femoral and popliteal arteries, and seemed to be largely formed of the external coat of the vessel. I have also placed on record a case of traumatic axillary aneurism, in which it was an open question whether a complete or partial rupture of a segment of the arterial circumference was the primary lesion. The patient was seventy years of age. Four days before admission, he had fallen out of a cart and displaced his left humerus at the shoulder-joint. Two days after the accident, the dislocation was reduced by a practitioner by extension with the foot in the axilla. On admission, there was considerable, but not excessive, swelling of the shoulder-joint, without pulsation in the swelling. The pulse beat naturally at the wrist. A small hard lump, about the size of a pigeon's egg, could be felt at the base of the axilla, over the site of the axillary vein, and not receiving impulse from the axillary artery. Under these circumstances, more fully detailed elsewhere, I diagnosed an effusion of blood into the axilla and shoulder-joint, stating my belief that the blood came, not from a ruptured axillary artery, but either from smaller vessels or from the vein. Strict rest, with bandages only, was enjoined to promote absorption. A month later, when the patient was not under my charge, aneurismal symptoms appeared, pulsation and *bruit* being both perceptible, and the pulse at the wrist became feebler. Various suggestions as to treatment were made, the most prominent being ligature of the subclavian artery; but the patient declined operative interference. Ultimately, ulceration of the skin took place, hæmorrhage occurred, the axilla was laid open, and the injured vessel was secured at the seat of lesion. A transverse aperture was found in the third part of the course of the axillary artery, occupying about a third of the circumference of the artery. In view of the uncertainty as to the source of the effusion of blood in this case, and the advanced age of the patient, which rendered any operative interference at any stage of his case wellnigh hopeless, there can be no doubt as to the propriety of the treatment recommended in the first instance by myself. I am pleased to have the opportunity of quoting a published opinion of my colleague Mr. Maunder in reference to this subject. In his Lettsomian Lectures (*vide Lancet*, February 27th, 1875, page 295, Case 24), Mr. Maunder speaks strongly against the application of a ligature to the subclavian artery, and states that either of the two other means of treatment at our disposal, amputation at the shoulder-joint and laying open the axilla, would in all probability have terminated fatally. This opinion effectually disposes of the suggestions made at the time that ligature of the subclavian artery ought to have been performed, and that it would have been better to operate at an early period in the history of the case.

A point of considerable interest arising out of this case is the possibility of locating the seat of injury in some of the cases of ruptured axillary artery. It is evident that, if the third part of the artery be involved, and if this can be rendered probable, the severe procedure of laying open the whole anterior wall of the axilla might be avoided. A careful exposure of the third part of the artery would then be sufficient, combined with a simultaneous removal of the clots in the axilla. The introduction of the finger would probably enable the surgeon to trace the vessel and find out where pulsation ceased or became less marked even in a case which he had not previously seen. If, however, the case had come under his observation early, and there existed, as in my own case, a small hard circumscribed lump over the third part of the course of the vessel, the diagnosis would be rendered more probable. At all events, a more prolonged search for the seat of injury should be made whenever such a course is practicable, without danger of serious hæmorrhage continuing during the exploration.

Next, I would strongly recommend the application of the stethoscope over the site of an artery which there is any reason to think is ruptured either partially or completely. There is good reason to believe that, by means of auscultation, partial and complete ruptures may be diagnosed in some cases shortly after the injury, and when pulsation cannot be perceived. Mr. Holmes states that he has never seen any case where pulsation was present in the extravasated blood in connection with a complete subcutaneous laceration of an artery; but he says that, in some cases, a *bruit* can be heard (*Surgery: its Principles and Practice*, page 78).

