that have been since published.

The conclusions deducible from an examination and comparison of the details of autopsies made at the Alipore Jail, and shown in the accompanying Tabular Statement lend no support to the blood-poison theory, and may be summarized as follows :—

1.—The blood of patients dying early in the stage of collapse, before probably the extreme of venosity is attained, like healthy blood, coagulates rapidly ; the longer collapse lasts, the more venous and incoagulable the blood becomes. In the stage of reaction the coagulability is gradually restored. the blood assumes the character called inflammatory, and this is intensified in proportion to the time that has elapsed since re-action set in. The tendency to form a clot with the cupped and buffy coat is as marked as in any of the diseases accompanied with altered nutrition, and the clots found in the heart in these cases, when the post-mortem examination has been deferred, are as firm and tough as those found in the heart after death from pneumonia or pyæmia.

The state of the blood when death has resulted from a severe burn is parallel to that in full collapse and reaction of cholera. Thus if the patient die in shock the blood is dark and there is no separation of fibrine ; if he die in reaction, firm white clots are found. To illustrate this statement the details of two cases of death from burn are arranged in the following tabular form :—

Name.	Lived after the burn.	Examined after death.	Post-mortem Appearances.
Reynolds	11 Hours ...	9½ Hours ...	Right heart filled with dark, half clotted blood ; left empty.
Chapman	8 Days ...	12 Hours ...	Heart contained hard white clots, prolonged into the vessels.

The analogy between both the symptoms and post-mortem appearance in cholera and in burns is very striking, and suggests the idea of a similar lesion producing a like disturbance of the circulation and respiration, by shock to the nervous system. This lesion may be irritation of cerebro-spinal fibres producing a

tendency to paralysis or disordered action of the sympathetic. It seems fair to conclude that in the stage of collapse the blood is prevented from coagulating from a known cause of retarded coagulation, viz., non-aeration due to the state of asphyxia,* into which the patients are thrown by the disease, and that in the stage of re-action it is contaminated as a result of altered nutrition in parts of the body which have sustained lesions. As cholera is frequently not followed by re-action, the alteration of the blood in this stage cannot be regarded as an essential part of the disease.

The possibility of the frequent formation of clots in the heart during life is pathologically of considerable interest. That clots do form occasionally is believed by almost all medical authorities. In recent text books on medicine ante-mortem clots are referred to as if their existence were generally accepted and unquestionable. We constantly read of surgical patients dying of clots in the heart, which by some is regarded as an accident dependent only indirectly on any operation the patient may have undergone and totally beyond the power of the surgeon to prevent. Formerly the formation of clot in the heart was regarded as the accompaniment of death due to the slow movement of the blood which precedes death, but the prevalent opinion now is that these clots are not merely the precursors, but actually the immediate causes of death, and that if their formation could be prevented by diminishing the coagulability of the blood, the patient might be kept alive. The experiments, observations and opinions of Dr. Richardson have chiefly been instrumental in persuading the profession that the formation of clots in the heart is a *common* cause of death.

In a lecture reported in the *Medical Times*, November 14th 1868, he expresses his views as follows :—

"As an ultimate cause of death, as the determining cause of death, separation of fibrine may still be considered as amongst the most frequent of causes ; it is the determining fatal cause in a majority of cases of acute inflammatory

* Morgagni only found the blood quite fluid after death in four instances. All these were cases in which death ensued from slow arrest of respiration. It is inferred that prolonged asphyxia in cholera produces the incoagulaber condition which is occasionally found. Separation of fibrine is only found in the stage of re-action. This often accompanies a very coagulaber condition of blood.

disorder, in croup, in scarlet fever, in malarial fever, in puerperal fever, in all the phlegmasiæ. It is a common cause of death in cases of lingering disease, and senile decay. Moreover it is now felt that the separation of fibrine from blood, is a terrible sequence to some cases of surgical operation. We are on this point much indebted to the observations of Mr. Spencer Wells, who has detected that, in many instances of ovarian dropsy where the operation of ovariotomy has been performed, the patient would have lived throughout all the chances against recovery, had not the separation of fibrine in the right side of the heart rendered the recovery, according to our present knowledge of treatment impossible."

"The local indications of disease from which many diseases take their common name, as croup, scarlet fever, erysipelas, peritonitis, pleuritis, and the like are no diseases of themselves, but the indices, probably of third or fourth value, of certain fundamental changes proceeding in the prime motion of the organism, one of which fundamental changes is the abnormal separation of fibrine from the blood. There seem to me four distinct classes of cases leading to separation of fibrine."

" 1. The true acute inflammatory cases, Type, Acute Pneumonia.
" 2. Case of stasis of blood, Type, Aneurism.
" 3. The case of febrile cachexia. Type, malarial fever.
" 4. The case of acute flux. Type, Cholera."

A careful examination of the facts on which Dr. Richardson bases his theory has convinced me that they are not sufficiently strong to support his conclusions, and I cannot help suspecting that the ready acceptance which they have met with from the profession is due to the satisfactory explanation they give of deaths which otherwise are difficult to account for by post-mortem appearances, and of disastrous results of surgical operations performed by the most skilful operators with all the precautions deemed necessary to ensure success. I was first led to doubt the correctness of Dr. Richardson's conclusions by endeavouring with the aid of his descriptions to distinguish the supposed ante-mortem from post-mortem clots. After examining a very large number of clots it seemed to me that they could be arranged in such a series that it would be impossible to decide where to draw a line separating the ante-mortem from the post-mortem clots, so gradually and imperceptibly did the appearances considered characteristic of

the two kinds of clot pass the one into the other. I became convinced that all clots were formed in the same manner, and I could see no reason opposed to their being formed in a perfectly stagnant fluid. It even appeared to me that the fact of a white clot filling a cavity of the heart, moulded on the columnæ carneæ and firmly fixed among the chordæ tendineæ was no proof, but rather the contrary, of its having been formed during life. It seemed impossible that with these large clots not only in the heart, but extending far into the arteries, life could be maintained for an instant.

I found no difficulty either in accounting for the forms and position of any of the clots on the supposition that they were formed after death. The gradual contraction of a mass of fibrine fixed at its extremities by the meshes of the chordæ tendineæ on the one side and by the valves and vessels on the other side, seemed to me to account for the shapes assumed by the clots, and the post-mortem contraction of the heart explained the removal more or less complete of the serum and semi-fluid blood corpuscles. This contraction of the heart also explained the comparative rarity of clots on the left side, as the left ventricle contracts more rapidly and completely than the right. Prolonged observation confirmed these suppositions for *cæteris paribus* the amount of fluid in the heart was found to be less, and the weight of the lungs greater in proportion to the time that elapsed between death and the post-mortem examinations.

I am not in a position to assert positively that clots are never formed in the heart during life, but I think that there is no evidence to prove that they ever do occur, and I can bring forward evidence sufficient to shew that their formation is by no means a common phenomenon. Thus the diseases in which they are said to occur in the *majority* of cases are, inflammatory diseases, senile decay, cholera and malarial fever. Were this statement true, it is very improbable that even a few consecutive post-mortem examinations in cases of death from these diseases could be made without displaying these clots in a single instance whereas the tabular statement appended shows details of twenty post-mortem examinations in which they were not present, and I have never yet found a clot when the examination has been made within twenty minutes of death, even when the symptoms which Dr. Richardson, describes as denoting the formation of clot were all present.

These symptoms it is true, accompany a very coagulable condition of blood, but they also accompany other conditions of this fluid, and it is more reasonable to suppose that they are due to a partial paralysis of the heart or disorder of the cardiac and respiratory nerves produced by the circulation of a poisoned or improper blood, than that they are caused by a solid obstruction to the circulation which, did it exist, would certainly produce *distinctly* audible signs.

Tabular Statement arranged to shew that ante-mortem clots rarely, if ever, form in the heart.

NAME.	Age.	Date of Death.	Date of Examination.	Time after death.	State of Heart and Blood.	DISEASE AND REMARKS.
Beakool, ...	40	13th December, 10-30 A. M.	13th December, 10-40 ...	10 Minutes	Heart flabby ; all the cavities full of fluid blood, which coagulated in three minutes.	This man had suffered for a long time from Chronic Dysentery, and was very anemic. He also suffered from a low form of Pneumonia, and at the *post mortem* examination a portion of the lower lobe of the right lung was found hepatized. Weight of Lung, Right ... 12 oz. ,, ,, Left ... 6 oz.
Bepeen Haree, ...	32	6th December, 9-30 A. M.	6th December, 10-30 A. M.	1 Hour	All the cavities contained fluid blood which coagulated in about ½ of an hour without separation of fibrine.	The left lung contained gangrenous abscesses. Recent pleurisy, and circumscribed empyema. Some ulceration of the large intestine. Weight of Lung, Right ... 8 oz. ,, ,, Left ... 8 oz.
Manick Mundul...	45	18th November, 8-30 A. M.	18th November, 8-40 A. M.	10 Minutes	The heart was flaccid, and all cavities contained fluid blood which coagulated with separation of fibrine in 5 minutes.	The cause of death was hemorrhage into the peritoneum from the rupture of some small vessel. Symptoms of hemorrhage came on 7 hours before death during which time he was collapsed. The Lungs were emphysematous. Weight of Lungs, Right ... 7 oz. ,, ,, Left ... 6 oz.
Noree Jogee, ...	32	15th November, 12-15 A. M.	15th November, 2 P. M.	1 Hour and 40 minutes	All the cavities of the heart contained fluid blood. The blood coagulated during the examination.	Some hepatization of the right apex. Cause of death Remittent fever. Weight of Lung, Right ... 8 oz. ,, ,, Left ... 7 oz.
Danesh Bydo, ...	32	7th November, 10 A. M.	7th November, 10-5 A. M.	5 Minutes	Heart was flaccid and all cavities full of fluid blood which coagulated rapidly with separation of fibrine. The buffy coat which formed on some collected in an earthenware vessel was half an inch thick.	Pneumonia ; some of the lobules of the left lung and an eighth part of the right lung were hepatized. The pleura of the left lung was blistered from inflammation. The contents of these blisters was solid lymph ; in places the lobes were adherent from recent adhesions. Weight of Lungs, Right ... 1lb. 3oz. ,, ,, Left ... 13oz.
Kangalee Dome,...	40	14th October, 5-30 ...	14th October, 6-30 ...	1 Hour	All the cavities full of fluid blood which coagulated in 5 minutes ; some received in a cup coagulated with a buffy coat one-eighth of an inch in thickness.	He suffered from enlargement of the spleen and anæmia. Nine hours before his death he was suddenly attacked with difficult breathing and fainted when seen by me, his pulse was feeble, but he was able to talk. Gradually his breathing became more difficult, and his extremities cold. The lungs were emphysematous. There were warts on the aortic valves. Weight of Lungs, Right 13 oz. ,, ,, Left ... 6 oz.

Name.	Age.	Date of Death.	Date of Examination.	Time after death.	State of heart and blood.	Disease and Remarks.
Horebux Sen Aheer	40	5th September ...	5th September ...	30 Minutes ...	All the cavities contained fluid blood.	The apex and posterior portion of the right lung were consolidated from pneumonia. Weight of Lungs, Right 1 lb ,, ,, Left. 6 oz.
Dhonai Sordar,...	45	20th August ...	20th August ...	1 Hour	All the cavities contained fluid blood.	Twelve hours before death complained of weakness, died of syncope suddenly. Spleen hard and large. Aortic valves thickened and insufficient.
Jawan Dwary ...	70	21st June ...	21st June ...	5 Minutes	The heart was flabby and contained fluid blood only.	Asthenia Lungs œdematous posteriorly Spleen atrophied. Weight of Lung Right, 1 lb 3 oz. ,, ,, Left, 8 oz.
Bodun Suttra ...	40	16th January 1870, 8 A. M.	16th January 1870, 8-10 A. M.	10 Minutes ...	Heart flaccid and full of blood which coagulated with slight separation of fibrine in a few minutes.	Tubercle in both lungs. Weight of Right Lung 1 lb 1 oz. ,, ,, Left 15 oz.
Mokeem Mundal	60	December 1869 10 A. M.	December 1869 10-20 ...	20 Minutes... ...	All the cavities of the heart full of fluid blood, which coagulated almost immediately.	Died of cholera without reaction within 36 hours. Weight of Lungs Right 9 oz. ,, ,, Left 7 oz.
Resha Manken...	...	26th August, 10 A. M....	26th August, 10-3 A. M.	3 Minutes ...	All the cavities full of fluid blood which coagulated almost immediately.	Died of Cholera without re-action in 10 hours. Weight of Lungs Right 9 oz. ,, ,, Left 8 oz.
Shiboo Kundu,...	45	15th June, 5-3 P. M. ...	15th June, 6-20 P. M. ...	1 Hour	Cavities of the heart full of fluid blood.	Died of cholera without reaction in 11 hours. Weight of Lung, Right 8 oz. ,, ,, Left 7 oz.
Doeram Mistry...	60	14th June, 10 A. M. ...	14th June, 10-40 ...	40 Minutes... ...	Blood in the heart fluid... ...	Died of cholera with partial reaction in 43 hours. Weight of Lung, Right 11 oz. ,, ,, Left 9 oz.
Rajoo Piocha, ...	50	10th June, 1 P. M. ...	10th June, 3 P. M. ...	2 Hours	Right ventricle contained fluid blood, left empty.	Died of cholera with imperfect reaction in two days. Weight of Lung, Right 10 oz. ,, ,, Left 9 oz.

Name.	Age.	Date of Death.	Date of Examination.	Time after death.	State of heart and blood.	Disease and Remarks.
Koosa, ...	68	12th June, 6 P. M. ...	12th June, 6-30. ...	30 Minutes... ...	All the cavities contained fluid blood	A feeble old man, died of cholera in 5 days reaction imperfect. Difficult breathing for three hours before death. Weight of Lung, Right 10. oz ,, ,, Left 8. oz
Mahdub, ...	40	20th April, 10 A. M. ...	20th April, 10-10 A. M...	10 Minutes... ...	The right ventricle contained fluid blood; no clots; blood coagulated rapidly.	Died in the secondary fever of cholera, reaction took place 5 days before death. He appeared to be doing well, when difficult breathing set in the day before his death. The lungs were emphytematous. Weight of Lung, Right 7 oz. ,, ,, Left 6 oz.
Gobeen Shaw Koringa.	...	6th April, 6 A. M. ...	6th April, 8 A. M. ...	2 Hours	Heart contained fluid blood which rapidly coagulated with separation of fibrine.	Died of cholera without reaction in 22 hours. Weight of Lung, Right 7 oz. ,, ,, Left 7 oz.
J. Martrose,	57	25th September, 6 A. M.	25th September, 8 A. M.	2 Hours	Blood in heart fluid	Lungs healthy, spleen enlarged with a cartilaginous capsule. Intestine sloughy ulcers of the large intestine. He was in Hospital for several days in a collapsed condition.
William Grey,	27	3rd October, 8 A. M. ...	3rd October, 10 A. M. ...	2 Hours ...	The right ventricle contained fluid blood the superficies of which was colourless from subsidence of the globules; the blood coagulated almost immediately with separation of fibrine.	Phthisis. Cirrhosis of the liver; sloughing and ulceration of the large intestine.
Frank, ...	24	28th Nov, 6 A. M. ...	28th November, 6 A. M.	3 Hours ...	All the cavities of the heart contained fluid blood which coagulated rapidly.	Remittent fever, with sloughing of the mouth; emaciation, Pigment in the lungs and liver.
John Moore,	30	4th January, 9 A. M. ...	4th Jan, 9-10 A. M. ...	10 Minutes ...	Heart contained only fluid blood which coagulated rapidly.	Malarial fever—peritonitis—he had been suffering for a very long time from fever caught in the Mauritius and splenitis; on the 19th of December, he was admitted into hospital suffering from tertian ague, and much emaciated; on the 25th Peritonitis was diagnosed He seemed to be progressing favourably when difficult rapid breathing with collapse came on about 10 hours before death. He was intelligent an hour before death.

State of the heart.—In all post-mortem examinations made immediately after death, whether the patient died in collapse or in re-action, the cavities on both sides have been found flaccid and moderately full of blood. No remarkable distention of the right side has been found. There is no reason, therefore, based on post-mortem appearances for concluding that there has been obstruction in the lungs. Shortly after death the left ventricle contracts, for it is always found contracted if the examination be deferred. The right ventricle in cases dying in collapse and examined several hours after death is found distended with dark clots, and in cases dying in re-action examined after the same lapse of time, it is not distended, but contains a firm light coloured clot. It seems as if in the former case the weak ventricle had been prevented from contracting by the presence of the dark thick clotted blood, and in the latter, as if not meeting with the same obstruction, it had contracted on the light coloured clot and pushed the fluid serum and corpuscles into the pulmonary artery, and even into the lung.

State of the Lungs.—The lungs, both in collapse and re-action, are as light as healthy lungs and contain very little blood, if examined immediately after death, but their weight increases gradually after death, and more rapidly and to a greater extent in re-action than in collapse, probably because in re-action, the serum from the clot in the heart penetrates the lung with greater facility than the dark, thick blood of collapse. Often in re-action the lungs are diseased and in these cases heavy, but as in some cases of undoubted and long lasting re-action the lungs are light and perfectly free from disease, it is right to conclude that lung disease is not an essential element of re-action.

These conclusions are slightly at variance with the opinions of Dr. Parkes, but they are supported by the details of all his published cases as shown in the appended tabular statement.

Tabular Statement compiled from the report of Dr. Parkes.

	Duration.	Examined after death.	Blood.	Weight and Lungs.	Remarks.
Case. Page, 417	100 hours	1½ hours	Some dark clots interlaced among the Columnæ Carneæ. The blood from the root of the lung and that from the vena cava coagulated immediately	R. 1 lb. 4 oz. L. 1 lb. 3 oz.	As the blood had such a tendency to rapid coagulation it is most probable that these dark clots among the columnæ carneæ formed during the examination.
Case. Page, 123	8½ hours	1 hour	Blood fluid, but rapidly coagulated	R. 8 oz. L. 6 oz.	Treated with injection of veins.
Case. Page, 125	14 hours	1 hour	Blood fluid, and coagulated slowly imperfectly on removal from the body	R. 8. oz. L. 8. oz.	
Case. Page, 21	40 hours	1 hour	Blood was uncoagulated an hour after the examination	R. 13. oz. L. 12. oz.	Treated with injection of veins.
Case. Page, 229	11 hours	1½ hours	Blood coagulated 20 minutes after examination	R. 14½. oz. L. 13½. oz.	Treated with injection of veins.
Case. Page, 231	X. 7 hours	4 hours	Blood fluid	R. 13. oz. L. 11 oz.	Treated with injection of veins.
Case. Page, 133	IV. 178 hours	12 hours	Polypi	R. 13 oz. L. 12½ oz.	
Case. Page, 141	V. 7 days 168 hours	12 hours	Polypi	R. 1. 10 oz. L. 1. 4 oz.	Complicated with pneumonia.
Case. Page, 149	III. 14 days	16 hours	Immense polypus	R. 14 oz. L. 13 oz.	Carcinoma of lung.
Case. Page, 183	I. 7 days	18 hours	Polypi extending far into vessels	R. 13 oz. L. 13 oz.	
Case. Page, 234	II. 9 hours	14 hours	Polypi	R. 15 oz. L. 13 oz.	