Lastly, in these days of antiseptic surgery, when knee-joints can be laid open and fingered with impunity, when the surgeon is freed from all anxiety as to the results of operations, when exposed blood-clots cease to break down and decompose, but become organised in the open wound under the charm of carbolic acid and the antiseptic dressing, it may fairly be asked whether, in such cases as that which I have related, amputation should not be set aside until the wounded vessels have been sought for and tied in the popliteal space. Some of the extravasated blood could be removed by the operator and the rest be allowed to become either organised or absorbed; and, if the knee-joint

had been opened by the accident, the triumph of antiseptic surgery would be all the greater. The only possible question would be, whether the circulation in the member could be carried on sufficiently to prevent the occurrence of gangrene. If this question should be capable of receiving an affirmative reply, no excuse would be left for the mutilation hitherto regarded by septic surgery as indispensable to save the life of the patient.

THE BEARING OF EXPERIMENTAL EVIDENCE UPON THE GERM-THEORY OF DISEASE.

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THOUGH it may be conceded that with our present state of knowledge an affirmative decision in regard to the absolute proof of the present occurrence of archebiosis may be still withheld, there is, I think, no similar warrant for suspense of judgment in regard to the germ-theory of disease, or, as it is also called, the doctrine of *contagium vivum*. Existing evidence seems to me abundantly sufficient for the rejection of this doctrine as untrue.*

My urine and potash experiments will go far to illustrate this difference in the weight of the evidence in regard to the two questions.

A "sterilised" fluid—that is, one which left to itself would always remain pure—may be caused to ferment by the addition of a certain proportion of liquor potassæ devoid of all living things, especially if the influence of the potash be favoured by certain accessory physical conditions. This fact is admitted by M. Pasteur himself. During the fermentation thus initiated, a matter (ferment) appears and increases, which is capable of spreading a similar process far and wide in suitable media.

But, on the strength of the analogy upon which the germ-theorists rely, we may find in such an experiment a warrant for the belief that in a healthy person, free from the contagium of typhoid fever or any other of its class, certain kinds of ingesta (solids or fluids), wholly free from all specific poison, may, with or without the favouring influence of other altered conditions, give rise to an independent zymotic process. And during the process thus initiated, a matter (contagium) appears and increases in certain of the fluids or tissues of the body, which is capable of spreading a similar disease far and wide amongst receptive members of the community.

Can the germless liquor potassæ, *plus* the favouring conditions (the principal of which is a certain high temperature), be regarded as the "cause" of the fermentation? The answer does not admit of doubt; the effect in question would not have taken place without their influence. The old logical formula in regard to the word, *cessante causa cessat et effectus*, completely justifies this point of view, and so also does the definition of Sir John Herschel. A "cause", said this philosopher, is "an assemblage of phenomena which occurring, some other phenomenon invariably commences or has its origin".

But there is a point of view which must not be lost sight of. It is of considerable importance, and has of late been dwelt upon by G. H. Lewes with his usual force and clearness. He says: "The fact, that it is a convenience to select some one element out of the group, either for its conspicuousness, its novelty, or its interest, and that we call it the cause of the change, throwing all the other elements into the background of conditions, must not make us overlook the fact that this cause—this selected condition—is only effective in coalescence with the others. Every condition is causal; the effect is but the sum of the conditions."

This brings us to the only point of doubt which can possibly exist in regard to the interpretation of my experiment. It is, whether our

* Since this paper was read, the doctrine has again been proclaimed—and never with more force and ability—by Dr. William Roberts (*BRITISH MEDICAL JOURNAL*, August 11th, 1877). Its essential points may be stated in the words of its latest exponent. He says: "I have already directed your attention to the analogy between the action of an organised ferment and a contagious fever. The analogy is probably real, in so far, at least, that it leads us to the inference that contagium, like a ferment, is something that is alive. . . . If, then, the doctrine of a *contagium vivum* be true, we are almost forced to the conclusion that contagium consists (at least in the immense majority of cases) of an independent organism or parasite; and it is in this sense alone that I shall consider the doctrine. . . . it is more than probable, looking to the general analogy between them, that all infective diseases conform in some fashion to one fundamental type. If septic bacteria are the cause of septicæmia, if the spirilla are the cause of relapsing fever, if the *Bacillus anthracis* be the cause of splenic fever, the inference is almost irresistible that other analogous organisms are the cause of other infective inflammations and other specific fevers."—Sept. 1877.

† *Problems of Life and Mind*, vol. ii, p. 390.

most prominent causal element, the liquor potassæ, exercises its influence (a) partly upon the fluid and partly upon certain otherwise dead or impotent germs still lurking within the vessel, or (b) simply upon the mere chemical constituents of the fluid medium, but in such a way as actually to engender minute particles of living matter which thereafter appear as ferment-organisms.

If a practically dead germ can by any treatment be revived, it may take its place as one of the causal conditions leading to fermentation; hence it is that a certain reserve may still be maintained as regards the absolute proof of the possibility of a germless origin of common fermentations, and the almost simultaneous occurrence of a new birth of living units (Archebiosis).

But all similar grounds for reserve are absent—are non-existent, in fact—in regard to the bearing of this experiment upon the possibility of an occasional independent origin for zymotic disease, whether or not such disease is characterised by the appearance within the body of any distinctive living organisms.*

This I will now endeavour to demonstrate.

It is the process of fermentation which is supposed to be in part analogous to the zymotic disease. It is true that a contagious something becomes engendered during fermentation and during zymosis, by means of which the process or the disease may be spread abroad. But there are important differences in regard to the possible independent origin of the two processes which have hitherto been only too much neglected. The treatment of this subject has often been much too superficial. In order to produce a kind of pictorial effect which may easily captivate the imagination, difficulties are often ignored, and many new, modifying, or antagonistic points of view have even of late been treated as though they were non-existent.

A few words will suffice to make plain some of the differences between the respective conditions which would be operative in the germless origin of fermentation on the one hand, and in the *de novo* origin of a contagious disease on the other. And in so doing I shall be able, I think, at the same time, to show how much simpler it would be to bring about an independent zymosis than an independent fermentation—that is, if we are to rely on the analogy upon which the germ-theorists base their arguments.

During the great majority of fermentations, living organisms make their appearance and rapidly multiply. These living organisms have been proved to be common producers of chemical principles, some of which are soluble ferments, others (like pyrogen) are poisons which may be almost as deadly as that of a serpent, whilst others still are inert and appear as mere pigment granules.† It is proved that some of these chemical principles act as true ferments.‡ It is thought, and it is probable, that the organisms themselves—altogether apart from their media and what else they may contain—may be capable of doing the same. Still this has not yet been definitely proved; so that the action of soluble chemical ferments is at present almost better substantiated than that of the living organisms by which they may have been formed. By means of boiling alcohol and other agents, these bodies can be isolated and freed from living impurity. It is, however, much more difficult entirely to separate minute living organisms from their media, and consequently more difficult to be perfectly certain in regard to their potencies. It is, however, on account of the derivation of the chemical ferments from the living units, and because of the presence of these latter bodies in all fermenting mixtures, that their own agency is still regarded by many as essential to the initiation of ordinary fermentations. But, as I have already indicated, we much need further information as to the precise mode in which fermentation is initiated and carried on by soluble ferments like that which M. Musculus discovered in and separated from urine. If they (all or any of them) are capable of setting up fermentations in germless fluids, in the course of which organisms appear, such phenomena would most effectually disprove an exclusive germ-theory.

Turning now to the process of zymosis, we find the available generative conditions altogether different. Here we have to do not with fluids only, but with tissues and organs composed of living elements characterised by all kinds and degrees of activity. Some of them produce the various soluble ferments of the body, some may produce poisons, and others habitually lead to the formation of pigment-granules—vital acts severally similar in kind to those which the common fer-

ment-organisms are known to manifest. Tissue-elements without number having such and multitudes of other properties are therefore ever present, capable, under certain influences, of being more or less easily diverted into unhealthy modes of action, so that many of them may become true living ferments in the modern sense of that term,* and, therefore, possible producers of chemical ferments (contagia) capable of initiating some or the whole of the series of changes by which they were themselves produced, in other suitable sites.