Tabular Statement showing some of the conditions of the Blood, Heart and Lungs found after death from Cholera.

No.	Date.	Time after death.	Duration of disease.	Blood.	Remarks.	Weight of Lung. Right.	Weight of Lung. Left.
1	14th March ...	22 hours ...	19 hours ...	Large soft clots	....	13 oz.	12 oz.
2	15th March ...	21 hours ...	54 hours ...	Clots extending into pulmonary artery hard and white marked by valves.	Lungs congested. Died slowly with gasping respirations	10 oz.	9 oz.
3	20th April ...	21 hours ...	8 hours ...	Clots and frothy decomposed blood	....	14 oz.	14 oz.
4	25th February	18 hours ...	26 hours ...	Clots, white color black below	Lungs not congested. Two hours before death, gaping respiration.	11 oz.	10 oz.
5	29th March ...	18 hours ...	55 hours ...	Clots, firm and white extending into pulmonary artery...	Lungs congested. Pulse perceptible throughout. Breathing became difficult a day before death. Lungs congested.	16 oz.	14 oz.
6	27th April ...	18 hours ...	10 hours ...	Clots, in the right cavities	....	10 oz.	16 oz.
7	7th April ...	18 hours ...	23 hours ...	Clots, white above	....	16 oz.	15 oz.
8	22nd April ...	17 hours ...	35 hours ...	Clots, in right ventricle	....	13 oz.	12 oz.
9	13th March ...	12 hours ...	36 hours ...	Clots, firm and white, marked by valves extending into the pulmonary artery and entangled in the chordæ tendineæ.	....	15 oz.	12 oz.
10	26th March ...	12 hours ...	6 days ...	Hard clots extending far into the artery	Difficult breathing for 24 hours	15 oz.	14 oz.
11	1st Sept. ...	12 hours ...	13 hours ...	Blood fluid	Lungs congested posteriorly and emphysematous anteriorly. Warm water injected into veins.	11 oz.	9 oz.
12	1st April ...	11 hours ...	12 hours ...	Blood fluid. No coagula	....	7 oz.	7 oz.
13	9th February	8 hours ...	17 hours ...	White clots in right auricle	....	9 oz.	8 oz.
14	18th April ...	8 hours ...	18 hours ...	Clots in ventricle—white above, dark below	....	11 oz.	10 oz.
15	18th April ...	7 hours ...	15 hours ...	Blood fluid	Pulse imperceptible after 3 motions. Perspiration profuse.	12 oz.	12 oz.
16	16th February	7 hours ...	15 hours ...	Large clots, white above, dark below	....	11 oz.	11 oz.
17	10th March ...	7 hours ...	24 hours ...	Soft discoloured clots	....	13 oz.	10 oz.
18	24th April ...	6 hours ...	15 hours ...	Clots extending into pulmonary artery	....	12 oz.	11 oz.
19	9th May ...	5 hours ...	15 hours ...	Clots soft discoloured	Lungs congested posteriorly. Pulse imperceptible after 3 Stools.	10 oz.	9 oz.
20	12th June ...	4½ hours ...	12 hours ...	A hard white clot extending into the pulmonary artery	Lungs emphysematous	12 oz.	10 oz.
21	10th June ...	2 hours ...	30 hours ...	Blood fluid, in the right cavities	Lungs emphysematous and congested. Rice water motions to the end	10 oz.	9 oz.
22	6th April ...	22 hours ...	22 hours ...	Blood fluid but coagulated rapidly	....	7 oz.	7 oz.
23	15th June ...	1 hour ...	11 hours ...	Blood fluid	Lungs emphysematous. Rice water in the intestines...	8 oz.	7 oz.
	24th March ...	1 hour ...	8 hours ...	Blood fluid	....	8 oz.	6 oz.
24	14th June ...	45 min. ...	43 hours ...	Blood fluid	Lungs emphysematous. No congestion fæces yellowish	11 oz.	9 oz.
25	12th June ...	30 min. ...	5 days ...	Blood fluid	Lungs emphysematous, very little blood in them. He had passed urine, but motions were still white. Difficult breathing 3 hours before death. He died gasping.	10 oz.	8 oz.
26	9th October...	20 min. ...	29 hours ...	The four cavities of the heart full of fluid blood which coagulated rapidly.	Lungs emphysematous; no congestion	9 oz.	7 oz.
27	20th April ...	10 min. ...	6 days ...	Blood fluid, but coagulated rapidly	Emphysema of lungs. No blood in the vessels of the lungs reaction 5 days before death. Stools coloured. Urine passed Sudden difficult breathing 12 hours before death.	7 oz.	6 oz.
28	26th August ...	3 min. ...	10 hours ...	All four cavities full of fluid blood which coagulated immediately.	Lungs slightly pink. No blood escaped on section of them	8 oz.	7 oz.

9	Male. Healthy.	9	1 hour after dinner.	20 minutes, m xlv. collected.	m xx. 0.02	m xx. 0.02	0.	0.035	0.055	0.275
10	Female. Healthy, medium height.	29	1 hour after luncheon.	20 breaths, m x. collected.	....	0.01	....	0.025	0.035	0.350
11	Female. Healthy, medium height.	24	2 hours after 6 o'clock tea.	20 breaths, m x. collected.	....	0.015	....	0.025	0.04	0.400
12	Air of Railway carriage. 8 passengers, after 15 minutes, windows shut, ventilators open, about 2 cubic feet.	...	....	Halitus condensed.	....	0.03	....	0.03	0.06	....

TABLE I.

Amount of Ammonia obtained from Healthy Human Breath by Messrs. Wanklyn and Chapman's method of Water Analysis.

(In Millegrammes.)

No.	Case.	Age.	Time.	Extent of Breathing.	Free Ammonia.	By Distillation with Carbonate of Soda.	Difference due to Urea, &c.	Organic Ammonia from Oxidation.	Total.	Amount in 100 drops of the fluid collected.
1	Male. Healthy, strong, large.	33	4 hours after late dinner.	10 prolonged breaths, about 2 in a minute.	....	not separated.		....	0.06	....
2	Male. Healthy, medium size.	35	4 hours after late dinner.	15 breaths, about 4 in a minute.	0.03	....	....	0.045	0.075	....
3	Male. Healthy, medium size.	35	5 hours after late dinner.	20 breaths, about 4 in a minute.	....	0.	....	0.10	0.10	....
4	Male. Healthy, medium size.	35	1 hour after late dinner.	25 breaths, m xx. collected.	m x. 0.03	m x. 0.03	0.	0.05	0.08	0.400
5	Male. Healthy, medium size.	35	1 hour after late dinner.	20 prolonged breaths.	....	0.035	....	0.05	0.085	....
6	Male. Healthy, strong.	31	1 hour after breakfast.	20 breaths, m xxv. collected.	....	0.035	....	0.06	0.095	0.370
7	Male. Healthy, undersized.	40	1 hour after late dinner.	7 minutes, m xx. collected.	....	0.	....	0.085	0.085	0.425
8	Male. Healthy, middle height, strong.	18	4 hours after midday dinner.	15 minutes, m xl. collected.	m xx. 0.02	m xx. 0.02	0.	0.055	0.075	0.375
9	Male. Healthy.	9	1 hour after dinner.	20 minutes, m xlv. collected.	m xx. 0.02	m xx. 0.02	0.	0.035	0.055	0.275
10	Female. Healthy, medium height.	29	1 hour after luncheon.	20 breaths, m x. collected.	....	0.01	....	0.025	0.035	0.350
11	Female. Healthy, medium height.	24	2 hours after 6 o'clock tea.	20 breaths, m x. collected.	....	0.015	....	0.025	0.04	0.400
12	Air of Railway carriage. 8 passengers, after 15 minutes, windows shut, ventilators open, about 2 cubic feet.	...	....	Halitus condensed.	....	0.03	....	0.03	0.06	....

TABLE II.

Showing the amount of Ammonia obtained from the Breath in different Diseases.

(In Milligrammes.)

No.	Case.	Age.	Time.	Extent of Breathing.	Free Ammonia.	By Distillation with Carbonate of Soda.	Difference due to Urea, &c.	Organic Ammonia from Oxidation.	Total.	Amount per 100 drops of the fluid collected.	Special Microscopic Objects.
1	Female. Measles. 10th day.	19	After breakfast.	15 minutes. m xlv.	m xx. 0.	m xx. 0.	0.	0.03	0.03	0.150	Small celled conferva, after 18 hours.
2	Male. Measles. 6th day.	28	Noon.	8 minutes. m xv.	....	0.005	....	0.02	0.025	0.175	Small celled conferva, red bodies. (Roberts, p. 83.)
3	Male. Measles. 6th day.	...	Noon.	m x.	....	0.005	....	0.01	0.015	0.150	
4	Female. Phthisis, much expectoration.	33	1½ hour after breakfast.	20 minutes. m lx.	m xxx. 0.01	m xxx. 0.01	0.	0.04	0.05	0.165	
5	Female. Phthisis, much expectoration.	28	Afternoon.	15 minutes. m l.	m xx. 0.01	m xx. 0.01	0.	0.03	0.04	0.200	Vibriones and spores, after 24 hours.
6	Female. Advanced Phthisis, both sides.	28	Afternoon.	20 short breaths.	....	0.	....	0.05	0.05	....	....
7	Female. Advanced Phthisis. Scanty expectoration.	18	11 A.M.	m vij.	....	....	....	0.02	0.02	0.290	....
8	Female. Catarrh, pregnant 6 months.	29	1 hour after luncheon.	15 minutes. m xlv.	m xx. 0.005	m xx. 0.01	0.005	0.03	0.04	0.200	Vibriones and spores, after 18 hours.
9	Male. Slight catarrh.	7	Shortly after dinner.	10 minutes. m xx.	....	m xx. 0.01	....	0.03	0.04	0.200	Very perfect epithelium.
10	Male. Slight catarrh.	32	½ hour after tea.	40 breaths. m xxx.	m xv. 0.	m xv. 0.	0.	0.045	0.045	0.300	
11	Female. Diphtheria, 6th day, improving; no albumen in urine.	28	10 A.M.	m lxx.	m xx. 0.	m xx. 0.01	0.01	0.03	0.04	0.200	Straight celled conferva.
12	Female. Whooping Cough, 4 weeks	10	1 hour after dinner.	25 minutes. m xc.	m xx. 0.01	m xx. 0.01	0.	0.05	0.06	0.300	Small celled conferva, after 12 hours.
13	Female. Whooping Cough, 4 weeks.	8	1 hour after dinner.	15 minutes. m xlv.	m xx. 0.01	m xx. 0.01	0.	0.045	0.055	0.275	
14	Male. Typhus Fever, 19th day.	20	3.30 P.M.	10 minutes. m xx.	....	m xx. 0.02	....	0.04	0.06	0.300	
15	Female. Ozæna.	34	3 P.M.	m l.	m xx. 0.02	m xx. 0.02	0.	0.08	0.10	0.500	Abundant red bodies.
16	Female. Severe Ozæna. Breath emerging from condenser free from odour.	15	10 A.M.	m xx.	....	m xx. 0.03	....	0.04	0.7	0.350	Red bodies.
17	Male. Senile. Gangrene.	70	7 P.M.	m xx.	....	0.01	....	0.02	0.03	0.150	
18	Female. Phthisis, abundant expectoration, incipient albuminuria. (Dr Roberts.)	17	4 P.M.	m xxiv.	m xij. 0.005	m xij. 0.02	0.015	0.06	0.08	0.666	
19	Male. Albuminuria slight, under hydropathy.	68	11 A.M.	15 minutes. m xl.	m xx. 0.02	m xx. 0.02	0.	0.08	0.10	0.500	
20	Male. Albuminuria. Uræmia impending. P.M. week after: large white kidney. (Dr. Roberts.)	13	4 P.M.	m xl.	....	m xl. 0.12	....	0.21	0.33	0.825	
21	Male. Albuminuria. Dropsy. P.M. in 2 weeks. Large white kidney. (Dr Morgan.)	45	4 P.M.	m xl.	m xx. 0.045	m xx. 0.08	0.035	0.10	0.18	0.900	
22	Rheumatic Fever. 12th day.	32	3 P.M.	m x.	....	m x. 0.010	....	0.02	0.03	0.300	A few red bodies.
23	Male. Albuminuria. Heart disease, Congestion of Lungs. Hæmoptysis. (Dr Roberts.)	40	4 P.M.	m xxiv.	m xij. 0.025	m xij. 0.025	0.	0.04	0.065	0.545	

TABLE II.

Showing the amount of Ammonia obtained from the Breath in different Diseases.

(In Millegrammes.)

No.	Case.	Age.	Time.	Extent of Breathing.	Free Ammonia.	By Distillation with Carbonate of Soda.	Difference due to Urea, &c.	Organic Ammonia from Oxidation.	Total.	Amount per 100 drops of the fluid collected.	Special Microscopic Objects.
1	Female. Measles. 10th day.	19	After breakfast.	15 minutes. m xLv.	m xx. 0.	m xx. 0.	0.	0.03	0.03	0.150	Small celled conferva, after 18 hours.
2	Male. Measles. 6th day.	28	Noon.	8 minutes. m xv.	....	0.005	....	0.02	0.025	0.175	Small celled conferva, red bodies. (Roberts, p. 83.)
3	Male. Measles. 6th day.	...	Noon.	m x.	....	0.005	....	0.01	0.015	0.150	
4	Female. Phthisis, much expectoration.	33	1½ hour after breakfast.	20 minutes. m xx.	m xxx. 0.01	m xxx. 0.01	0.	0.04	0.05	0.165	
5	Female. Phthisis, much expectoration.	28	Afternoon.	15 minutes. m L.	m xx. 0.01	m xx. 0.01	0.	0.03	0.04	0.200	Vibriones and spores, after 24 hours.
6	Female. Advanced Phthisis, both sides.	28	Afternoon.	20 short breaths.	....	0.	....	0.05	0.05	....	....

From the *Journal of Anatomy and Physiology*, Vol. IV.

ON THE ORGANIC MATTER OF HUMAN BREATH IN HEALTH AND DISEASE.

By ARTHUR RANSOME, M.D. Cantab. Manchester.

THE following analyses of the amount of organic matter contained in human breath were made by the method of Water-analysis invented by Messrs Wanklyn and Chapman. The aqueous vapour of the breath was condensed in a large glass flask, surrounded by ice or snow and salt, by which a temperature of several degrees below zero was obtained. In the first essays the number of breaths was counted, and the flask washed out with distilled water; but this was soon found to be unsatisfactory, as the extent of the expirations varied so greatly. The aqueous vapour was then collected and measured and tested as follows.

If enough fluid had been obtained, a certain quantity (generally about 20 minims) was mixed with 50 c. c. of distilled water, and tested for free ammonia by means of the Nessler test. An equal portion of the fluid was then mixed with 30 minims of a saturated solution of carbonate of soda and about 10 oz. of pure distilled water, ascertained, by further distillation, to be free from ammonia. The mixture was then distilled and the distillate tested for ammonia until it ceased to give any indications of its presence. This testing would give all the free ammonia, together with any of this gas arising from the action of the carbonate of soda—for instance, from the decomposition of urea[1]; 50 c. c. of a strong solution of permanganate of potash and caustic potash were then added to the retort and distillation again continued; the quantity of ammonia now given off would arise from the destruction of organic matter. The results of these examinations are given in the following tables. Table I. giving the records relating to healthy breath. Table II. of breath from persons affected by various disorders. In both Tables are given in successive columns (1) the number of the observation, (2) the nature of the case, (3) the period of the

[1] See *Water Analysis*, by Wanklyn and Chapman. Trübner and Co. p. 55.

day, and (4) the extent of breathing; then follows in millegrammes the amount of free ammonia or ammoniacal salts determined (5) directly by the Nessler test, and (6) by distillation with carbonate of soda; a column (7) is then provided for any difference between these two readings, giving the ammonia from urea, or other matter decomposable by the weak alkali. The ammonia obtained by oxidation of the organic matter comes next, (8) then the total amount of ammonia obtained, (9) and (10) a calculation of the quantity of ammonia to be obtained from 100 minims of the fluid collected; finally a note is appended to those cases in which any peculiar microscopic appearances were observed.

The number of examples I have collected is still small, but they are brought forward now in the hope that others may be induced to undertake the same enquiry. It is one which requires many observers, and I think that the results so far as they have been obtained justify the attempt to enlist others in the work.

I. Healthy Breath.

The breath of 11 healthy persons was examined and the quantity of aqueous vapour was ascertained in 7 instances. The persons examined were of different sexes and ages, and the time of the day at which the breath was condensed varied.

It may be observed that the amount of free ammonia varies considerably, and I have not so far been able to connect the variation with the time of the day, the fasting or full condition.

It has been stated by more than one observer that urea is sometimes present in the breath, it was therefore sought for in 15 instances, 3 healthy persons and 12 cases of disease, but it was only found in two cases of kidney disease, in one case of Diphtheria, and a faint indication of its presence occurred in the breath of No. 8, Table II, a pregnant female suffering from catarrh[1]. The quantity of ammonia arising from the destruction of organic matter also varies somewhat, possibly from the oxidation of albuminous particles by the process of respiration; but it may be noticed that in healthy persons there is a re-

[1] No Albuminuria was present in these two last cases.

markable uniformity in the total quantity of ammonia obtained by the process: amongst adults the maximum quantity per 100 minims of the fluid collected was 0·425 and the minimum is 0·35 millegrammes. It is not easy to estimate the total quantity of organic matter thus got rid of by the lungs of even healthy persons. We are told by Messrs Wanklyn and Chapman that every part of organic ammonia discovered corresponds to about 10 parts of albuminous matter, but, on the other hand, the quantity of aqueous vapour carried off by the breath varies with age and season. If, however, we take the ordinary quantity of this fluid, for an adult, to be about 10 oz. in the 24 hours, and the average amount of ammonia given off as 0·4 of a millegramme in every 100 minims of fluid, then we obtain the rough approximation that in ordinary respiration about 0·2 of a gramme or 3 grs. of organic matter is given off from a man's lungs in 24 hours. At first sight this seems to be a very minute quantity to be thus disposed of; but when it is considered that the most impure water, examined by the authors of the process, only contained 0·03 of a gramme of organic matter per *litre*, it will be allowed that there is ample quantity to permit of putrefaction, and to foster the growth of organic germs.

We cannot doubt that the diseases which arise as a consequence of overcrowding, find at least a starting-point in the impure vapours arising from the lungs, and the general surface of the body.

II. In Disease.

In diseased states of the system we find a much greater variation in the amount and kind of organic matter given off. The breath of 23 cases of disease was examined. In 3 cases of Catarrh, in 2 of measles, and 1 of Diphtheria, the total ammonia obtained was much less than in health; a result which is probably due to the abundance of mucus in those complaints by which the fine solid particles of the breath were entangled. The cases of whooping-cough were children, and therefore the deficiency noted in the organic matter given off by them may be due to age, and this is the more probable since the only healthy child's breath examined contained about the same quantity (0·275 of a millegramme) of organic ammonia, considerably less than the breath of any healthy adult.

In two cases of Phthisis, with abundant expectoration, the total ammonia was also less than in health; but in one case of this disease with abundant purulent sputa, associated however with Bright's disease, a large amount of organic matter was given off. We cannot doubt however that the albuminuria which was present in this case had an influence upon the result. A portion of the ammonia was in fact due to urea, or to some kindred substance; and we may perhaps ascribe the general excess of organic matter to some peculiarity in the breath due to the kidney disease. It is in truth in kidney diseases that the largest amount of organic matter of all kinds is to be found in the breath. Five cases suffering from these diseases are recorded. In two cases urea was found, in one it was not sought for, and in two others it was absent. The free ammonia in all the cases is abundant, in two of them (Nos. 20 and 21) excessively so, and the organic ammonia is also large in amount. The total quantity of ammonia found is in excess in all the cases; in one it rises as high as 0·9 millegrammes in 100 minims of fluid, and in another to 0·825.[1]

Probably if the sputa in these cases had been examined, a much larger proportion of matters decomposable by Carbonate of Soda would have been found. I would suggest that the presence of these substances either in the bronchial mucus, or in the aqueous vapour of the breath, would be a fair indication that their elimination by the kidneys and skin was deficient, and that measures should be taken to improve the action of these organs. In one case of Ozæna the total quantity of ammonia obtained was greater than in any of the healthy subjects, but the free ammonia did not seem to be in excess. In another case, however, a girl of 15, whilst the total quantity of the gas was probably not greater than normal, the free ammonia formed nearly half the amount collected.

The case of typhus fever was obtained in the fever wards of the Manchester Royal Infirmary; but it was scarcely a fair example of this disease, since it was already convalescent.

[1] 4 of these cases of kidney disease were in the Manchester Royal Infirmary, under the care of Dr Roberts and Dr Morgan.

There was however, apparently, a deficiency in the total amount of organic matter got rid of from the lungs. I might have attributed this fact to the feebleness of respiratory power, the blast of air being insufficient to carry with it much foreign matter, had not the cases of kidney disease (Nos. 18, 20, 21, and 23) been equally if not more feeble. This explanation is however still a possible one, and it is strengthened by the fact that in the case of senile gangrene, a feeble old man, but without catarrh, the organic matter of the breath is very small in quantity. The case of rheumatic fever showed no very definite peculiarity; the organic ammonia was slightly less than in health.

As a matter of curiosity, the air of a railway carriage, containing 8 persons, was examined, after 15 minutes' occupation, with the windows shut and the ventilators open. In this instance the breath was inspired through the apparatus; about 80 inspirations being taken, probably between 2 and 3 cubic feet of air would thus pass through the freezing mixture; very little moisture was condensed, but what was obtained was strongly charged both with free ammonia and organic matter (see Table I. No. 12). Before considering the *nature* of the organic matter to be found in human breath, it may be well to advert briefly to the prior question of the amount and kind of organic matter in the air breathed.

There has lately been much discussion as to the priority of the discovery of organic matter in both fresh and respired air. And yet it is certain that from very early times men have recognized the fact that the air is the vehicle of many substances, both organic and inorganic. The old writings are full of disquisitions upon the teeming air. Boerhaave[1] calls it the "instrumentum catholicum" and speaks of the "corpuscula... quæ in aere perpetuo obvolitent," and he shows how "Terra tota ex aere cadentia recipit omnia, ita rursum aer de Terra universa accipit. Fitque inter bina hæc perpetua, quasi omnium revolutio, distillatio assidua." Medical men of all epochs have been only too prone to ascribe diseases to the constituents of the atmosphere, and since the time of Spallanzani it has been surmised that fermentation and putre-

[1] *Elementa Chemiæ* (Leyden Ed. 1732), p. 484.

faction were the result of the action of living animals or plants whose germs were derived from the air. A conclusion which has steadily gained strength through the researches of Astier, Schwann, Cagniard de Latour, Turpin[1], and more recently of Pasteur. The great controversy which has now been going on for many years, chiefly between Mon. Pasteur and Mon. Ponchet, on the subject of spontaneous generation, turns entirely upon the difficulty of keeping out of the experimental flasks all taint of organic matter from the atmosphere.

I do not know who first used cotton-wool as a filter for the air, but it was certainly employed many years ago by different observers, Schwann, Schroeder and Dusch, Helmholtz and Van den Broek.

It is however to Dr Angus Smith that we owe the discovery of the large proportion of organic matter contained in respired air, and the readiness with which living organisms develop in the condensed breath of crowded meetings. He has also shown the presence of organic matter in the air of different places. The following table gives the quantity washed down by the rain.

TABLE III.

AMMONIA IN RAIN WATER.

Place.	Date.	Ammonia parts in 1000000, or grammes in a cubic metre.	Ammonia of Albumen.
Row, near Helensburgh...	Jan. 16, 1869	0.00	0.
Clydeford, Glasgow.........	Jan. 1869	1.25	0.0
London Hospital...........	Feb. 1869	2.	0.3
" "	" "	2.2	0.3
" "	" "	3.	0.4
Glasgow, St Rollax.........	Dec. 1868	3.75	0.
Glasgow, Netherfield	Jan. 1869	5.5	0.
Manchester	Dec. 1868	6.	1.
Newcastle on Tyne.........	Dec. 1868	5. and 0.6	0.

[1] "Point de fermentation, sans l'acte physiologique d'un vegetation." *Comptes Rendus*, VII. p. 392.

In an Appendix to Dr Angus Smith's last report to the Privy Council, upon the working of the Alkali acts, Mr Dancer has remarked upon the nature of the organic matter contained in the washings of 2495 litres of the air of Manchester.

He discovered in these many forms of life, fungoid matter, sporidiæ and zoospores, and much lifeless organic substance, vegetable tissue, partially charred objects, fragments of weather-worn vegetation, hairs of leaves, fibres, cotton filaments, granules of starch, and hairs of animals. Mr Dancer makes the calculation that about "37½ millions" of spores or germs of organic matter would be contained in the quantity of air examined—an amount "which would be respired in about 10 hours by a man of ordinary size, when actively employed."

I would submit however, that in this calculation there is a serious possibility of error. There seems to have been a considerable interval of time (how long is not stated in Dr Smith's report) between the commencement of the collection of the fluid and the examination of it by the microscope. It is well known how rapidly organisms increase in numbers in suitable fluids, and it seems reasonable to believe that many of the spores discovered by Mr Dancer may have been developed in the fluid itself.

I have myself made a few observations upon the organic contents of respired air which may be interesting at the present time. In the year 1857, in consequence of a letter in the *Times* newspaper, signed 'Investigator,' I exposed glass plates covered with glycerine in different places, amongst others in the Manchester Infirmary, and in the dome of the Borough Gaol, Manchester. In this latter establishment all the air from the cells is conducted, by the system of ventilation employed, into the dome. The plates were afterwards carefully searched with the microscope, but at that time I could recognize little except fibres of cotton and wool, and shrivelled epithelial scales; there were also some singular looking bodies, but these I found afterwards were contained in the glycerine used to cover the slips of glass.

Upon another occasion during a crowded lecture at the Free Trade Hall, about 3000 persons being present, I drew the air from one of the private boxes (raised about 40 ft. above the audience) by means of exhausting bellows, through a system of narrow tubes, filled with distilled water; the operation being conducted for a space of about 2 hours. The water was emptied from the tubes, allowed to settle for 36 hours, and the sediment was examined microscopically. The following objects were noted at the time and sketched under the microscope, the ¼ inch power being used:—fibres; separate little cellules; nucleated cells, surrounded by granular matter (about 6 in 1 drop of water); numerous scales like degenerated epithelial scales.

The dust from the top of one of the pillars in the private boxes which had not been disturbed for three weeks, was also examined shortly afterwards, and the following objects were noted as being present:—1. A few fibres of cotton and wool. 2. Variously shaped and sized black masses, which were taken to be specks of coal dust. 3. Semi-transparent little lumps refracting light strongly. 4. Crystalline substances having a laminated texture (query fragments of glass). 5. Shrivelled pieces of membrane, epithelial scales. 6. Collections of granules. 7. Variously coloured fragments, blue, pink and yellow; probably portions of dress.

I have also searched with the microscope most of the specimens of aqueous vapour from the lungs. In all of them epithelium in different stages of deterioration was abundantly present, and a difference in the appearance of the scales could be marked according to the age of the patient, those from young persons being notably the most perfect and fresh looking. In one case of kidney disease, the only one examined, they had a granular appearance. Probably a large portion of the organic matter of the breath consists of these epithelial particles. Very few spores were found in any fresh specimen; but, on the other hand, after the fluid had been kept in some instances for only 12 hours, even in a cold room, myriads of active vibriones and many spores were found. In several instances—in one healthy person, in two cases of ozœna, in one case of measles and one of rheumatic fever—very abundant specimens of the red and yellow bodies, called "pigmentary particles" by Dr W. Roberts[1], were found, and it was noticed that after being kept for a day

[1] *On Urinary and Renal Diseases*, p. 83.

or two, the colour of these bodies darkened materially. In one case of diphtheria, straight-celled, greenish-coloured, confervoid filaments were noticed; and in three other cases, two of measles and one of whooping-cough, abundant specimens of a small round-celled conferva were found, resembling the Penicilium glaucum, and these were seen to increase in numbers and in size for two days, after which they ceased to develop.

It may be interesting to note that the fluid in which these objects were found was neutral or slightly alkaline, whereas the mould-fungus generally prefers a slightly acid fluid. These differences in the nature of the bodies met with are interesting as showing some occult differences in the nature of the fluid given off in the several cases, but many additional observations would be needed before we could draw any inferences from them.

They certainly do not as yet afford any proof of the germ theory of disease, nor do they justify the alarming doctrines which have been rife of late, as to the presence of organisms in the breath. They simply show the readiness with which the aqueous vapour of the breath ferments or putrefies, and the consequent danger of overcrowding, and the paramount importance of ventilation.

REPORT

ON

Means for Warming & Ventilating

THE INFIRMARY AT CHESTER.

PRINTED AND CIRCULATED
BY ORDER OF THE COMMITTEE.

CHESTER:
PULLAN, PRINTER, WATERGATE STREET.
1864.

TO THE GOVERNORS OF THE CHESTER INFIRMARY.

My Lords and Gentlemen,

In consequence of notes in the Visitors' Book, made by Lord Westminster, the Right Honourable W. E. Gladstone, M.P., and others, "On the defective Ventilation of the Infirmary," the Weekly Board, on 8th November, 1864,

Resolved—"That a Committee, consisting of

THE MARQUESS OF WESTMINSTER, K.G.,
THE CHAIRMAN AND VICE-CHAIRMAN OF THE WEEKLY BOARD,
MR. ALDERMAN TREVOR,
MR. ALDERMAN GRIFFITH,
THE CANON IN RESIDENCE,
THE REV. ARTHUR RIGG,
JNO. NEVITT BENNETT, ESQ.,
AND THE MEDICAL OFFICERS,

be appointed to consider . . . . the Ventilation of the House."

In accordance with this Resolution, the Committee met on 11th November, The Marquess of Westminster in the Chair; it was then

Resolved—"That the Rev. Arthur Rigg be requested to make an examination, and draw up a Report on the present state of Warming and Ventilating the Building, with suggestions for alterations or additions."

At an Adjourned Meeting, held on 25th November, 1864, the following Report was submitted, and a Resolution passed, "That it be printed and circulated."

Under these circumstances, a copy is forwarded to each Governor, in the hope, that thus the subject may receive that attention its great importance demands.

When it is borne in mind that Patients are sent to the Infirmary from distant parts of the county, the claim for consideration is not a slight one. The Committee would therefore feel obliged by any communications upon the subject, addressed either to the Secretary or the writer of the Report, (the Rev. Arthur Rigg, College, Chester); and they would wish the document now issued to be regarded as containing suggestions for consideration rather than perfected schemes for adoption. The future course of the Board must be guided by these communications; it is, however, hoped that they may shortly be enabled to take such action as the necessity of the case manifestly requires.

JOHN JONES,
SECRETARY.

Board Room, Infirmary, Chester,
6th December, 1864.

REPORT
ON
MEANS FOR WARMING & VENTILATING
THE INFIRMARY AT CHESTER.

The Building.—The Building is arranged in the form of a square, enclosing on all sides a small Court-yard. There are large Cellars in the Basement. The Rooms of the Resident Officers and Servants are on the ground floor. The relative positions of these may be seen on the Plan. On each of the two floors above the Rooms numbered 5 to 9, and the adjoining passages, are two large Wards for Male Patients. Above 13 to 20 is similarly arranged for Female Patients. Access to these Wards is had by passages, which project over what was once the inner Court. An additional projection contains, on the ground floor, two Bath Boiler-rooms, numbered 23, 25, Store-rooms 24, 26, and two Water-closets, 32, 33. On the upper floors the corresponding additions, with Rooms for Nurses and requisite offices, are numbered 28, 29, 30, 31. There are four of these: one is shown in the margin. The Rooms 28, 29 and 31, are supported on pillars, arranged as marked by a dotted line on the Ground Plan. Access is had to the various Rooms by passages about five feet wide, lighted from the inner Court.

Existing Provision for Ventilating and Warming.—The means now provided for communication between the Rooms and outer air are Chimneys, Windows, Doors; and (between the West or Walls Front, and the inner Court-yard) two Open Grids, each about 7 ft. 6 in. **X** 3 ft. 0 in., marked [*aa*].

In the Upper Wards, openings have been made through the ceilings, thus establishing a communication with the space under the slates.

But little air appears to pass through the "Walls Front" Grids, for the soot of adjoining chimnies accumulates upon the floor of the inner Court-yard. As the air in this inner Court obtains access to the Rooms and Wards by Windows, and as there are not less than ten Water-closet Windows, four Bath-room Windows, four Nurses-room Windows, two Furnace-room Windows, opening into the Court, measuring about 200 square feet, and the Water-closet ones seem always fully open, perhaps it need not be regretted that so little air passes from the inner Court to the Rooms of the Infirmary. These Grids are the only special arrangements that have been made for Ventilating purposes.

The plans for Warming are open Fire-places. In the Wards, these are small compared with the size of the Rooms and the number of Patients; they partake, too, rather of the form of cressets, or cages, for the burning of fuel than of places for the production and distribution of Heat. In considering, therefore, the future Warming and Ventilating, it may be assumed that no contrivances now exist, by which the Authorities can control the admission and dis-

tribution of currents of Heated or Cooled Air. For it must be conceded that ventilation by open windows is not under control—the direction of the wind, and not the will of the Authorities, determines whether the air makes an entrance or an exit at an open window.

Means for Controlling Ventilation.—One of two means is usually resorted to, for controlling the Ventilation of Rooms in which persons are *continually* resident. The most common is—The Extraction of Air by some of the many schemes for Exhausting or Drawing Air out.

The other is—Forcing Air into the Room. Although in each case fresh air is introduced, and that which has been vitiated removed; yet the effects are so different, that circumstances must determine which plan is most likely to meet particular requirements.

In the process by Exhaustion, every crevice becomes an inlet for air, and there is thus at each chink or cranny, a draught, hence the draughts in ordinary dwelling-rooms with fires, which depend for the combustion of fuel upon the air of the room. Further, the intensity and character of the burning are continually changing the amount of air influenced by these causes.

In the process by condensation, these crevices become outlets, and consequently there are no draughts from them.

As these leading principles will be referred to hereafter, the former will be called the Vacuum, and the latter the Plenum System.

Source for Air.—The two Grids at the "Walls Front" present an available means for a large supply of Air, and that too from the side where the Air may be expected to be most pure.

Water as a Conveyer of Heat.—To warm Air for purposes of Respiration, and to store up Heat which may be slowly imparted to currents of cold air, Water is the best conveyer between the Fuel and the Air to be warmed. Our inability under ordinary circumstances to raise the Temperature of Water so high as to prove injurious to health by the decomposition of organic matter in the Atmosphere is one great recommendation. And further, its "Capacity for Heat" renders it most efficient in producing a uniformly mild Atmosphere. This will be manifest when it is considered that 1 lb of Water, in losing one degree of Temperature, would heat 4 lbs. of Air one degree. Now, Water is 770 times heavier than Air, therefore, comparing volumes, one cubic foot of Water in losing one degree of Temperature would heat 3080 cubic feet of Air one degree. To this latter property (for example) is due the fact that, although in various parts of the Continent fruits grow which our summers cannot ripen, yet in these same parts our evergreens are unknown, for they cannot live through the winters. The winter of an Island, (even Iceland) is, as a general rule, milder than that of Lombardy.

Influence of Gases, Vapours, Odours, &c.—The influence of Gases, Vapours, Odours, Moisture, &c., upon Heat is so important, that this Report should direct the attention of the Authorities of the Infirmary to it. As an illustration of the question now under consideration—The light of the

moon is the reflected light of the sun. The light of the sun abounds in heat, and yet in the light of the moon there is so little heat that *practically* there is none. Again, how common is the use of glass fire-screens to permit the passage of Light, but intercept that of Heat. It is evident, therefore, that Light may remain apparently unaltered, and yet its usual ally, Heat, may be withdrawn from attendance upon it.

From these facts, which are patent to all who make for themselves the observations, let us pass to others which, although less easily observed, are not on that account less true. Many Odours, Gases, Vapours (more particularly that from water) possess the power, as in the illustrations given, of absorbing Heat whilst permitting the free passage of Light. Hence the amount of Odour, or of Moisture, in the atmosphere of a room materially affects the distribution of Radiant Heat. These considerations, when generalized, seem worthy of attention, especially in an Infirmary, when the means of Warming and Ventilating are being discussed.

Again, this absorption of the rays of Heat, except by the constituent simple elements of Air and Water, is sometimes in a very high degree. Now in the Wards of an Infirmary, and especially in the atmosphere which rests about and around wounds and sores, there are Odours of very varied kinds. May not these form almost impenetrable barriers to the Heat which would otherwise freely radiate from erysipelatic and other sores? It should be also borne in mind, that if these Odours absorb Heat-rays, they become themselves centres of Radiated Heat, and in fact return to the wounds and sores Heat received from them; thus excluding the cooling influence from Radiation almost as effectually as though a bright metallic concave reflector were suspended over the wound. This question obviously embraces others; and if the retentive capacity of odours for the poisons of the liquids or solids by which they are caused is at all analagous to that of gases for the heat or light-rays emanating from the combustion of the liquids or solids from which they are produced, then there is little difficulty in accounting for much that is but partially comprehended in the communication and continuance of disease. The only apparent remedy is *perfected* Ventilation, and that, too, of pure fresh air, where the atmosphere, as at Chester, can supply it. Athough the purity of air in the centres of many large towns cannot be compared with that of Chester, yet where impurity is caused by a moderate admixture of carbon *only*, beneficial rather than deleterious results are not unlikely to follow. Hence, perhaps, arise certain anomalies in Hospital experiences, which do not seem to admit of obvious solution, and which introduce apparent discrepancies in facts and opinions, when questions of Warmth and Ventilation are being considered. The conclusion, however, seems forced upon us, that by Ventilation mainly, can these Absorbent and Radiant Poisons be removed from the vicinity of the sources which generate them. For if not removed, they apparently avail themselves of their 'retentive capacity,' and of the moisture usually attendant upon sores, to perpetuate their own existence, and materially aggravate those sufferings, the relief of which is a main object in an Infirmary.