The essential difference between the two problems thus becomes plain. The only point which my experiment leaves in the least doubtful in regard to the causal conditions initiating fermentation is, whether any latent, powerless, and, as it were, dead organised ferment may still, in spite of the usual evidence to the contrary, lurk in the seemingly "sterilised" fluid. This, however, is the very point about which there is no shadow of doubt in regard to zymosis. Possible ferments without number are, by necessity, present in the form of tissue-elements. So that if we are to be guided by the analogy upon which all germ-theorists so strongly rely, the independent generation of a zymotic process should, for the reason above specified, be incomparably more easy to be brought about than fermentation in a germless fluid. In regard to the independent origin of a zymosis, the all-important point is, not whether latent ferments exist, but whether any causes, or sets of unhygienic conditions, can rouse or modify, in certain special modes, the activity of any of these myriads of potential ferments of which the human organism is so largely composed. And if, as some germ-theorists would have us believe, impotent germs of common ferment-organisms, incapable of exclusion, are also widely disseminated throughout the body, these, if they are such unavoidable elements, could (in regard to the etiology of disease) only be looked upon as components of the body, ranking side by side with the tissue-elements themselves.

Thus such organised ferments or germs as are possibly absent from the "sterilised" experimental fluids are confessedly present by myriads in persons who may be sickening under the influence of various unhygienic conditions or non-specific states of the system; and the only point which is regarded as doubtful in connection with the *de novo* origin of a zymosis, is what analogy might lead us to affirm as completely proved by my experiments, viz., that certain conditions, or states of system, may be capable of rousing some of such ferments into a specific kind of activity, wholly apart from the influence of any specific contagia coming from without.†

Even if independent ferment-organisms of common or special kinds do make their appearance during any process of zymosis originated in the manner above suggested, they would, from the point of view of the etiology of disease, be just as much consequences of the morbid influences, as proliferation of tissue-elements is a consequence of the direct application of acetic acid or any other irritant.

But here, in order to make this point of view more plain, a short digression is necessary.

The intracellular fermentation in vegetal tissues supplies us with a kind of link between the ordinary processes of fermentation and the zymotic processes of animals. MM. Lechartier and Bellamy, as well as Pasteur and others, have now clearly shown that in vegetal tissues placed under certain abnormal or unhealthy conditions, fermentative

* How legitimate this statement is may be seen from what M. Pasteur himself says. These are his most mature views: "I have been gradually led to look upon fermentation as a necessary consequence of the manifestation of life, when that life takes place without the direct combustion due to free oxygen. . . . We may partially see, as a consequence of this theory, that every being, every organ, every cell which lives or continues its life without making use of atmospheric air, or which uses it in a manner insufficient for the whole of the phenomena of its own nutrition, must possess the characteristics of a ferment with regard to the substance which is the source of its total or complementary heat."—*Compt. Rend.*, 1872, t. lxxv. p. 784.

† A very interesting address by Dr. B. W. Richardson, F.R.S., has been lately published (*Nature*, October 4th, 1877), entitled "A Theory as to the Natural or Glandular Origin of the Contagious Diseases". In it the author advances many strong arguments against the germ-theory; he also propounds some interesting speculations as to the mode of origin and action of the chemical principles, or poisons, which constitute, as he believes, the "contagia" of the communicable diseases. Some such views make a very fitting supplement to the doctrines which I have been here attempting to establish in regard to these diseases; only we must, as Dr. Richardson observes, seek gradually to put well proven facts in the places now occupied by mere speculations. In regard to the practical aspects of the two opposite doctrines, Dr. Richardson makes some very pertinent observations. "If the *contagium vivum* view be true," he says; "if the air around us is charged with invisible germs, which come from whence we know not, which have unlimited power to fertilise, which need never cease to fertilise and multiply, what hope is there for the skill of man to overcome these hidden foes? Why on some occasion may not a plague spread over the whole world, and destroy its life universally? Whilst, on the other hand, if the opposition notion be true, we have complete mastery over the diffusion of the poisons of all the communicable diseases. We have but to keep steadily in view that the producing and the reproducing power is in the affected body; and we can, even with our present knowledge, all but completely limit the action to the propagating power of that body—its power, I mean, of secretion and diffusion of secretion."—October 6th, 1877.