The Medical Officers will, I feel sure, excuse my reminding them of these facts. The importance of Ventilation is so generally based upon general considerations only, that its

direct influence upon wounds, sores, and diseases, is sometimes overlooked. Permit me further to remind you, that this question deals with *absolute* amounts of Heat, and not with Heat only thermometrically manifested. Thermometers indicate variable temperatures, but they do not indicate the amount of Heat which cause those variations.

Apparatus for Warmth.—Guided by these considerations, it is essential that a place be chosen where the Heat, having been communicated to the Water, may be conveyed by it to suitable receptacles for the use of the Infirmary. The basement floor under the Rooms Nos. 1, 2, 3, 21, 22, is now used as a Laundry, Mangle-room, &c. There are other places where these could be as conveniently located. It is therefore suggested, that a Boiler be erected in one of these rooms. Greater facilities exist here for the delivery and storage of coals than in any other part of the building; and access could be had to the Apparatus (if requisite) from the outside only of the Infirmary. From the boiler two pipes (one for outflow and one for in-flow) should be brought to each of the Rooms in which at present boilers and worms are fixed for the heating of water for the Baths, and general purposes of all the Wards. These Rooms are numbered 23 and 25 on the Plan.

Systems of four-inch pipes should be erected here, and the whole fitted with proper expansion-boxes and joints, as well as valves and thermometers. Similarly connected with the Boiler, viz., with out-flow and in-flow pipes, it would be well to place appropriate extents of Heating Surface in the Patients' Waiting-room [No. 21] under the stairs, and on the ground floor passage in the Windows, where marked thus [x] The central source of Heat and extent of Heating Surface should be in excess of any ordinary requirements, in order that, as a rule, the temperature of the Water circulating may be as low as possible; since the less the temperature is elevated, the more agreeable is the quality of the warm air.

Water for Baths.—Should the Plans described be carried out, then arrangements could readily be made for the erection in the future Boiler-house, of means for warming water, and supplying it to the respective Baths and Nurses'-rooms; even the piping as laid down would be available; and the present system might be continued or improved, as the Authorities may determine. This particular question need not therefore, at present, be considered.

Distribution of Heat.—Air-ducts, viz., tubes of zinc covered with boiler-felting, or of laths well and smoothly plastered inside and outside, should be formed from each of the two larger systems of pipes, in the Rooms numbered 23 and 25. The sectional areas of these ducts being proportioned to the demand. Branch tubes, similarly constructed, are to be fixed to the ceilings, under the Rooms to be warmed, along a central line. Small perforations, increasing in number as the distance from the source of heat is greater, must be made at intervals above the ducts, extending to a breadth of twelve or eighteen inches.

The systems of heating pipes, on the male and female sides respectively, are appropriated to the corresponding Rooms; but provision for throwing the Heat of either into the air-

ducts of the other should be made. Proper valves, or hinged doors, might be placed in the pipes where requisite. These valves might conveniently be made of light wood framing, covered with canvas, and painted; access to the moving of them being had only by a key, in possession of the attendant. Not only is the distribution of Heat regulated by these valves, but power is had to raise the temperature of any Ward, or portion of a Ward, as may seem desirable.

Egress of Vitiated Air.—Ingress for warm and fresh air being along a centre line in the floors of the Wards, Egress for vitiated air may be provided at the ceilings near the side walls. Such being the relative positions of inlets and outlets, Patients would have the advantage of Warmth and Ventilation more by a process of diffusion than of current.

From the Lower Wards the outlets must be carried to the space between the ceiling and the roof by flues, boxed in against the walls of the Upper Wards. These boxings need not project more than six or eight inches into the rooms. From the Upper Wards, openings in the ceiling would admit the vitiated air to the same space. Having thus collected under the slates discharge ducts for vitiated air, they should be brought in sets, or groups, to openings through the roof, provided with moveable louvres, and placed as may seem most suitable, when the details are being determined.

Ventilation.—Ample provision having been made for the admission of either warm or cooled air, it is essential that such air should enter. Opening of inlets does not ensure entrance. The only known means by which entrance can be compelled, or induced, are those based upon the principles described on a previous page.

A combination of these seems best adapted for the Chester Infirmary. Reference to the Plan of the Floors will shew how conveniently the Rooms allotted to each system are located. The Wards should be ventilated upon the Plenum System—the Water-closets, 28, 31, 32, 33, and six others; Bath-rooms, 29, and three others; Store-rooms, 24 and 26; and Nurses'-rooms, 30, and three others; being placed on the Vacuum System.

Ventilation of Wards.—The whole of the air required for this purpose may be obtained from the West Front Grids [*aa*]. In certain directions of wind and states of atmosphere, the ventilation would be accomplished by simply opening the appropriate valves.

On many days, however, this would not be the case; and no system of Ventilation can be satisfactory which is not effective at all times, and in the degree required. The following arrangements are suggested to enable the Authorities of the Infirmary to exercise complete control over the Ventilation, irrespective of either the intensity or direction of atmospheric currents:—

Two rotating noiseless Fans (one for each system of warmed piping) should be placed with their central openings in closed communication with the West Front Air Grids [*aa*]. The rotation of these, combined with the adjustment of the inlet of the

air-ducts previously described, would ensure a uniform and duly proportioned supply of air to all, or any one or more of the Wards. Whether in winter or summer, suitable air could be admitted; for, whilst by the systems of piping, requisite heat for winter is provided, they are means for partially cooling air in summer. Various plans for driving these Fans might be suggested—as by the agency of Water, Weights, Electricity, Air, Steam, Gas, Animals, &c. &c. After carefully considering these and similar respective sources of power, none seems likely to be so economical and efficient as Steam. Independently of other purposes to which it may be applied, it would, in regard to Ventilation, very much promote the health and comfort of the infirm—and this is no doubt, the chief purpose of an Infirmary, as distinguished from a simple Dispensary. Therefore near the Boiler, to be used as a source of Heat, there should be placed suitable steam apparatus for driving the Fans when the motion of them is requisite.

Ventilation of Water Closets, Bath-rooms, Store-rooms, and Nurses'-rooms.—Although the Vacuum System of Ventilation is less easily controlled, and more irregular in its action than the Plenum, yet it offers many features well adapted for the purpose to which it is here proposed to be applied; and even those of its characteristics, which are objectionable in some cases, become recommendations here.

The modes of application to the above-mentioned parts of the Infirmary are simple. Lengths of down spouting for rain being fixed in the corners near Rooms 29, 31, and the corresponding ones, and the socket-joints rendered air-tight, to the extent of about six inches of water; branches should be taken, having one end terminating near the ceilings of the respective Rooms, and the other in the descending spouting. Each of these descending spouts should be led, by a closed drain, to the ash-pits of the Apparatus for supplying the Baths with warm water. As the fires of this Apparatus are in full operation night and day, in winter and summer, ample provision for Vacuum Ventilation can be had. The amount of air drawn through the Rooms now under consideration, could be materially regulated by the ash-pit door, when the experience of a week or two had determined the areas of the Room orifices. For example:—The combustion of one ton of coals, employed in heating water for the Baths and general purposes of the Infirmary, could induce a quantity of air to pass through these pipes, which would fill more than fifty miles in length of them.

Conclusion.—In conclusion, it may be well that the Authorities of the Infirmary should direct their attention to the size of the Pneumatic Machine, which must be kept working, in order that an ample supply of healthful fresh air may be furnished to the Inmates. This may be estimated from the circumstance, that provision should be made for passing through the building every day, as much air as, if put into a tube one foot square, would extend to a distance of about eight hundred miles.

The very magnitude of the work to be done, sufficiently indicates the extent of the appliances requisite. Whether, therefore, the opening of windows and other orifices left to the influence of casualties, or whether these, aided by such means as ingenuity can suggest, or whether means of

sufficient magnitude for the whole work, irrespective of casual openings, are most desirable, is a question on which there will be different opinions. However scientifically perfect a plan may be, yet if it is too small for the work required, failure, really due to deficiency in power, is often attributed to the principle of the apparatus. Such is no unusual charge against means for artificial Warmth and Ventilation; therefore, between the Governors of the Chester Infirmary and those whom they may employ in regard to the subject of this Report, it would be well there should be a clear understanding as to how much is to be accomplished.

ARTHUR RIGG.

COLLEGE, CHESTER,
24TH NOV., 1864.

Pullan, Printer, Watergate Street.

Present Allotment of Rooms on the Ground Floor of the Chester Infirmary.

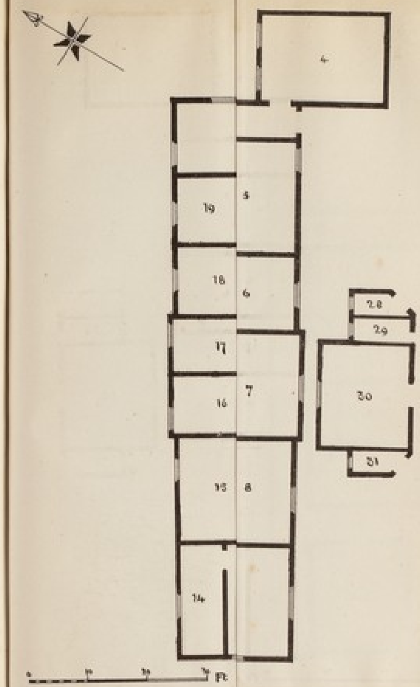
No. 1.—East Entrance.
2.—Area, with Stairs.
3.—Kitchen.
4.—Back Kitchen.
5.—Servants' Bed-room
6.—Store-room.
7.—Matron's Sitting-room.
8.— „ Bed-room.
9.—Board Room.
10.—Dining Room.
11.—West Entrance.
12.—Library.
13.—House Surgeon's Sitting-room.
14.— „ Bed-room.

No. 15.—Pupils' Bed-room.
16.—Assistant House Surgeon's Bed-room.
17.— „ Sitting-room.
18.—Admission Room.
19.—Surgeon's Room.
20.—Dispensary.
21.—Patients' Waiting Room.
22.—Bath-room.
23, 25.—Bath Boiler-room.
24, 26.—Store-rooms.
27.—Court-yard.
32, 33.—Water-closets.
aa.—Grids.

Second and Third Floors above Rooms Nos. 25, 26, and part of 27.

28, 31.—Water-closets.
29.—Bath-room.
30.—Nurses'-room.

The Floors above 23, 24 and part of 27, are similarly divided.



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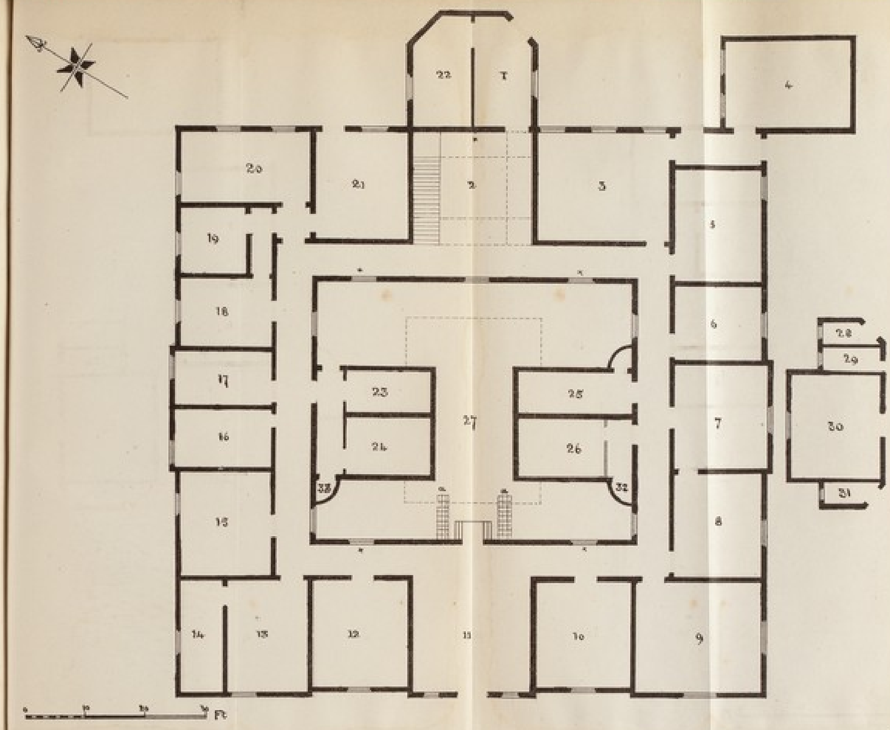

THE

ARTICLES AND PREPARATIONS

OF THE

BRITISH PHARMACOPŒIA,

POINTED

ACCORDING TO THEIR RELATIVE VALUES:

DESIGNED

TO AID IN THE STUDY OF THE MATERIA MEDICA.

BY

ALEXANDER HARVEY, M.D.,

PROFESSOR OF MATERIA MEDICA IN THE UNIVERSITY OF ABERDEEN;

AND

ALEXANDER DYCE DAVIDSON, M.D.,

ASSISTANT PROFESSOR.

ABERDEEN:

PRINTED BY ARTHUR KING AND COMPANY,

CLARK'S COURT, TOP OF BROAD STREET.

1871.

TO

E. A. PARKES, M.D., F.R.S.,

Fellow of the Royal College of Physicians, London, and Professor of Hygiene in the Army Medical School, Netley, &c. &c.

MY DEAR DR. PARKES,

ALTHOUGH these pages are printed solely for the use of my own pupils, I cannot forbear connecting your name with them. You have long taken a warm interest in the teaching of the *Materia Medica*, and striven earnestly to have it put on a better footing. You have just succeeded in having it separated from Therapeutics,—a subject indeed of the very highest importance, but quite beyond the reach of first or second year's students. This is a great point gained. Yet, as thus restricted, and now to be designated PHARMACY, much still remains to be done to make the teaching of it really efficient, and the study of it as satisfactory to students as is desirable.

You will, I know, give your hearty approval to much that I have here suggested. I shall be glad if I am fortunate enough to have an equally cordial approval of the *mode* devised for giving effect to the principle of *Selection*,—and of my idea that it is one of *general* application.

If so, I shall not despair of its receiving the sanction of the General Medical Council, and of seeing, ere long, the whole system of Medical Education and of Licensing, throughout the United Kingdom, conducted on the basis of carefully prepared official Catalogues of each department of the *Curriculum*,—catalogues of the highest value, comprising a volume well fitted, as a National work, to take its place side by side of the British Pharmacopœia.

Believe me, dear Dr. PARKES,

With sincere regard,

Very truly, yours,

ALEX. HARVEY.

UNIVERSITY OF ABERDEEN,
July 20, 1871.

"There are many heads, and I confess mine to be one of them, which no amount of labour would enable to carry so great a mass of details, at one time, as that which would be required to pass a rigid examination on all the subjects comprehended in the list."—Dr. Carpenter.

"A well-weighed scheme of professional education, sound and practicable, comprehensive yet moderate in its requirements, and adapted to all, besides the many good purposes it would serve, would have the special benefit of satisfying the minds of students themselves that at each step of their progress they are in the right path."—Dr. P. M. Latham.

INTRODUCTION.

The exceeding great number of *Articles* and *Preparations* that have a place in the Pharmacopœia, amounting in all to nearly 900, is, to most students, a sore let and hindrance in the study of the Materia Medica; while, moreover, the multifarious details they embrace are, to a great extent, held together in the mind, for reproduction afterwards at the examination-board, by a bond no firmer than casual or verbal association.

The difficulties thus attaching to that study would be greatly lessened, were students officially advised of the *relative values* of those articles and preparations. It would enable them to better purpose than they can at present, to apportion the time and attention to be devoted to each part of the subject, and, *par voie d'exclusion*, to avoid burdening the mind with what there is little or no use remembering, and the taking in of which seriously mars the storage there of what is important and essential.

It is with this view that this *Catalogue Raisonné* has been prepared. Of the primary articles of the Materia Medica, four degrees of value have been assumed, and these are indicated by four different forms of type. The Galenical preparations, again, have in a similar way had assigned to them a higher and a lower degree of value; and as these derive their value from the articles they represent, this two-fold subdivision seems sufficient. Yet when any, whether of the lower or higher degree, appears to have a special value, or on any account to deserve special consideration, this is indicated by an asterisk (*). The intention here is to bespeak for the preparation a corresponding measure of attention.

The writer is well aware that opinions will differ as to the estimates thus made by him. This is unavoidable. Every estimate of the sort must be more or less open to criticism. He may say, however, that in this matter he has not acted solely on his own judgment.

The writer, who has now had nearly twenty-five years' experience as a teacher and as an examiner, eleven of these as Professor of Materia Medica, would here take occasion to bring under the notice of all whom it may concern, and the General Medical Council in particular, another point bearing on this branch.

1. As far as regards the mere *recognition* and *naming* of the primary articles of the Materia Medica, he would make the range of acquirement co-extensive with the entire list. This amount of acquaintance with the subject is of easy attainment. Yet, in order thereto, students must have opportunities given them of again and again handling and inspecting specimens. The requisite knowledge is not to be acquired by looking at specimens on the Professor's table in the lecture-room—at the distance of several feet or yards. Nor yet by "walking" the Professor's museum. It can be gained only by placing specimens on tables at which students can sit comfortably, text-book, or, still better, Pharmacopœia, in hand, and carefully compare the specimens with the descriptions of them ("*Characters*") there given,—tasting and smelling, or otherwise making themselves familiar with each article. Even the choicest and costliest articles in the Museum may be made available to students, without risk of damage, by placing them in glass cases on a table,—under lock and key.

2. But as to all beyond this, the writer would make the whole system of school-teaching,—of private study,—and of pass-examination, to hinge on a principle of SELECTION. If the *entire* subject—indiscriminately—shall be insisted on as the *measure* of a candidate's knowledge of it, and as the *range* of the pass-examination, then, it is simply impossible for nine-tenths of the class to do more than acquire a mere smattering of it. Even as divorced from Therapeutics and comprising Pharmacy alone, the subject is too large for even the one or two foremost men in the class—(the prize men)—really to master. Demanding of candidates the impossible, they necessarily come far short of the possible.

Far better would it be for students to master well, thoroughly, *so much* of the subject. This done, the rest would follow afterwards as a matter of course. And, what is of capital importance, the pass-examination might then be pitched on a much higher scale than it can, in conscience, be at present. An examiner that vividly realises, as he ought, the fact—that his subject is virtually *illimitable*, will, as things are, make up his mind to rest content with *mediocriter* attainments in it, on the

part of candidates. How *few*, anywhere, pass with credit,—even with the lowest mark of credit. With the mass, everywhere, it is a bare pass,—with not a *few*, a squeeze through. Yet this need not be. It is simply the necessary result of the system presently in use—of sacrificing the possible to the impossible,—the attainable to the unattainable. A student's own work turns on the future pass-examination, and is regulated or influenced by the known character of this. As things are, everywhere, he learns that the standard, although professedly high, is really low; and that while he must strive to get up *something of every thing*, it will suffice if he give proof that he is not ignorant of what he shall chance to be examined on. The negative rather than the positive best expresses the qualifying standard!

But how practically work a system of teaching, of study, and of examination by *Selection?* In the manner indicated in these pages. There, besides indicating, by using different forms of type, the relative values of the several articles and preparations,—itself a great help viewed in relation to the existing "*Omnium*" system, the writer has placed a dagger (†) before certain of these. To these he would virtually *restrict* the class-teaching.* To these, he would advise students to give heed *first*, or, if they like, exclusively. And these *alone*, and no others, he would make the subjects of the pass-examination. *Subordinately*, the teacher, in the instruction he imparts,—the student, in the application he gives,—and the examiner, in the exactions he makes, would have regard, severally, to the *relative values* of the articles and preparations,—as set forth in the official or accepted programme or syllabus.

Sure he is, that were some such plan adopted, students would feel a load taken off their shoulders. And he is persuaded that on a plan like this, they would work with a will,—and work to good purpose. Idlers there would still continue to be. But their

* Of course one could not absolutely restrict a teacher. Yet independently of this, *passing allusions* might even advantageously be made by the teacher, in connection with articles selected, to articles excluded; while, by doing so he could obviate any exceptions he might be disposed personally to take to the official programme enjoined. Thus, *e.g.*, in speaking of *Jalapa*, selected, he might refer to *Podophyllum* and *Scammonium*, excluded; of *Buchu*, selected, to *Pareira* and *Uva Ursi*, excluded; of *Catechu*, selected, to *Kino*, excluded; and so on. But the license thus taken by the teacher, must be *absolutely denied* to the examiner. Otherwise, as regards candidates, the whole end and aim of the principle of selection would be defeated.

numbers would be greatly reduced,—because the due reward of idleness could then, and would be meted out *unsparingly*.

And what he here suggests as applicable to his own department, he would venture to suggest as applicable to all the departments of Medicine and Surgery. It is in the highest degree absurd,—and in its results mischievous, to examine students, as at present, on the *entire* field of each,—every one of which has reached dimensions that may well be called *Encyclopædic*. And he would rejoice were the General Medical Council to give its sanction to some such plan as is here sketched out, and to take steps for preparing, or having prepared, an Authoritative Directory of this kind, covering all the branches examined in for a Licence or Degree.

A. H.

*** The Estimates hereinafter given of the *Relative Values* of Articles and Preparations in the British Pharmacopœia,—are thus indicated:—

FIRST ORDER, **Second Order,** THIRD ORDER, *Fourth Order,* (the lowest,)	Of Value—of Primary Articles in B. P.
Higher Degree, *Lower Degree,*	Of Value—of Galenical Preparations,—an asterisk (*) being appended to certain of these, as explained in Introduction.

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†‡† Articles that have a dagger (†) prefixed, are those here suggested as *selected* articles, to which the teaching of the Materia Medica might be confined, and to which also the pass-examination should be rigidly restricted.

And of these, it is only those preparations that are printed in block, or, if in italics, that have an asterisk (*) appended, that are to go along with them in the teaching and examining.

MATERIA MEDICA.

(PHARMACY.)

INORGANIC DIVISION.

I.

†AQUA.	**Aqua Destillata.**
†CHLORUM.	*Liquor.* *Vapor.*
Calx Chlorata.	*Liquor.*
†Liquor Sodæ Chloratæ.	*Cataplasma.*
†BROMUM. **†POTASSII BROMIDUM.** **†AMMONII BROMIDUM.**	
†IODUM.	**Linimentum.** **Liquor.** **Tinctura.** *Unguentum.* *Vapor.*
†POTASSII IODIDUM.	*Unguentum.* *Linimentum c. Sapone.*
SULPHURIS IODIDUM.	*Unguentum.*
Plumbi Iodidum.	*Emplastrum.* **Unguentum.**

Cadmii Iodidum.	*Unguentum.**
†**FERRI IODIDUM.**	**Syrupus.** *Pilula.**
†**Sulphur Sublimatum.**	**Confectio.** **Unguentum.**
Sulphur Præcipitatum.	
†**Acidum Sulphurosum.**	
Potassa Sulphurata.	*Unguentum.*
†**Carbo Ligni.**	*Cataplasma.*
CARBO ANIMALIS PURIFICATUS.	
Phosphorus. } **Acidum Phosphoricum Dilutum.** }	

II.

†**Acidum Sulphuricum.**	**Acidum Dilutum.*** *Acidum Aromaticum.*
†**Acidum Hydrochloricum.**	**Acidum Dilutum.**
†**Acidum Nitricum.**	**Acidum Dilutum.** **Acid. Nitro-Hydrochloricum Dilut.***
ACIDUM ACETICUM GLACIALE. †ACIDUM ACETICUM.	*Acidum Dilutum.* *(Oxymel.)*
†**Acetum.**	
†**Acidum Tartaricum.** †**Acidum Citricum.**	

III.

Potassium.

POTASSA CAUSTICA.	*(Liquor.)*
†**Liquor Potassæ.**	
POTASSÆ CARBONAS.	
†**POTASSÆ BICARBONAS.**	**Liquor Effervescens.**
Potassæ Sulphas.	
†**POTASSÆ NITRAS.**	
†**POTASSÆ ACETAS.**	

†**POTASSÆ CHLORAS.**	*Trochisci.*
†**POTASSÆ TARTRAS ACIDA.**	
†**Potassæ Citras.**	
POTASSÆ TARTRAS.	

Sodium.

Soda Caustica.	
Liquor Sodæ.	
SODÆ CARBONAS.	*Exsiccata.**
†**SODÆ BICARBONAS.**	**Liquor Effervescens.** *Trochisci.*
SODII CHLORIDUM.	
†**Borax.**	**Mel.** **Glycerinum.**
Sodæ Sulphas.	
SODÆ PHOSPHAS.	
†**SODA TARTARATA.**	
SODÆ CITRO-TARTRAS EFFERVESCENS.	

Lithium.

†**Lithiæ Carbonas.**	*Liquor Effervescens.**
†**Lithiæ Citras.**	

Ammonia.

LIQUOR AMMONIÆ FORTIOR.	*(Liquor.)* **Linimentum.** *(Spiritus Fœtidus.)*
†**LIQUOR AMMONIÆ.**	
†**AMMONIÆ CARBONAS.**	**Spiritus Aromaticus.**
†**AMMONII CHLORIDUM.**	
†**AMMONIÆ ACETATIS LIQUOR.**	
AMMONIÆ BENZOAS.	
AMMONIÆ PHOSPHAS.	

Calcium.

Calx.	
†CALCIS HYDRAS.	**Liquor.** **Liquor Saccharatus.** **Linimentum.**
†**Creta Præparata.**	**Mistura.** **Pulvis Aromaticus.**
CALCIS CARBONAS PRÆCIPITATA.	
CALCII CHLORIDUM.	
CALCIS PHOSPHAS.	**Pulvis Antimonialis.**

Magnesium.

†**MAGNESIA.** (*Ponderosa.*)	
†**MAGNESIA LEVIS.**	(*Pulv. Rhei. Comp.*)
†**MAGNESIÆ CARBONAS.** (*Ponderosa.*)	
†**MAGNESIÆ CARBONAS LEVIS.**	**Liquor.**
†**MAGNESIÆ SULPHAS.**	*Enema.*

Aluminum.

†**Alumen.**	*Exsiccatum.**

Cerium.

†**Cerii Oxalas.**

Manganesium.

†POTASSÆ PERMANGANAS.	**Liquor.**

Ferrum.

FERRUM.	
†**Ferrum Redactum.**	*Trochisci.*
Ferri Peroxydum Hydratum.	**Emplastrum.**
Ferri Peroxidum Humidum.	
Ferri Oxidum Magneticum.	
†**Liquor Ferri Perchloridi Fortior.**	*Liquor.**
	Tinctura.*
†**Liquor Ferri Pernitratis.**	
†**FERRI SULPHAS.**	*Exsiccata.**
	Pil. Aloes et Ferri.
FERRI SULPHAS GRANULATA.	
LIQUOR FERRI PERSULPHATIS.	
†**FERRI CARBONAS SACCHARATA.**	**Mistura.**
	*Pilula.**
†FERRI PHOSPHAS.	**Syrupus.**
†FERRUM TARTARATUM.	**Vinum Ferri.**
†**Ferri et Ammoniæ Citras.**	*Vinum.*
†**FERRI ET QUINIÆ CITRAS.**	
TINCTURA FERRI ACETATIS.	
MISTURA FERRI AROMATICA.	

Cuprum.

†**CUPRI SULPHAS.**

Zincum.

†**ZINCI OXYDUM.**	**Unguentum.**
†ZINCI CHLORIDUM.	*Liquor.**
ZINCI CARBONAS.	
ZINCI ACETAS.	
†**ZINCI SULPHAS.**	
†**Zinci Valerianas.**	

Bismuthum.

†**BISMUTHI CARBONAS.**	
†**BISMUTHI SUBNITRAS.**	*Trochisci.*
†**LIQUOR BISMUTHI ET AMMONIÆ CITRATIS.**	

Plumbum.

PLUMBI OXIDUM.	*Emplastrum.**
Plumbi Carbonas.	*Unguentum.*
†**PLUMBI ACETAS.**	**Unguentum.**
	*Suppositoria Comp.**
	Pilula c. Opio.
†**Liquor Plumbi Subacetatis.**	**Liquor Dilutus.**
	Unguentum Comp.

Antimonium.

Antimonium Nigrum.	
†ANTIMONIUM SULPHURATUM.	(*Pil. Hyd. Subchlor. Comp.*)
†ANTIMONII OXIDUM.	**Pulvis Antimonialis.**
†**ANTIMONIUM TARTARATUM.**	**Vinum.**
	*Unguentum.**

Arsenicum.

†*Acidum Arseniosum.*	**Liquor Arsenicalis.**
	Liquor Arsenici Hydrochloricus.
†**SODÆ ARSENIAS.**	**Liquor.**
FERRI ARSENIAS.	

Hydrargyrum.

†**HYDRARGYRUM.**	**Hydrargyrum c. Creta.**
	Pilula.
	Emplastrum.
	Emplast. Ammon. c. Hydrarg.
	Linimentum.
	Suppositoria.

†HYDRARGYRUM.—*(Continued.)*	Unguentum.
	Unguentum Comp.
†Hydrargyri Oxidum Rubrum.	Unguentum.
Hydrargyri Iodidum Viride.	
†Hydrargyri Iodidum Rubrum.	Unguentum.*
†HYDRARGYRI SUBCHLORIDUM.	Pilula Comp.
	*Unguentum.**
	Lotio Nigra.
†HYDRARGYRI PERCHLORIDUM.	Liquor.
	Lotio Flava.
HYDRARGYRUM AMMONIATUM.	*Unguentum.**
†Liquor Hydrarg. Nitratus Acidus.	Unguentum.

Argentum.

Argenti Oxidum.	
†ARGENTI NITRAS.	

ORGANIC DIVISION.

VEGETABLE KINGDOM.

EXOGENÆ.

Ranunculaceæ.

†ACONITI FOLIA.	*Extractum.*
†ACONITI RADIX.	Tinctura.*
	Linimentum.
†Aconitia.	Unguentum.
PODOPHYLLI RADIX.	*Pulvis.*
PODOPHYLLI RESINA.	Pulvis.

Magnoliaceæ.

Illicium Anisatum.	*(Oleum Anisi.)*

Menispermaceæ.

Pareiræ Radix.	Decoctum.
	Extractum.
	Extractum Liquidum.
†CALUMBÆ RADIX.	*Pulvis.*
	Extractum.
	Infusum.*
	Tinctura.

Papaveraceæ.

Rhœados Petala.	*Syrupus.*
PAPAVERIS CAPSULÆ.	*Decoctum.*
	Extractum.
	Syrupus.*

†OPIUM.	Pulvis. *Pulvis Comp.* *Confectio.* *Emplastrum.* Enema. Extractum. *Extractum Liquidum.* Linimentum. Pil. Saponis Comp. Pil. Plumbi c. Opio. Pulv. Cretæ Aromat. c. Opio. Pulv. Ipecacuanhæ Comp. Pulv. Kino Comp. Tinctura.* Tinct. Camphoræ Comp.* *Tinct. Ammoniata.* *Trochisci.* *Unguentum Gallæ c. Opio.* Vinum.
†MORPHIÆ HYDROCHLORAS.	Liquor.* Suppositoria. *Trochisci.* *Trochisci Morphiæ et Ipecac.**
Morphiæ Acetas.	*Liquor.*

Cruciferæ.

†SINAPIS.	Pulvis. Cataplasma.
†SINAPIS OLEUM.	*Linimentum Comp.*
Armoraciæ Radix.	*Spiritus Comp.*

Polygalaceæ.

†SENEGÆ RADIX.	Infusum.* Tinctura.

Krameriaceæ.

†KRAMERIÆ RADIX.	*Pulvis.* *Extractum.* Infusum. Tinctura.*

Malvaceæ.

†GOSSYPIUM.	
PYROXYLIN.	
†COLLODIUM.	Collodium Flexile.

Byttneriaceæ.

THEOBROMÆ OLEUM.	

Aurantiaceæ.

AURANTII CORTEX.	*Infusum.* *Infusum Comp.* Syrupus. Tinctura. *Vinum.*
AURANTII FLORIS AQUA.	*Syrupus.**
LIMONIS CORTEX.	*Tinctura.** Syrupus.
†LIMONIS SUCCUS.	Syrupus.
Limonis Oleum.	
Belæ Fructus.	Extractum Liquidum.

Guttiferæ.

†CAMBOGIA.	Pilula Comp.

Canellaceæ.

Canellæ Albæ Cortex.	(*Vinum Rhei.*)

Vitaceæ.

Uvæ.	

Linaceæ.

LINI SEMINA.	*Infusum.*
†**Lini Farina.**	Cataplasma.
Lini Oleum.	

Zygophyllaceæ.

GUAIACI LIGNUM.	(*Decoctum Sarsæ Comp.*)
†**Guaiaci Resina.**	Pulvis. *Mistura.* Tinct. Ammoniata.*

Rutaceæ.

RUTÆ OLEUM.	
†**Buchu Folia.**	**Infusum.***
	Tinctura.*
Cuspariæ Cortex.	*Infusum.*

Simarubaceæ.

†**QUASSIÆ LIGNUM.**	*Extractum.*
	Infusum.* (Cup.)
	Tinctura.

Rhamnaceæ.

Rhamni Succus.	*Syrupus.*

Anacardiaceæ.

Mastiche.	

Amyridaceæ.

†**MYRRHA.**	*Pulvis.*
	Tinctura.
	Pil. Aloes et Myrrhæ.
Elemi.	*Unguentum.*

Leguminosæ.

Balsamum Peruvianum.	
BALSAMUM TOLUTANUM.	**Syrupus.**
	Tinctura.
Pterocarpi Lignum.	
KINO.	*Pulvis.*
	Pulvis Comp.*
	Tinctura.
†**SCOPARII CACUMINA.**	**Decoctum.**
	Succus.*
Glycyrrhizæ Radix.	*Pulvis.**
	Extractum.
TRAGACANTHA.	*Mucilago.*
	*Pulvis Comp.**

†**PHYSOSTIGMATIS FABA.**	*Pulvis.*
	Extractum.*
†**SENNA ALEXANDRINA.** }	**Confectio.***
†**SENNA INDICA.** }	**Infusum.**
	Mistura Comp.*
	Tinctura.
	Syrupus.*
Cassiæ Pulpa.	(*Confect. Sennæ.*)
†**HÆMATOXYLI LIGNUM.**	**Decoctum.***
	Extractum.
Tamarindus.	(*Confect. Sennæ.*)
†**COPAIBA.**	
†**Copaibæ Oleum.**	
†ACACIÆ GUMMI.	*Mucilago.**

Rosaceæ.

Amygdala Amara.	(*Oleum.*)
Amygdala Dulcis.	*Pulvis Comp.**
	Mistura.
Amygdalæ Oleum.	
Amygdalæ Oleum Amaræ. (Not officinal.)	
†**ACIDUM HYDROCYANICUM DILUTUM.**	**Vapor.**
Prunum.	(*Confect. Sennæ.*)
LAURO-CERASI FOLIA.	**Aqua.***
Rosæ Caninæ Fructus.	*Confectio.*
†ROSÆ GALLICÆ PETALA.	**Confectio.**
	Infusum Acidum.
	Syrupus.
Rosæ Centifoliæ Petala.	**Aqua.***
†**Cusso.**	**Infusum.**

Myrtaceæ.

†CARYOPHYLLUM.	*Infusum.*
†CARYOPHYLLI OLEUM.	
Pimenta.	*Aqua.**
Pimentæ Oleum.	

†CAJUPUTI OLEUM.	*Spiritus.*
GRANATI RADICIS CORTEX.	*Decoctum.*

Cucurbitaceæ.

†COLOCYNTHIDIS PULPA.	*Pulvis.* **Extractum Comp.** **Pilula Comp.*** **Pil. Colocynth. et Hyoscyami.**
†ELATERIUM.	

Umbelliferæ.

Carui Fructus.	*Aqua.*
CARUI OLEUM.	
†**Anisi Oleum.**	*Essentia.*
Fœniculi Fructus.	*Aqua.*
†**Anethi Fructus.**	*Aqua.**
Anethi Oleum.	
CORIANDRI FRUCTUS.	
CORIANDRI OLEUM.	
†ASSAFŒTIDA.	**Enema.** *Tinctura.* *Spiritus Ammoniæ Fœtidus.* **Pilula Comp.*** **Pil. Aloes et Assafœtidæ.**
†**Ammoniacum.**	**Mistura.** *Emplast. c. Hydrargyro.*
GALBANUM.	*Emplastrum.*
†CONII FOLIA.	**Succus.*** *Extractum.** *Vapor.** *Pilula Comp.** *Cataplasma.*
†**Conii Fructus.**	**Tinctura.**
SUMBUL RADIX.	*Tinctura.*

Caprifoliaceæ.

Sambuci Flores.	*Aqua.*

Cinchonaceæ.

†IPECACUANHA.	**Pulvis.*** **Pulvis Comp.** **Vinum.*** *Trochisi.* **Pilula c. Scilla.**
†CINCHONÆ FLAVÆ CORTEX.	*Pulvis.* **Decoctum.*** **Extractum Liquidum.*** *Infusum.* **Tinctura.***
†**Cinchonæ Pallidæ Cortex.**	*Pulvis.* **Tinctura Composita.***
†**Cinchonæ Rubræ Cortex.**	*Pulvis.*
†QUINIÆ SULPHAS.	**Pilula.** **Tinctura.** **Vinum.**
Cinchoniæ Sulphas. **Quinidinæ Sulphas.** **Cinchonidinæ Sulphas.**	Not yet officinal, but of great value.
†CATECHU PALLIDUM.	*Pulvis.* **Pulvis Comp.** *Infusum.* **Tinctura.*** *Trochisci.**

Valerianaceæ.

Valerianæ Radix.	*Infusum.* *Tinctura.** *Tinctura Ammoniata.**
†**Zinci Valerianas.**	

Compositæ.