* The rule is, that organisms are present in fermentations, whilst they are, so far as we know, quite exceptional in zymotic diseases.

† Pasteur, *Compt. Rend.*, July 3rd, 1876, p. 4.

‡ The more efficient means of filtering organisms from their media, which we now possess, by means of porous earthenware, ought to be useful in this direction. Such organisms and their germs might be subsequently washed with several distilled waters, just as a chemist would wash a delicate precipitate. It would be strange indeed if this very mild usage interfered with the properties of organisms which at other times are credited with such remarkable powers of endurance.

phenomena take place essentially similar to those occurring in solutions containing independent ferment-organisms. And just as the vegetal cell can do what, in other cases, the independent organism does, so it is supposed that in the process of zymosis, tissue-elements may take on a specifically faulty action, leading to the formation of certain chemical principles or "contagia" in the fluids or tissues of the animal body; so that, in the great majority of zymotic diseases, offcast particles from the body, whether living or dead, when saturated with such principles, may constitute the veritable contagia by which the specific disease is spread abroad amongst the community.

In the majority of the cases of intracellular fermentation, no independent organisms are generated, though in others, as in that of the beetroot and the potato, they are invariably concomitants. Similarly, in the majority of zymotic diseases, no independent organisms are generated, though in others, such as relapsing fever and splenic fever, they are invariable concomitants; and being engendered in diseased parts and fluids, they may thereafter themselves act either as real contagia or as carriers of contagion.

The causal conditions capable of inducing fermentation in the beetroot and the potato, and with it the appearance of bacteria in swarms throughout their tissues, are known, and have no ordinary connection with pre-existing bacteria. And similarly, the causal conditions capable of inducing relapsing fever and splenic fever, though not so definitely known, may nevertheless have no ordinary connection with pre-existing spirilla and bacilli resembling those which appear in the blood or tissues of the patients suffering from either of these diseases.

Thus the mere fact, that in certain zymotic diseases living organisms have been proved to appear, affords of itself no support whatever to an exclusive germ-theory, as I shall, after this digression, endeavour to show.

The fact may be quite otherwise explained, either (1) in accordance with the views of certain germ-theorists, though these are in direct opposition to the statements of others of the same party; (2) in accordance with the statements of this second section of the germ-theorists, supplemented by a belief in heterogenesis.

(1) The presence of latent germs of common, though modifiable, ferment-organisms throughout the body is invoked by one section of the germ-theorists, who contend that certain altered states of health, together with altered vitality of tissues, may rouse such hitherto latent common organisms into activity, and occasionally convert them into so-called "specific" forms capable of new actions. But based as this view is upon wholly insufficient evidence, and with its fundamental position denied by other leading germ-theorists, it would, even had it been securely founded, be quite inadequate to meet the necessities of their position. A special zymotic disease, which had arisen in the manner above indicated, would assuredly have had what is termed a *de novo* origin—it would have started from no specific cause, and would never have developed, but for the existence of those "determining conditions" which brought about the altered state of health and tissues. This group of conditions would therefore constitute the cause of the disease; and inasmuch as, by the hypothesis we are now considering, the common germs are held to be *ever present and unavoidable*, any changes or developments which they might take on could only be studied in the same rank and side by side with those of the other tissue-elements—that is, as consequences or phenomena of the disease.

(2) It was originally affirmed by Professor Burdon Sanderson,* and it has of late been distinctly reasserted by M. Pasteur,† that the blood and internal tissues of healthy animals and of man are entirely free from ferment-organisms or their germs. Some have sought to modify this view, on the strength of certain experiments, which are so extremely inconclusive as to make it almost puerile to have brought them forward.‡

For, however strong the evidence is that living units may, on certain occasions, be even proved experimentally to appear in fluids, in which no living matter previously existed (archebiosis), it is even stronger to

show that, under certain conditions, similar low independent forms of life may originate in the midst of living tissues previously free from them, by a kind of transformation (heterogenesis) of some of the units of protoplasm, which, though still living, have been modified in nature and tendency by reason of their existence in a partially devitalised area.