Santonica. †SANTONINUM.	
†**Anthemidis Flores.**	**Infusum.** **Extractum.**

Anthemidis Oleum.	(*Extractum.*)
†**Taraxaci Radix.**	**Succus.** *Extractum.** **Decoctum.**
ARNICÆ RADIX.	**Tinctura.**
Pyrethri Radix.	*Tinctura.*
Lactuca.	*Extractum.*

Lobeliaceæ.

†LOBELIA.	**Tinctura.*** **Tinctura Ætherea.**

Styraceæ.

Benzoinum.	*Tinctura Comp.**
ACIDUM BENZOICUM.	*Ammoniæ Benzoas.*
Styrax Præparatus.	(*Tinct. Benz. Comp.*)

Ericaceæ.

Uvæ Ursi Folia.	**Infusum.**

Oleaceæ.

OLIVÆ OLEUM.	
†SAPO DURUS.	*Pulvis.** **Linimentum.** *Emplastrum.* *Emplast. Cerati Sap.* (*Pil. Sapon. Comp.*)
Sapo Mollis.	
†GLYCERINUM.	**Glycn. Acidi Carbolici.** **Glycn. Acidi Gallici.** **Glycn. Acidi Tannici.** **Glycn. Amyli.** **Glycn. Boracis.**
MANNA.	

Asclepiadaceæ.

Hemidesmi Radix.	*Syrupus.*

Loganiaceæ.

†NUX VOMICA.	**Extractum.*** **Tinctura.***
†STRYCHNIA.	**Liquor.***

Gentianaceæ.

†GENTIANÆ RADIX.	**Extractum.** **Infusum Comp.*** *Mistura.* **Tinctura Comp.***
CHIRATA.	**Infusum.** **Tinctura.**

Convolvulaceæ.

SCAMMONIÆ RADIX.	(*Resin.*)
Scammonium.	*Pulvis.* **Pulvis Comp.*** *Confectio.*
Scammoniæ Resina.	*Pulvis.* **Mistura.**
†JALAPA.	**Pulvis.** **Pulvis Comp.*** **Tinctura.*** *Extractum.*
†**Jalapæ Resina.**	(*Jalapine.*)

Solanaceæ.

CAPSICI FRUCTUS.	*Pulvis.* **Tinctura.**
Dulcamara.	*Infusum.*

Atropaceæ.

†BELLADONNÆ FOLIA.	**Tinctura.** **Extractum.** **Emplastrum.** **Unguentum.**

†**BELLADONNÆ RADIX.**	(*Atropia.*)
	Linimentum.
Atropia.	*Liquor.*
	Unguentum.
†**ATROPIÆ SULPHAS.**	**Liquor.***
Stramonii Folia.	
STRAMONII SEMINA.	*Extractum.*
	Tinctura.
†**Hyoscyami Folia.**	**Extractum.**
	*Tinctura.**
Tabaci Folia.	*Enema.*

Scrophulariaceæ.

†**DIGITALIS FOLIA.**	**Pulvis.***
	Infusum.*
	Tinctura.*
Digitalinum.	

Labiatæ.

Lavandulæ Oleum.	*Spiritus.*
	Tinctura Comp.
†**MENTHÆ PIPERITÆ OLEUM.**	**Aqua.***
	Essentia.
	Spiritus.
Menthæ Viridis Oleum.	*Aqua.*
ROSMARINI OLEUM.	*Spiritus.**

Polygonaceæ.

†**RHEI RADIX.**	**Pulvis.**
	Pulvis Comp.*
	Pilula Comp.*
	Extractum.
	Infusum.
	Tinctura.
	Syrupus.
	Vinum.

Lauraceæ.

Sassafras Radix.	(*Decoct. Sarsæ Comp.*)
†**Camphora.**	*Pulvis.*
	Aqua.
	*Spiritus.**
	Linimentum.
	Linimentum Comp.
	(*Tinctura Comp.*)
CINNAMOMI CORTEX.	*Pulvis.*
	Pulvis Comp.
	*Aqua.**
	*Tinctura.**
CINNAMOMI OLEUM.	
NECTANDRÆ CORTEX. }	
Beberiæ Sulphas. }	

Myristicaceæ.

MYRISTICA.	*Pulvis.*
MYRISTICÆ OLEUM EXPRESSUM.	
MYRISTICÆ OLEUM.	*Spiritus.**

Thymelaceæ.

MEZEREI CORTEX.	*Extractum Æthereum.**

Aristolochiaceæ.

†**Serpentariæ Radix.**	*Infusum.**
	*Tinctura.**

Euphorbiaceæ.

Cascarillæ Cortex.	*Pulvis.*
	*Infusum.**
	Tinctura.
†**CROTONIS OLEUM.**	**Linimentum.**
†**RICINI OLEUM.**	
†**Kamala.**	(*Tinctura.*)

Urticaceæ.

LUPULUS.	*(Lupuline.)* *Extractum.* *Infusum.* *Tinctura.*
†CANNABIS INDICA.	**Extractum.** **Tinctura.***

Artocarpaceæ.

Mori Succus.	*Syrupus.*
Ficus.	*(Confect. Sennæ.)*

Ulmaceæ.

Ulmi Cortex.	*Decoctum.*

Piperaceæ.

Piper Nigrum.	*Pulvis.* **Confectio.**
†Cubeba.	**Pulvis.*** *Tinctura.*
Cubebæ Oleum.	
MATICÆ FOLIA.	*Infusum.*

Cupuliferæ.

QUERCUS CORTEX.	*Decoctum.**
†Galla.	*Pulvis.** *Tinctura.* *Unguentum.** **Unguent. c. Opio.**
†ACIDUM TANNICUM.	**Glycerinum.** *Suppositoria.* *Trochisci.**
†ACIDUM GALLICUM.	**Glycerinum.**

Coniferæ.

†TEREBINTHINÆ OLEUM.	**Confectio.** **Enema.** **Linimentum.** *Liniment. Aceticum.* *Unguentum.**
†RESINA.	*Emplastrum.** **Unguentum.**
TEREBINTHINA CANADENSIS.	
Thus Americanum.	
PIX BURGUNDICA.	*Emplastrum.**
†Pix Liquida.	*Unguentum.**
JUNIPERI OLEUM.	*Spiritus.**
†Sabinæ Cacumina.	**Tinctura.** *Unguentum.**
Sabinæ Oleum.	

ENDOGENÆ.

Smilaceæ.

†Sarsæ Radix.	*Decoctum.** **Decoctum Comp.** *Extractum Liquidum.**

Zingiberaceæ.

†ZINGIBER.	**Pulvis.** **Syrupus.** **Tinctura.** *Tinctura Fortior.*
CARDAMOMUM.	**Tinctura Comp.***

Iridaceæ.

Crocus.	*Tinctura.*

Liliaceæ.

†**ALOE BARBADENSIS.**	*Pulvis.** **Extractum.** **Pilula.** **Pil. Al. et Ferri.***
†**ALOE SOCOTRINA.**	*Pulvis.** **Extractum.** **Pilula.** **Pil. Al. et Assafœtidæ.*** **Pil. Al. et Myrrhæ.** **Decoctum Comp.*** *Tinctura.** *Vinum.** *Enema (B. or S.)*
†**SCILLA.**	*Pulvis.** **Pilula Comp.*** **Syrupus.** **Tinctura.** *Oxymel.** *Acetum.*

Melanthaceæ.

†**COLCHICI CORMUS.**	*Pulvis.* **Vinum.*** **Extractum.** *Extractum Aceticum.**
†**COLCHICI SEMEN.**	**Tinctura.***
SABADILLA. } **Veratria.** }	*Unguentum.*
Veratri Viridis Radix.	*Tinctura.*

Graminaceæ.

Tritici Farina.	
Panis Mica.	
†*Amylum.*	*Pulvis.* *Mucilago.** **Glycerinum**

Hordeum Decorticatum.	*Decoctum.*
†**ERGOTA.**	**Pulvis.*** **Extractum Liquidum.*** *Infusum.** **Tinctura.**
Saccharum Purificatum. *Theriaca.*	*Syrupus.*

ACOTYLEDONES.

Filices.

†**FILIX MAS.**	*Pulvis.* **Extractum Liquidum.***

Lichenes.

CETRARIA.	*Decoctum.*

ANIMAL KINGDOM.

MAMMALIA.

Rodentia.

CASTOREUM.	*Tinctura.*

Ruminantia.

MOSCHUS.
SEVUM PRÆPARATUM.
SACCHARUM LACTIS.
FEL BOVINUM PURIFICATUM.

Pachydermata.

†ADEPS PRÆPARATUS.	**Adeps Benzoatus.*** **Unguentum Simplex.**

Cetacea.

†CETACEUM.	**Unguentum.**

AVES.

Ovi Albumen. / *Ovi Vitellus.*	

PISCES.

†OLEUM MORRHUÆ.	

INSECTA.

Hymenoptera.

Mel.	*Mel Depuratum.* *Oxymel.**
†CERA ALBA. *Cera Flava.*	Unguentum Simplex.

Hemiptera.

Coccus.	*Tinctura.*

Coleoptera.

†CANTHARIS.	*Acetum.* Charta Epispastica.* Emplastrum. *Emplast. Calefaciens.* Liquor Epispasticus.* Tinctura. Unguentum.

ANNELIDA.

†HIRUDO.	

ALCOHOLIC DIVISION, &c.

Alcohol. †SPIRITUS RECTIFICATUS. †SPIRITUS TENUIOR.	*(Spirit. Tenuior.)*
Spiritus Vini Gallici.	*Mistura.**
Vinum Xericum.	*(Vina Pharmacop.)*
VINUM AURANTII.	Vinum Ferri Citratis. Vinum Quiniæ.
†ÆTHER.	*Spiritus.**
†Spiritus Ætheris Nitrosi.	
†CHLOROFORMUM.	Spiritus. Tinctura Comp. Linimentum.
†CHLORAL. (Not officinal.)	Hydrate.
Cerevisiæ Fermentum.	*Cataplasma.*
†CREASOTUM.	*Mistura.** *Unguentum.* *Vapor.**
†ACIDUM CARBOLICUM.	Glycerinum.

*** In view of the object aimed at,—namely, the acquiring within the short time that can be given to it, of a sufficient and really satisfactory knowledge of PHARMACY,—the Selection here made seems still too large. Being a first attempt of the sort, however, we hesitated carrying the reduction further, lest we should prejudice the principle of *Selection*, or, as designated by the Medical Council, the "definition of the areas of Instruction and of Examination." But if, as in this University, the Teacher be also an Examiner, an understanding can easily be come to (as will be done in this Medical School), between him and his pupils, whereby the labours of the latter may be still further lightened,—or, rather, by the *area* of effort being still further narrowed, they may be made proportionally more productive of the "kindly fruits" of this branch of their professional education.

A. H.
A. D. D.

WHAT TO WEAR IN INDIA.*

THE RATIONALE OF HEAT APPLIED TO CLOTHING.

By C. J. F. McDOWALL, Assist. Surgeon Bombay Army.

[This little essay, when it first appeared in the public Press, was so flatteringly quoted in Bengal and Madras that I am induced to think it may not prove altogether uninteresting or useless in a more permanent form.]

THICKNESS and density, colour and material, tightness or looseness, form, weight, &c. &c.—these are some of the most important points for consideration by all residents in India, who wish to clothe themselves in accordance with the laws of health. The most extraordinary diversity of opinion prevails, among all classes, on the subject. Lord Clive is known to have ordered dozens of "shirts, the finest that could be made." One gentleman wears flannel all the year round, another silk. "Give me a well "*padded dark* coat, and I'll fear no sun," cries one sportsman; "no padding, but the *right colour*," declares another. Some ladies praise silk and loudly defend the "genre bouffé," and *gored skirt*; others are faithful to muslins and moderate crinoline. All are free to choose for and please themselves, (even the sepoy, to some extent) save one!—he, whose friend we all ought to be, and, I hope, we all are, viz., the British soldier! He has had his lot much ameliorated by the spirited and enlightened management of the higher authorities of the present day, and it is amongst the chief rewards of the writer to think that modifications, proposed by him (long ago) by drawings and writings, have contributed in some degree to the present improved style of uniform in India. There is still room for amelioration however, and amongst the most important considerations with regard to health, judicious philosophical clothing still holds a prominent place.

On the subject of hats opinions are still more singular than on that of dress. Some will pile fold upon fold of cotton or silk without being solicitous to shade their eyes (which are the closest *direct* continuations of the brain). Others care for their eyes only, and leave the upper part of the spine at the mercy of the sun, neither caring about colour, weight, or ventilation. It may be well to state here that we have rea-

* The physical data referred to in this paper are found in "Pouillet's Physique, Abel and Bloxam, Fownes, Golding Bird, and Professor Wilson's chemical works, or any manual of natural science.

son to believe the eyes to be less protected than the spine. Most of the symptoms of uneasiness about the temples are more or less traceable to the effects of *light* and heat on the optic nerves, and it remains to be proved that light has not much to do with some cases of insolation or *sun* stroke.*

It is absolutely necessary to investigate some points in the natural philosophy of heat before anything definite can be accomplished.

Those sources of heat which we have to investigate for the object in question are, probably, expressed pretty correctly as follows: the sun, the blood and body generally (animal vitality, whether chemical combustion, electric, or otherwise), friction or pressure.† The actions and modes of behaviour of these are so numerous and complex with regard to different bodies and to different states of the same bodies, some being constant and others continually varying, that it becomes a very difficult matter indeed to prescribe rules for maintaining the blood at the proper temperature (98 deg.), and preventing its rising to the *few degrees above* this, which constitute inconvenience and danger. Like most subjects of domestic economy, involving some knowledge of physical science, this has been treated cavalierly in proportion to its difficulty. The following is an attempt to insist upon facts well known but little applied.

We shall endeavour to be as simple and plain as possible. Caloric or heat has the following modes of action. It is radiated outwards in all directions from its source. It is reflected back (from white polished surfaces especially), it can be transmitted, to a certain extent, conducted, absorbed, and lastly it has the power (whilst producing evaporisation or liquefaction), the *most important* power of becoming insensible, unfelt, harmless, *latent*, as it is scientifically expressed. This wonderful provision it is which alone enables us to bear temperatures which would otherwise destroy us.‡

Heat produces in its turn certain effects, viz., expansion, liquefaction, evaporation, and lastly combustion.

In attempting to adapt clothing to our knowledge of the philosophy of heat, we have therefore to strive by every means in our power to guard the body from the absorption of heat, whether direct, reflected, conducted, radiated, &c., &c., to favour the access of cold and dry portions and the evection of hot and moist air, and, when this is not possible, to reduce the temperature of the body (which would otherwise rise to a dangerous height) by favouring the access of dry air alone with a view to the

* I would venture to point out that there are two distinct classes of cases in this disease, heat stroke and *light* stroke *with* heat.

In the Parliamentary sanitary and medical reports for 186[illegible] will be seen the great frequency of disease of the Retina and interior of the eye generally produced in India and Malta. The ophthalmoscopue demonstrates them so as to leave no doubt whatever of their existence. Detachments, opacity, pigmentary deposits, wasting consequent on inflamation, ulceration, &c. &c.

† Terrestrial heat as appreciated in mines, artesian wells, &c. does not come within the limits of the subject.

‡ It is thus that extraordinary degrees of heat have been borne for a short time by experimentalisers, in hot ovens, hot air, baths, &c. much above 2 (two) hundred degrees, (see Watson.) None suffered from coming suddenly into the cold air again.

evolution of those conditions in which heat becomes insensible, latent, harmless. In other words, to favour *evaporation*, to see that the clothing is not of a nature to *interfere* with evaporation, during the prevalence of the hot weather. Also we must avoid irritative friction.

All the experienced Indian physicians of the day have recognised, without cavil or dispute, that after Malaria, heat is the bane of the tropics (Martin, Morehead, Leith, &c.,) Cold water, for those who can bear it, both as drink and for external use, is universally recognised as a panacea. It has been pointed out with extreme acuteness that the too frequent change of linen—three or four times a day—tends materially to increase the temperature by irritating the skin. It is of no use to attempt to *shut out* entirely the heat from the body, by clothing as you can from a house, by closing the doors and windows. For although absolutely opaque bodies do not transmit heat Sir J. Leslie, Franklin, Melloni, Stark, &c. &c.) They prevent evaporation, and getting heated themselves radiate caloric to the body, dark ones more than white. A house is shut up with advantage, in very hot weather, because enough cool dry air is collected to carry on the evaporation necessary for the one or more individuals who inhabit it.*

But what amount of cool air can be enclosed in a tight coat or hat? Certain modifications of this are rational enough however, viz. an opaque hat well ventilated, or a very loose cloak, which in Spain is much patronized, &c. &c.

The usual temperature of the blood is 98 deg. Now although in India the heat is not often or long continuously over or up to 98 deg. in the shade, still the body is frequently raised above that temperature. A variation of 4 deg. is scarcely compatible with health far less with comfort. Anything beyond this (save in some animals, as birds) produces disease and ultimately death. This seemingly extraordinary phenomenon of the body's heat rising above its natural standard (about 98 deg.) even in shade, and in air which may be less than 98 deg., is brought about, strange as it may appear, by improper clothing.† At this stage of our researches we must begin to perceive that the subject is no trifling one.

When clothing is thick and dense and tight (without considering its colour and other qualities), it keeps a layer of hot and moist air in constant contact with the skin. When muscular exertion, excessive combustion of the blood in the lungs, emotion, disease, &c, &c, increase the animal heat of the body, and cause perspiration, evaporation cannot take place; for in a moist atmosphere it is impeded,—the air is not sufficiently dry! Hence the known danger of very hot and very moist climates. Of course there are limits, many and various, to the effects or results produced by this state of things; the tendency however is always in the same direction—"Disease and ultimately death."

I am aware that it may be said that evaporation still goes on. This

* The use of Tatties (wet screens of fragrant roots from which evaporation take place, and causes heat to become latent) proves however the actual superiority of evaporators over all other means of cooling the body. They may be abused however. We must not sit too close to them.

† I rejoice to see that Dr. Peet (Principles and Practice of Medicine) leans to this doctrine that inefficiency of "refrigeration by the skin" actually raises the temperature of the blood and solid textures. Page 97.

is true; but where? At the outer surface of the clothing, and, the thicker the clothing may be, so is the cold produced less capable of being useful to the body. And in proportion as the space between the body and clothing is confined and small, the sooner does it become saturated and an impediment to evaporation! This is carried to excess when a tight hat presses on the forehead. It is positively unbearable. Again, when heat is excessive it may prevent or impede evaporation, by causing a too abundant and rapid excretion of perspiration, which cannot be evaporated *quickly* enough and in proportion. For the globules only evaporate at their surfaces. By bringing those surfaces nearer to the skin (as by removing the collected perspiration with the handkerchief) evaporation is brought nearer to the skin, and a sense of coolness is the result.

If the clothing be tight and close only at the extremities, about the situation of the wrists, neck, ankles, &c., the evection of hot and moist air and the substitution of dry (whether cold or not) is seriously impeded, and the evil is as great almost as if the whole were tight. Who will be astonished at the numerous cases of fatal syncope and congestion, the so-called sun-strokes occurring on the shady sides of streets, in America and elsewhere, these and a hundred more lingering and insidious, but equally formidable, shapes of disease and death!

Loose, light clothing, open at the extremities, by permitting the exit of moist, saturated air and entrance of dry (and by not causing the heat and fatigue of irritation and weight) is the best, as a rule, so far as regards some of the qualities mentioned at the head of this paper, viz., form, weight, and density (colour we will speak of later).

I proceed to a much more disputed subject: material; here is a well-known table showing the comparative conducting powers of the following substances:—

Linen as a conductor of heat. (very active.)

Raw Cotton	(less)
Do. Wool	(do.)
Do. Silk	(do.)
White Fur	(do.)

Linen by reason of its density of texture *remains wet.* West Indians declare it to be very grateful, however, and the objections to its feeling cold and wet are, or were, quite over-ruled.* But sudden changes of temperature alone are still believed to be dangerous, and by people too who take a prolonged shower bath of cold water every morning. Cotton or Flannel are however preferred and perhaps with great justice. But it is impossible to understand a preference

* Great changes of temperature are not always dangerous; but as absolute cold is not what we have to fear in the hot weather, we must avoid making the contrasts or changes of temperature great (relatively) by not *heating* ourselves in the sun rather than by loading the body with covering in the shade Again a sudden lowering of the heat of a body which has been already exhausted and *cooled down* is the danger to be avoided; a cold douche is excellent and *safe* after a hot bath in many cases. *Drinking cold draughts* of water however when *heated by exertion* and when we are *exhausted* is never safe. Near the stomach is what may be called *vital centre of organic nerves*; either heat or cold may give it a shock from which it cannot recover.

for *thick* woollen fabrics in the hot weather. When thick they interfere with evaporation and being rough, absorb the heat better and radiate more of it to the body than smoother textures, bad as they may be as mere conductors of heat. It is known that if two bodies, one rough the other polished, be equally hot, the rough one radiates more heat, as appreciated by a thermometer, in a given time than the smooth. Wool, moreover, sometimes greatly irritates the skin. A woollen texture must be very fine and light indeed to compete in the hot weather with our good cotton ones. The coarse, heavy, thick, tight jacket of our soldiers (to say nothing of its colour) is most dangerous in India, and indeed, always, so in the middle of the day. It is practically abandoned. Yet this same cloth, deadly as it is when made into a tight jacket, would be almost innocuous if the garment were made loose, without collar (or at least without a tight one),* and if it were unconfined by belts, &c. (which should be worn underneath, not over it). The soldier frequently throws away his jacket in India, at sieges and in batteries, &c. He has to pay for it, if lost, but prefers to fight in his shirt sleeves. A slight fall of temperature is constantly cited as a cause of disease when, in the evening, the soldier throws away his garment. He cools down and if he fall asleep he *does not* resume it. A sailor on shore after exhaustion cools himself with still greater indiscretion, for he will sometimes lie down and sleep all night in the gutter. It is not the sudden change but rather its accompanying cooling *from exhaustion* and *over exertion*, which is dangerous.† But, if the body have not been overheated, there will be neither sudden change nor cooling. Thick wollen cloth, if useful to a man who *feels cold*, (in the cold shade of evening) cannot be said to keep out the sun's heat to any advantage. It prevents cooling when he is warm. A stoker is subject to catch cold, for although, when he leaves the fire room, he invariably covers himself, he throws away his overcoat, as soon as he begins to cool.‡ It cannot reasonably be said that cotton textures feel wet to the skin, as linen ones do, but any objection on this head is avoided, by ingenious light manufactures of wool, or cotton with wool or silk combined. Such are common enough, but present no sufficient advantages, in hot weather, over cotton, as yet, to compensate for their extra density, and price, &c. &c.‡ However, as we have shown that evaporation is to be considered the great cooler of the body (with reflection to be noticed presently) the nature of any of these textures, whether good or bad conductors is entirely a secondary consideration to their being made loose and unconfined at the collar and wrists, &c. &c. Under these conditions flannel is perhaps the best material for a coat, in the cold weather especially. Sleep-

* This was written in 1858, and presented to the authorities with drawings. Whether in consequence of this or not, the suggestions, saving the belts, have been fully carried out.

† No man should bathe in cold water, if he feel chilly.

‡ Flannel (soft and light) is the best as its porous texture best favors equable evaporation. This is why Mr. Jefrey's found a low temperature of skin under it (Report of Sanitary Commission). Its low conducting power was not the only cause.

ing in blankets, whether cleanly or not, is soldier-like and commendable. They should be changed, washed, and well aired.

Some extra clothing at night also is often desirable at this season. The difference of temperature during the night and day is sometimes prodigious. Dews, however, are neither so heavy or common in India as in some parts of even European countries. The degree of dryness and expansion of what vapour there is (especially in the Deccan) is such that it requires a much lower temperature than usual to condense moisture. In short, the well known difference of the due point and the actual heat of the atmosphere is nearly twice as great as in England. The heat of the surface of the ground must, sometimes by radiation, be reduced 61 deg. before dew be produced. This expansion of vapour (dryness of atmosphere) is in itself a cause of rapid radiation of heat and of the great difference of temperature during day and night. These great changes are confined to the cold season, however, and do not require more than a properly made cloak for those who are exposed to their influence. In the hot weather it is quite inadmissible to wear anything of the sort.

I now come to the important consideration of colour as influencing the blood-temperature, health, and comfort of the wearer. It has long been known that polished and white substances reflect back much more heat than dark rough ones (and do not become so hot themselves, nor radiate so much, when heated)* I am setting aside for a time their conducting power. In Franklin's experiment snow was found to melt much sooner under a piece of black cloth than under one of white or of polished metal, which neither got so hot nor radiated so much heat to the snow. Likewise the fact of reflection of heat can be shown by placing a piece of burning charcoal in the focus of one parabolic mirror and some gunpowder or tinder in the focus of another. At no inconsiderable distance off these will catch fire by the sole agency of reflected heat, and the mirrors themselves remain cool. The following is a table drawn up by an able French experimentalist, (whose work I have not been able to procure), Dr. Coulier, published in the public journals. Sir Humphrey Davy and Stark proved the same by equally ingenious methods.†

Thermometer	not covered in the sun		37.5	centigrade.
Do.	covered with cotton shirting		35.1	,,
Do.	do.	do. lining	35.5	,,
Do.	do.	unbleached linen	39.6	,,
Do.	do.	dark blue cloth	42.0	,,
Do.	do.	red cloth	42.0	,,
Do.	do.	do. (a little finer)	41.4	,,

It was only with regard to direct solar heat that these results were attained, ordinary fire, heat, &c., having little effect. I remember well

* In the arctic regions, where the external heat is less that of the body, white wool would be the proper clothing from the same reasoning. Such is the case. The hares, foxes, bears are white.

† Davy placed some bees' wax behind plates of copper, painted in different colours. The rate at which the wax melted confirmed the above. Stark plunged thermometers covered with different coloured cloths into boiling water. Between white and black he found a difference of 25 per cent., and the following order :—Black, blue, green, red, yelow, white.

that when encamped on the shores of the Bosphorus, in Turkey, a little incident occurred which vividly impressed this physical fact on my memory and shoulders! The forenoon being bright and inviting, we (some other officers and myself) determined, Leander-like, to lave our limbs "in the classic waters of the East," though not precisely at the same spot that he patronized. The walk was pretty long and hot. We one by one took our coats off, and carried them on our arms, both on going and coming from the bath. I wore a cherry coloured flannel shirt, and although all our shoulders were more or less burnt by exposure, while in the water, I was literally scorched, and for some days suffered greatly from the slightest movement or friction of my coat. We all suffered in exact proportion to the depth of shade of our flannel. Now in the Jungle, in the hottest weather and in the sun, I have often ridden and do ride still both for experiment and comfort in my shirt sleeves; but that shirt is white. Any other color requires a proportioned thickness nay even some padding, according to many.

In providing for a protection for the head the same principles are applicable; but, as a hat can be, and is ventilated by a variety of contrivances, absolutely opaque substances, such as metals, felt, pith, &c., may be employed; evaporation takes place easily through the apertures of ventilation. Melloni and others have shown (chiefly by the use of the thermo-electric pile, an instrument which measures the most delicate variations of heat) how different are the interfering proportions of bodies with regard to the rays of caloric. Rock-crystal, glass, and ice, all transparent and diaphanous to light, transmit a widely different proportion of heat. The first is so pervious that it is called the glass of heat. Glass fire-screens are much used in England. As a rule bodies transmit, according as they approach to transparency or opacity. Opaque bodies are impervious, but their conducting powers may be great, and, if dark, they both absorb and radiate, as well as conduct very quickly the heat rays (or vibrations). As previously mentioned,—unless freely ventilated,—opaque coverings prevent evaporation, and we know that a rabbit may be easily killed by simply covering its skin with an opaque mixture. Hats should therefore be white to reflect off the sun's rays; of an opaque substance that does not easily become heated, lined with a colour (light) calculated not to radiate heat to the head, and very carefully ventilated.* Lined with a white cotton quilting inside, the metal helmets of our dragoons (apart their weight and want of ventilation) would by no means be inadmissible as head pieces for the field in India. Their polished surfaces would reflect off the heat, as proved on a former occasion. They should be of a light colour and not heavy. The metal aluminium (many times lighter than silver) and some others, or combinations of others, might be chosen. Their being lined with white padding would, at least partly, neutralize their disadvantage as conductors of heat. The felt, mill board, wicker, pith cane, cork, &c. &c. covered with white are decidedly the most safe and comfortable for temporary wear. They cannot however be part of the

* Some people object to ventilation of hats, as it dries the scalp? They undervalue the enormous cooling power of evaporation, and the danger of interfering with it.

permanent uniform of an army for all seasons and countries. For a soldier-like, useful and strong helmet or shako, white polished leather would seem to be strongly recommended by these two off-reflecting qualifications. Black varnished leather is largely employed on our shakos, &c.; why not white? The former colour absorbs heat, the latter does not!

We repeat however that all *these* hats must be ventilated. This is accomplished in various ways. There must always be shaded apertures at the top for the escape of heated, moist air (the turban of the Indian admits of this through its folds). There must also be access to dry air round the forehead below. A turban has these qualifications all through its extent, being by its very texture and folding a mass of minute perpendicular ventilating apertures, and somewhat larger, longitudinal ones; (they do not usually shade the eyes properly however). It is useless fancying moreover that folds of linen, piled up outside a hat, or cap, or helmet, which is opaque, can offer the equivalent of a turban. Anything like a tight opaque ring round the forehead should be avoided (a white changeable and washable strip of cloth, linen, or cotton or wool, is better than the leather band usually sewn inside our hats and caps, which would thus come nearer to the condition of a turban, with moreover a proper shade). For hats I have found four cushions, leaving apertures between them for air, very agreeable and efficient. They fit one on the forehead, one at each side, and one at the back, are made by the native tailors, and sewed inside a helmet which is larger than the head of the wearer. But there is no dearth of systems and efficient ones, for the purpose in view, and when fully carried out, they will rid us* all of that tight hot band which presses like a circlet of iron into our very brain, and is so often the handmaid of disease. A word on reflected and radiated heat will close this part of the subject. We all know how heat reflected from the surface of the water (the glare as it is called) burns the face of fishermen and fair anglers, who are on the sunny side of a pond or stream. The face, temples and occiput may be in the shade of a broad brimmed hat, and yet exposed to intense and dangerous heat reflected from the ground or water. Heat radiated or convected has much less influence. For the face, the Indian will sometimes tie a fold of his turban under his chin and over his mouth from temple to temple during hot diurnal marches. I have found a mask (white) very grateful in boating or even riding, in the hot season. I beg to recommend to my fair fellow exiles a white or pink satin mask trimmed with handsome lace as likely to preserve and increase their beauty, enable them to ride out and sail more during the day, and as by no means an inelegant piece of costume. The mouth and chin are not covered, so there is none of the close feeling complained of by those who wear veils. The mask is quite useless and superfluous in the shade, and so of course their charming faces will not always be obscured, even during a picnic. The "ugly" as worn at the sea side is simply the very worst

* Mr. Jefreys F. R. S. recommends most admirable models with a double and even treble roof. I have only this day seen his remarks in the Report of Sanitary commission.

contrivance that could be conceived for attaining the object in view. I take the opportunity of offering this prescription (an elegant light, not dark, half-mask) to all Queens, Empresses, and particularly to pretty princesses, and ladies who may be fond of sea-side watering places!

The temple and occiput must be protected by a white loose curtain, passing round the back of the head from the corner of one eye to the other. It must fall, almost perpendicularly, to be of any use. If it stick out like a wing, as that generally worn, the heat, reflected from the ground, passes under it directly to the occiput and temples. It should hang loosely, and so as to favour, by gentle movements, the shifting of moist heated and useless air. It need not then be so thick as would otherwise be requisite.

In conclusion I beg to impress on my readers that although I do not undervalue the dangers of sudden changes of temperature, especially when following serious gradual lowering of the body's heat (even such lowering changes as can be experienced in the tropics) I must more strenuously warn then against over heating, improper clothes, &c. Again, I am fully alive to the many advantages of woollen textures, even in the hot weather, if fine, light and "confined. But I must protest against tight clothing of any kind, thick, dense, heavy wool being then inferior to cotton. All these remarks apply of course only to stations and seasons, where and when *tropical* heat prevails. I beg my readers never to forget that evaporation is the antidote to heat. It is this which* is favoured by, and gives its repute to flannel. It is evaporation which alone enables us to take exercise and to live in the tropics at all!

* As I all along have kept the soldier's interests in view, I add that the knapsack invented by Colonel Carter is the best I have seen, and, when covered with white or made of white materials, must be nearly perfect. *

In elevated stations and after some years residence in India (when the power of generating heat is diminished) neither cold water nor light clothing will be borne, in many cases.

* Written before the publication of Parkes' great work on Hygiene.

"From the author."

ON THE

MODERN ASPECTS

OF

THERAPEUTICS.

BY

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1871.

ON THE

MODERN ASPECTS OF THERAPEUTICS.*

It must be allowed that the reproaches which have been so often levelled against the *practice* of medicine have had much foundation in the past history of therapeutics, and all will re-echo Sir T. Watson's opinion, that "certainly, the greatest gap in the science of medicine is to be found in its final and supreme stage—the stage of therapeutics." Some of its keenest satirists have been physicians of the highest eminence and most varied acquirements, and, on the whole, it must be admitted, that the improvement of therapeutics, contrary to the other arts and sciences, "bears no proportion to its antiquity." It is the consciousness of this disproportion which damps the spirits of so many in the profession, and which has led to so much distrust and doubt. Dr. Radcliffe used to say that "the whole art of physic, for which he had a profound contempt, might be written on one sheet of paper," and it is not so long since the late Sir W. Hamilton, of Edinburgh, asked the question:—"Has the *practice* of medicine made a single step since Hippocrates?" a revival of the older query, *an datur ars medicinæ?*

There are many evidences that the need for a more careful study of therapeutics is urgently felt by the body of the profession at large. In 1865, the Physiological sub-section of the Brit. Med. Assoc. drew up a memorial to the Gen. Med. Council praying the council "by pecuniary grants, and the appointment of suitable persons, to undertake investigations into the physiological action of medicines." This memorial was supported in the council by the Regius Professors of Physic in the Universities of Dublin and Oxford, but was negatived on the ground of want of powers in the council to comply with the petition.

* Thesis for the degree of M.D., 1870. Read before the Medical Society of the College of Physicians, March, 1871.

A sub-committee was then appointed by the Brit. Med. Assoc. and the results of its labours are seen in the elaborate report brought out by Dr. Hughes Bennett, on the action of mercury, podophyllin, and taraxacum on the biliary secretion. About the same time the Royal Med. Chir. Soc. intrusted the examination of the method of subcutaneous injection to a committee, and the valuable observations embodied in their report furnish the most satisfactory data which we possess respecting this method. Quite recently the Med. Psychol. Assoc. of Edinburgh, have appointed a committee for the purpose of taking into consideration, among other things, the medical treatment of insanity, and they suggest propositions for combined therapeutical investigation, and ask for special information on the action of chloral. The Clinical Society of London owes its establishment in 1868, to the expressed want of more real knowledge on the various remedies in daily use, and the appearance of numerous detached papers, and of some works of merit on the doctrines and requirements of therapeutics testify to the deep-seated interest which now attaches to the prosecution of this subject.

I propose, now, briefly to inquire what are the resources at our command, and how far it may be said that therapeutics has advanced within the last quarter of a century, what are the hindrances to its progress, and, more particularly, in what directions we may hope for still further and more solid advances than have yet been gained.*

To avoid entering upon too wide a field my observations will be chiefly confined to the domain of what may be called medicinal therapeutics, *i.e.*, of remedial agents as directly applied to the treatment of disease, and accordingly the steady progress and increased knowledge of sanitary science and preventive medicine, the splendid results of operative surgery, and the development of state medicine, will be passed over without comment.

The retrospect of the history of therapeutics for centuries past, is, in many respects, not encouraging, and one can scarcely help wishing that much, if not most, of what is called the accumulated experience of ages were swept clean out of remembrance, so overladen is it with confusion, mis-statements, and unproven theories. In fact since the prevailing ideas as to the action of drugs became in some degree fixed at a time when pathology was less exact than it is now, when there were no such accurate means of testing the

* For many suggestions I am especially indebted to, and have largely made use of Sir W. Jenner's admirable address on medicine, delivered last year, in Leeds, and Dr. Rogers' recent able work on therapeutics.

real effects of remedies, and when physics and chemistry were in their infancy, we cannot avoid insisting on the necessity for renewed observations, carried out under better auspices, and with a better directed aim.

Yet it will be conceded that the materia medica abounds in agents by means of which very remarkable effects can be produced on the human frame, and a speculative mind might engage itself in showing that the possession of such powers by various medicines is an argument in favour of our being intended to exercise a due control over the progress of disease. Even as it is we can, at will, exalt or depress the action of the heart, the great fountain of life, and can, to some extent, control the capillary circulation, we can compel the stomach to eject its contents, and the intestines to discharge their excreta. We have agents that act on special functions of the encephalon, on the spinal cord, on the sensitive nerves, and purely on the motor nerves. By suitable means we can increase or diminish the exhalation from the skin and mucous membranes, and can alter in quality and quantity the secretions of many important glandular organs. At pleasure we can contract or dilate the pupil of the eye, can stimulate striped and unstriped muscles, can poison some internal parasites with certainty, and can aid in the elimination of metallic poisons from the body. And, let it be observed, that not only have we these and other powerful means at our disposal, but that, in many, very many cases, we have the knowledge *how* to apply them to the treatment of disease with benefits which cannot be gainsaid, and in a few cases we know *why* we so apply them.