The evidence in favour of this last kind of change may be regarded wholly apart from that furnished by the closed-flask experiments, from which it is quite distinct. It suffices, I think, to account for the presence of organisms in some of those local and general diseases with which they are known to be associated, and therefore to complete the proof that even such disease may originate *de novo* (as well as by contagion), and that the organisms which characterise them are, in such cases, consequences or concomitant products, not causes, of the local or general conditions at whose bidding they appear. The elements of the proof are these:

(a) First, there is the evidence which has been adduced by various observers as a result of the study by the microscope of the mode in which organisms appear within tissue-elements. I do not lay much stress upon this here, because evidence of such a nature is more open to various objections than that which is to follow.*

(b) Although the blood and internal tissues of healthy animals and of man are free from independent organisms and their germs, yet such organisms will habitually show themselves after death, in the course of a few days, throughout all the organs of one of the lower animals or of man—even when life has been abruptly terminated during a state of health. It cannot be said, in explanation of this, that the organisms naturally present in the intestinal canal have been enabled to spread through the body so as to reach its inmost recesses *after death*—since many of the organisms found are motionless, and others have mere to-and-fro movements of a non-progressive character. The blood, again, has ceased to circulate, so that this fluid, germless during life, cannot after death be considered to act even as a carrier. If the organisms themselves cannot make their way through the tissues, and if no carrier exist, they must naturally have been born in or near the sites in which they are found.

Phenomena of this kind are to be witnessed even in insects, such as silkworms and flies; and the organisms that habitually develop in them after death are, as in the case of higher animals, just such organisms as appear in some of their best-known contagious diseases.† Certain of these diseases, like "muscadine", seem to be generable *de novo* at the will of the operator, by merely placing the animal for a few days under particular sets of unhealthy conditions.

(c) Some of the ferment-organisms may also be made to appear at will in certain parts of still living and previously healthy animals by determining in any such part either (1) a greatly lowered vital activity, or (2) an active perversion of the nutritive life of the part of considerable intensity.

1. This subject has been studied experimentally by Messrs. Lewis and Cunningham,‡ two thoroughly competent and trustworthy observers, whose researches during recent years have won for them a deservedly high reputation. They say: "The object of the experiments was to ascertain whether, by interfering with the vascular supply of certain tissues and organs of the body of an animal without injuring the isolated tissue, we should be able, within the course of some hours, to detect organisms in those parts in the same manner as we had been able to do when an animal had been killed under chloroform and set aside in a warm place. We found that such was the result, and that a kidney, for example, when [its artery was] carefully ligatured without interfering with its position in the abdomen, would be found after some hours to contain precisely similar organisms; whereas the other kidney, whose circulation had not been interfered with, contained no trace of any vegetation whatever."§

* On this subject, see *Beginnings of Life*, vol. ii, p. 342.

† *Ibid.*, pp. 527, note 1, and 330, and *Transactions of Pathological Society*, 1875, p. 343.

‡ *The Fungus-Disease of India*; Calcutta, 1875, p. 89.

* *Thirteenth Report of the Medical Officer of the Privy Council.*

† *Comptes Rendus*, April 30th, 1877, p. 900.

‡ Cutting out portions of the internal organs of recently killed animals, enveloping them with superheated paraffin, and then placing them in an incubator at a suitable temperature to see whether germs and organisms will appear, would, even if taken alone, obviously permit no certain conclusion to be drawn from their appearance. But the evidence relied upon by Sanderson and Pasteur tends as strongly to show that they are not developments of pre-existing germs, as certain other evidence subsequently to be mentioned tends to show that they are heterogenic products (*Transactions of Pathological Society*, 1875, p. 267). Yet, following a now long established custom of ignoring the possibility of the heterogenic origin of bacteria, the results of such experiments are by some supposed to demonstrate the existence of latent germs in an organ like the spleen, for instance, which is wholly cut off from outside communication—and even when the blood itself is declared to be germless.