Our theories as to the nature of disease are undergoing a profound change, necessarily followed by corresponding modifications in the way in which we endeavour to meet or anticipate it. The notions of elimination, and allopathy, of antidotes, and of counter-irritation, have all their measure of truth, and are all usefully applied in practice, but it is to be hoped that none of them will ever again be raised to the rank of a system to cramp and fetter our ideas. As a positive and well-founded advance in the doctrines of therapeutics, it could easily be shown that certain injudicious or noxious lines of treatment have been abandoned, and that, in general, the habit of overdrugging has been given up. This beneficial change is due partly to a more accurate acquaintance with the local causes of disease, *e.g.*, the parasitic skin diseases, partly to a more intimate knowledge of the pathology of disease, *e.g.*, chronic pulmonary phthisis, and partly to a recognition of the principle that we are not to treat our patients as so many sponges doomed to soak up the maximum quantity of medicine possible, but, as living beings, whose functions are disordered by disease, and whom we seek to restore to health by aiding the natural tendency to recover, and by striving to modify the direction of action of the natural forces of the body. We know now that a large number of acute diseases occurring in previously healthy persons naturally run a definite course and tend to spontaneous recovery, in the absence of or even in spite of misdirected drugging, and we have recognized that certain acute diseases supposed to be of indefinite duration lie within appointed limits. We, therefore, by this advance in knowledge, avoid drawing false conclusions as to the efficacy of drugs in particular maladies, and although we do not pretend to be able to strangle acute disease by specifics, or suddenly arrest the cycle of morbid action, much still remains for our art in meeting special symptoms and controlling intercurrent complications. Sometimes advances in knowledge teach us a more correct appreciation of the composition and mode of action of drugs, or at least displace a faulty explanation. This certainly is a gain, and we know too little yet to see how far the application of the physical processes, dialysis, diffusion, and osmosis may before long enlighten some of the dark recesses of therapeutics.

Among the tributes levied from chemistry and natural history, we can reckon carbolic acid and its compounds, the alkaloids, the bromides, permanganate of potassium, sulphurous acid and the sulphites, the whole group of anæsthetics, chloroform, ether, bichloride of methylene, nitrous oxide, and nitrite of amyl, Calabar bean, glycerin, pepsin, santonin, podophyllum, and lastly chloral, and its allies bromal and iodal. The mention of the class of alkaloids suggests the thought that very great benefit would, doubtless, accrue from the more extended use of the alkaloids in the room of the crude vegetable products from which they are derived. Our therapeutical experience would be rendered infinitely more accurate by the employment of these definite active principles which are chemically stable, and whose dosage can be exactly proportioned, and the differences which are often asserted to exist between the active principle and the crude drug itself would doubtless be found to be much less considerable than is generally thought. In the case of belladonna and conium, for example, the efficacy of these drugs is fairly and fully represented by their respective alkaloids, and even in the case of a complex substance like opium which con-

tains several organic bases of different properties, it would be quite possible, after proper investigations, to combine these bases in a compound solution so as to represent perfectly the action of the crude opium. As illustrations of the confirmation and extension of the curative powers of single drugs we can adduce the mass of evidence that now exists as to the respective value of mercury and iodide of potassium in different stages of syphilis, and of mercury especially in infantile syphilis, of the utility of arsenic in the *relapsing* skin diseases, of bromide of potassium in epilepsy, and certain other abnormal conditions of the brain and sexual organs, of quinine in periodic diseases other than ague, and of ipecacuanha in dysentery. We are better acquainted with the action of digitalis, opium, belladonna, hyoscyamus, and conium, and there is a clearer understanding gaining ground as to the worth and indications for the employment of alcohol in the treatment of disease.

The uses of iodide of potassium have been brought into greater prominence and have been more sharply defined, and amongst the results "we may boast the disappearance of radesyge in Norway, of yaws in our West Indian colonies, and of most of the severe forms of tertiary syphilis at home." Since the more important of these drugs are of quite recent introduction, they are to be looked on as but an earnest of the harvest we are yet to reap from the domain of the natural sciences. Improved modes of administration are only second in importance, and hypodermic injection is an aid for which we cannot be too grateful, triumphing especially in the relief of painful and spasmodic affections. Lastly, a discrimination between the properties and uses of the direct and induced currents, *i.e.*, of galvanization and faradization, has led to most important and gratifying results in the treatment of such formidable diseases as epileptiform neuralgia, infantile paralysis, and progressive muscular atrophy. It is proved that it is possible and feasible to galvanize directly the brain and spinal cord, and the galvanic irritation of the sympathetic nerve may yet furnish us with a powerful lever for controlling the nutrition of even remote parts.

Many circumstances have contributed to clog the progress of therapeutics, some of which belong to the inherent difficulties of its investigations, while others, and that a large portion, are due to the ignorance and incompetence of those to whom we should look for aid. The fallacies connected with the application of the inductive method of reasoning to the science of medicine, and the sources of error in practical and theoretical medicine, have been well exposed by Sir G. Blane and by Dr. Barclay, and I would merely remark that the principles enunciated by these authors, while they are the philosophical basis of the practice of physic, constitute the best answer to morbid scepticism on the one hand and vulgar credulity on the other.

Faulty modes of preparation, and the use of entirely worthless compounds, are fruitful sources of error, and we can point in illustration to the investigations of Dr. Harley on the galenical preparations of conium, in which he proves the absolute valuelessness of the extractum conii. Again, the assemblage of a number of active drugs in a prescription, often introduced at random, is destructive to a right appreciation of the effects of medicines, and, as a rule, the principle of combination should not be extensively tried till we are in a better position to estimate justly the influence of certain drugs on special diseases.

It has lately become the fashion to decry the study of materia medica, and it is asserted that the possession of such knowledge is a useless burden on the memory. I am persuaded that this is a mistake, and a serious one, and I am sure that many will from repeated experience bear me out in the belief that an accurate knowledge of the characters and properties of drugs is of every day utility to the prescriber, in enabling him to formulate correctly, to detect imposture, to avoid improper combinations, and to explain any phenomena that may unexpectedly arise.

Since our ignorance of the curative resources of the organism, and of the healing powers of drugs have been, and still are, the chief sources of error in therapeutics, and the chief obstacles to its improvement, it follows that the foundation stone for positive knowledge must be laid in more accurate investigations into the real properties of drugs, and this leads me to consider how we may best set about such improvement, and in what directions we can look for assistance in such a course. I shall pass over without further reference the direct gains to therapeutics, and the lessening of the chances of confusion which flow from improved methods of diagnosis, from the more strict localization and classification of disease, and from the prosecution of physiological and pathological studies, and will direct attention, in the first place, to the influence which organic chemistry and physics are now extending over practical medicine.

The outcome of all recent developments in science, and in especial, the doctrine of the correlation of force, *i.e.*, the indestructibility or conservation of energy, the corner-stone of science, has been

to render it in the highest degree probable that plants and animals are under the operation of the same laws as inorganic nature, and that all the changes and processes which are unceasingly at work within us are mainly the result of the action of physical and chemical forces upon the material constituents of our frame. The human body has often been resembled to a machine, and though the comparison between a living body and an inanimate machine should not be pushed too far, still the forces operating on each can reasonably be compared, and the more closely we know the limits of health, and the deviations that may occur from it consistent with life, the more surely can we propose to rectify the errors in function. Hence it is plain that a truly expressed science of medicine cannot be evolved except by endeavouring to refer the processes going on in the animal body, and therefore also the influence of remedies on these, to the ultimate laws of physics, chemistry, and physiology. "Chemical inquiry is now finding its way into many of the remoter secrets of function, and is likely before long to establish some laws of molecular constitution which will enable us to classify unknown remedies, and to explain and calculate their actions."—(Dr. Allbutt.)

The observations of Bence Jones and Dupré, who were the pioneers of this work in this country, have disclosed a rich mine of discovery, and they have demonstrated the existence of a chemical circulation within the body, which rivals in importance that of the older mechanical circulation of the blood. By the application of spectrum analysis they have shown the wonderful rapidity with which crystalloids diffuse from the blood into the colloid tissues, and from the tissues into the absorbents, and so the passage of all substances through the human body is determined by the laws of diffusion, modified by pressure. For example, if 20 grs. of carbonate of lithium are taken into the stomach, it will in two and a half hours have passed into every particle of the textures, and beyond the blood circulation even into the most distant parts, and in three and a half hours it will be distinctly present in each particle of the lens. In about seven days the lithium will be entirely eliminated from the body. When 7 grs. of carbonate of lithium were given eight hours before delivery, the lithium was subsequently detected in each particle of the umbilical cord.

Again, they have determined the existence, in animals, of a widely diffused substance which closely resembles quinine, and which has been named animal quinoidine. This leads to a plausible supposition, the only one yet offered, as to the mode of action of quinine in curing ague, and the hypothesis, though not proven, opens up a hopeful prospect of possible discovery.

The history of organic synthesis dates only from the year 1828, and remained comparatively barren for some years, but since the year 1845, its progress has been truly marvellous. The most complex substances are being formed at will, while the last barriers between organic and inorganic bodies are disappearing, and as the advances in this branch of science are, if I may say so, in the highest degree cumulative, the time is probably not far distant when, by the artificial formation of morphia and quinia, we shall be able to dispense with the production of opium, and the cultivation of cinchona in our colonies.

Every schoolboy is now familiar with the derivation of the most diverse colours from coal tar, and it is but the other day that alizarine, the colouring principle of madder, has been built up from another component of coal tar—the first instance of the artificial production of a vegetable colouring matter. We have just learned that artificial indigo has been isolated, and we may confidently hope soon to see the alkaloids brought into the market, derived not from their natural sources, and dependent on precarious supplies, but furnished to us by the laboratory of the chemist—the true magician of our age. [Even since these lines have been written, Schiff has announced the first attainment of this result in the artificial formation of conia.] The insight which we will thus gain into the constitution and intimate nature of complex organic molecules must prove of inestimable value as a stepping-stone to a true classification of remedies. So comprehensive is the aim of modern chemistry, and so wide the means of research, that "we can foresee a state of chemistry in which, without studying the properties of different bodies in detail, and knowing only the number, atomicity, and electric polarity of the elements, it will be possible to determine by simple calculation the formulæ, properties, and mode of preparation of all compounds possible" (Naquet.)

In a philosophic and suggestive paper, Dr Broadbent has made a bold attempt to apply chemical principles in explanation of the action of remedies and poisons, in which are contained, I believe, the elementary principles of scientific therapeutics. Starting from the two postulates—1st. That there must be some relation between the substance administered and the human organism on which the effects produced depend. 2nd. That, so far as the substance is concerned, the basis of the relation can only be its *chemical* properties, using this term in its widest sense, certain important corollaries flow from these:—1. That the physiological and therapeutical actions of the same substance must be similar in kind. 2. That the action of foods, medicaments, and poisons, in the system, must be capable of explanation on the same principle. 3. That substances closely allied chemically, must have an analogous action on the system, or the diversity in their operations should be capable of explanation on chemical principles; in other words, chemical groups ought to form therapeutical groups. This is an outline of the path to be pursued, and some steps of importance have been already gained by individual workers. In England and Scotland the names of Bence Jones, Richardson, Crum Brown, and Fraser, stand out in honourable relief; in France, among a number of observers, Mialhe, Rabuteau, and MM. Pélissard, Jolyet, and Cahours; and in Germany, Liebreich, Binz, and many others, have pursued the investigation of the physical and chemical action of drugs with results most encouraging, though, as yet, imperfect and incomplete.

In determining the action of any substance from a chemical point of view, Dr. Richardson has shown that we have to consider five points, viz :—1. The elementary basic or radical composition of the substance to be tested, and the changes of constitution to which it may be subjected; 2. The physical qualities of the substance; 3. The chemical stability of the substance; 4. The physical peculiarities of the animal body subjected to the substance; and 5. The special action of the substance on special centres of the animal organism.

Some scattered attempts to express the relation which, no doubt, exists between the physiological action of a substance and its chemical composition and constitution (*i.e.*, the mutual relation of the atoms in the compound) have from time to time been made, but until lately with trifling success. For example, it has long been observed that, as a rule, the salts of the same base and of the same acid have respectively a common physiological action, and Mr. Blake, of California, pointed out many years ago, and has lately extended his experiments, that, in general, isomorphous substances have analogous actions.

But the most decided step in this direction has been made by Drs. Crum Brown and Fraser, in their important papers on *the Connexion between Chemical Constitution and Physiological Action*, (1868-69). By introducing a known chemical change into the constitution of a physiologically active substance, without breaking up its molecule, they have shown that the physiological action of the substance may be completely altered, and, in fact, inverted in kind.

They have examined with great care the physiological action of the salts of the ammonium-bases derived from eight of the better known alkaloids, and their results lead to the suspicion that chemical *condensation* (*i.e.*, susceptibility of addition) is in some way connected with physiological activity, and that saturated bodies (*i.e.*, whose condensation = °) are inert or nearly so. Thus by the addition of iodide of methyl to the non-saturated base strychnia, the poisonous activity of that alkaloid is diminished at least 210 times, and a quantity of iodide of methyl-strychnium, containing 21 grs. of strychnia, can be given to a rabbit with impunity. These observations are of the highest value, though at present they must be considered as but foretastes of what is to come, and it is remarkable that almost immediately after, two French physiologists, MM. Jolyet and Cahours published results corresponding in almost every respect with those of Brown and Fraser.

Dr. Richardson has done good work in the field of anæsthetics in their chemico-physical relations, and he has brought out the curious and interesting fact that, in the alcohol group, the anæsthetic effect has a definite connexion with the chemical composition of the alcohol, the anæsthesia rising in proportion to the number of atoms of carbon; for example, contrast the action of ethylic alcohol, containing C_2, with amylic alcohol, containing C_5. It is observed also that definite changes are produced by the addition or substitution of new elements or radicals, such as H, Cl, I, C_2H_5, &c., and when the chemical relationships between different bodies are more thoroughly understood, we may eventually be able to deduce *à priori* the physiological action of a body from its known chemical history.

Dr. Rabuteau, who has made many contributions to physiological chemistry, believes that he is justified by his investigations in propounding, as a general law, that "the metals are more active physiologically, according as their atomic weights are more elevated, or, what is the same thing, as their specific heats are lower," *e.g.*, Na, K, and Tl. The diatomic metalloids conform also to this atomic law, but the monads, curiously, are governed by a law which is the reverse of this. Thus F, Cl, Br, and I, is the order of physiological activity of the halogens, and this is precisely the inverse order of their affinity for O.

These illustrations are, at least, sufficient to shadow forth the assistance, qualitative and quantitative, which we may expect from

physical and chemical science, and warrant us in believing in a sure foundation for future therapeutics. It is true that the facts, as yet known, are mostly isolated and disconnected, but we may compare them to separate bricks which, though singly of little value and without cohesion, yet when cemented and fitted together, will form a firm and durable superstructure. The physiological school, headed by C. Bernard and Brown-Séquard, has done much to elucidate the action of some most important drugs, and it is likely that the doctrine of physiological antagonists will lead to practical results.

The different effects of remedies when introduced by different channels, the principle of the administration of smaller doses frequently repeated, and the potency of drugs over the vaso-motor nerves are all receiving a greater or less share of attention, and are exerting a wholesome influence on our habits and methods of prescribing.

Yet even with the most perfect knowledge of the chemical and other properties of drugs, we cannot satisfactorily judge of the influence which they exert on disease, unless we know, in any case of recovery in which medicine has been used, what share is to be assigned to the curative power of the organism itself. The evident importance of this inquiry was recognized by the Austrian School of Medicine for years before it attracted much attention in these countries, and we have now, at all events, learned that a large proportion of diseases, numbering some of the most formidable character, may get well without the use of any drugs whatsoever, or, in other words, they have a natural tendency to terminate in the restoration of health. This salutary change of doctrine is due in part to an examination of the undeniable results afforded by homœopathic practice, but largely owes its impetus to the improved state of physiological and pathological science. There is, however, some danger of being over-zealous in our respect for nature's operations, for the efforts of nature are not always of a benignant tendency, and what is called "expectant medicine" may sometimes prove but "a meditation upon death."

A more accurate knowledge of the real properties of drugs than we have hitherto possessed, lies at the root of all future progress, and the mode of its accomplishment claims attention at the outset. This will be best carried out by carefully conducted trials on *healthy* individuals, checked by collateral experiments on the lower animals, and on patients suffering from diseases whose diagnosis, general course, and variations, are tolerably well known. Hitherto it has been almost exclusively the custom to endeavour to acquire a knowledge of medicines by instituting trials with them in disease, a method which has borne little fruit in return for the labour bestowed upon it. To Hahnemann, in particular, before he was carried away by the delusion of infinitesimal doses, belongs the credit of actively pushing forward the proving of medicines on healthy individuals, recommended by Störck, Alexander, and Haller, and it is strange that, with very few exceptions, no provings of worth have been made by other practitioners until very lately.

Within the last two years Dr. J. Harley has shown the value of this line of inquiry in his elaborate and searching work on the action of opium, belladonna, conium, and hyoscyamus, in which he has done much towards defining our knowledge of the effects and uses of these ancient neurotic remedies.

One most important issue of the careful testing of drugs would be the better determination of the "sphere of action" of each medicine, for it is already well known that certain drugs affect particular organs and tissues, and I believe, with Dr. Rogers, that this significant fact of drugs possessing elective affinities for certain textures will occupy a prominent place in our future therapeutics. We have reason to believe that the physiological and therapeutical actions of medicines are very closely related, and it is probable that the modifications impressed by various diseased conditions will not so materially alter their sphere of action, as is sometimes supposed.

Another real gain from this probation of drugs would be the expulsion from the Materia Medica of a crowd of articles which only serve to keep alive the embers of polypharmacy, and to obstruct our advance towards a more rational system of therapeutics. If we accept, as we may safely do, the axiom that a drug, which produces no perceptible effects when properly tested on healthy individuals, will prove equally inert in disease; what a host of reputed medicines would be cast into deserved oblivion?

Before concluding, I wish to point out most emphatically that we should not allow ourselves to overlook the continued necessity for bedside observation in our admiration of the progress and prospects of the scientific departments of medicine. Though our theoretical knowledge were ever so perfect, yet clinical experience must always hold an important position to every true physician, and "it is to the experience of the

mass of the profession that we look for the final establishment of doctrine and rules of practice." The most rapid and complete advances in science can never do away with the necessity for watchful observation, and "the nice adaptation of means to end can only be gained by experience." The past history of medicine should teach us not to be too hasty in condemning or ridiculing a line of practice which united and prolonged experience has approved, even if it be contrary to the received dogmas of the day, or be incapable of immediate explanation. Rational experience must and will keep its place. Let it by all means be reinforced and directed aright, but not trammelled, and clinical researches and empirical decisions must eventually prove the touchstone of therapeutical theory.

Keeping in view, then, that the three chief aims and objects of medicine, especially so far as concerns the non-professional public, ought to be the cure of disease, the prolongation of life, and the alleviation of physical suffering, we can sum up, in Sir W. Jenner's words, our gains in practical medicine as resulting in "advances in knowledge, in the addition to the science of medicine of new facts, the elimination of supposed facts, the more correct appreciation of the bearing of old facts, and the application of this new knowledge to the advancement of the practical objects of the science."

And, though the discoveries of our own time naturally appear to us of greater importance than those of preceding ages, even the most incredulous will admit that we have reached a stage when ignorance is giving way to knowledge, hypothesis to facts, and that the time is approaching when we shall be able to free ourselves from the quicksands of uncertainty, and rest on the firm basis of knowledge and truth.

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