§ On September 17th, 1877, I had an opportunity of seeing how far this would hold good for the human subject. On that day I made an examination, twelve hours after death, of the body of a young man who had been suffering from severe heart-disease in University College Hospital. His temperature had only been slightly raised for about forty-eight hours before death; but there was reason for believing that embolic obstructions had recently occurred in one or both kidneys. Abundant "vegetations" were found on the mitral and aortic valves, and two or three embolic patches existed in each kidney, some being recent and others of older date. One large yellowish embolic patch was likewise found occupying the upper extremity of an enlarged spleen. Some blood from the right ventricle and some urine from the bladder, carefully removed with capillary tubes, on examination with the microscope and a 1-12th object-glass, showed no organisms of any kind. Portions of tissue cut from the interior of the liver also showed no organisms. On the other hand, the embolic patch in the spleen as well as those in the kidney, both old and recent, showed, when portions of their disintegrated substance were examined, organisms, more or less abun-

2. Facts of this second order have been thoroughly established by the important researches of Professor Burdon Sanderson. He says: "If a few drops of previously boiled and cooled dilute solution of ammonia are injected underneath the skin of a guinea-pig, a diffuse inflammation is produced, the exudation liquid of which is found after twenty-four hours to be charged with bacteria." "Other chemical agents", he adds, "will lead to the same results, and always under conditions which preclude the possibility of the introduction of any infecting matter from without."

Elsewhere,[†] the same investigator refers to experiments which were made about the same time in order to throw light upon the cause of the appearance of bacteria in certain peritoneal exudations, and to ascertain whether or not their presence was to be considered as "a mere result of the intensity of the peritonitis". He says: "To determine this, experiments were made during the following month (May 1871), which consisted in inducing intense peritonitis by the injection, not of exudation liquids, but of chemical irritants, particularly dilute ammonia and concentrated solution of iodine in hydriodic acid. As regards the ammonia, precautions were taken to guard against contamination by boiling and cooling the liquids, as well as the implements to be used, immediately before injection. In the case of the iodine solution, this was, of course, unnecessary. In every instance, it was found that the exudation liquids, collected from twenty-four to forty-eight hours after injection, were charged with bacteria, whence it appeared probable that the existence of these organisms was dependent, not on the nature of the exciting liquid by which the inflammation was induced, but on the intensity of the inflammation itself."

From the various evidence, more or less fully referred to in the present section, it seems to me legitimate to conclude:

First, that, if we are to be guided by the analogy now dwelt upon as existing between fermentation and zymosis, it would be perfectly certain that the latter process can originate *de novo*—that is, under the influence of certain general or special conditions, and where specific contagia of any kind are at first absent, though they subsequently appear as results or concomitant products. So that an exclusive theory of "contagion", as the only present cause of communicable diseases, is not supported by experimental evidence.

Secondly, that some contagia are mere not-living chemical principles, though others may be living units.

Thirdly, that, even in the latter case, if the primary contagious action be really due to the living units and not to the media in which they are found, such primary action is probably dependent rather upon the chemical changes or "contact actions" which they are capable of setting up than upon their mere growth and vegetative multiplication.

Fourthly, that, where we have to do with a true living contagium (whether pus-corpuscle or ferment-organism), the primary changes which it incites are probably of a nature to engender (either in the fluids or from the tissue-elements of the part) bodies similar to itself, so that the infected part speedily swarms therewith. When pus from a certain focus of inflammation comes into contact with a healthy conjunctiva, and therein excites a contagious form of inflammation, no one adopts the absurd notion that all the pus-corpuses in this second inflammatory focus are the lineal descendants of those which acted as the contagium; and the mode of action may be altogether similar when matter containing bacilli, by coming into contact with a wounded surface, gives rise to splenic fever and the appearance of such organisms all through the body. The old notion about the excessive self-multiplication of the original contagium is probably altogether erroneous.

Thus, all the distinctive positions of those who advocate a belief in the so-called "germ-theory of disease", or rely upon the exclusive doctrine of a "contagium vivum", seem to be absolutely broken down and refuted. We may give that attention to the appearance and development of independent organisms in association with morbid processes which the importance of their presence demands, but we must regard them as concomitant products, and not at all, or except to an extremely limited extent, as causes of those local and general diseases with which they are inseparably linked.

dantly distributed, similar to those which Messrs. Cunningham and Lewis have figured. Some were bacilli, and some were more like what Cohn now distinguishes as vibrios. They were not so abundant as to be always found without careful examination; and, on the other hand, in the diseased splenic tissue there were a multitude of small acicular crystals, which an inexperienced observer might mistake for motionless organisms. In the lower healthy portion of the spleen no organisms were found.

* *Transactions of the Pathological Society*, 1872, pp. 306-308.

† *Reports of the Medical Officer of the Privy Council*, etc., new series, No. vi, 1875, p. 57.

ON REDUPLICATION OF THE FIRST SOUND OF THE HEART.

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IN commenting on reduplication and accentuation of the sounds of the heart, it is necessary, in order that all ambiguity may be avoided, to state at the onset what is considered to be the mechanism of the sounds. Here, however, it is not proposed to enter upon the controversy, but merely to specify that in the following observations the first sound is considered to be caused by tension of the auriculo-ventricular valves, and the second by tension of the semilunar valves.

The fact that a doubling of the first sound of the heart sometimes occurred was known to the older authors; but, in his Lumleian lectures, the late Dr. Sibson propounded a theory, that this reduplication is due to an asynchronous action of the ventricles; and Dr. George Johnson, in the Lumleian lectures for the year 1877, propounds another theory, that it is due to an audible contraction of a dilated and hypertrophied auricle. The condition in which this phenomenon is most frequently observed to exist is an abnormal degree of arterial tension, which is a constant accompaniment of Bright's disease. Briefly, Dr. Sibson's theory is this. In consequence of the greater tension of the arteries, the left ventricle, meeting with an increased resistance, acts more tardily than the right; hence, a doubling of the first sound. Generally, there is no doubling of the second sound; and this is a difficulty which at first sight appears to stand in the way of accepting the theory of asynchronism. Dr. Sibson met this difficulty by the following explanation. The greater tension, both relative and absolute, of the aorta, causes it to recoil more rapidly than the pulmonary artery and make up in speed what it has lost in time, the two arteries thus delivering their backstroke at the same instant.

Dr. Johnson considers that this explanation raises a more formidable difficulty; for, if "the greater tension of the aorta, in the cases of renal disease under consideration, enable it to overtake the earlier but less rapidly and forcibly contracting pulmonary artery, it seems obvious that, in the normal condition, when the aorta and the pulmonary artery commence their elastic contractions at the same instant, the much greater tension of the aorta, with its thicker and stronger walls, should react upon and close its valves before those of the more feebly contracting pulmonary artery are closed, and the result would be reduplication of the second sound as a constant and normal condition".

And again, Dr. Johnson says, in reference to cases of emphysema; "In these cases, the increased tension of the pulmonary artery consequent on the obstruction in the lungs can never equal the normal tension of the aorta. However great may be the hypertrophy of the right ventricle in cases of emphysema, the thickness of its wall is never equal to that of the left ventricle. If, then, in accordance with Dr. Sibson's theory of asynchronous ventricular contraction, the right ventricle, in consequence of increased tension in the pulmonary artery, complete its contraction later than the left, and thus cause the doubling of the first sound, the closing of the pulmonary valves must inevitably be effected later than that of the aortic, and the second sound must also be doubled. The reverse, however, is the case. The second sound is single in these cases of emphysema, while the first is distinctly reduplicated."

It is difficult to see the point of Dr. Johnson's criticism. In normal conditions, the pulmonary artery is quite as capable of receiving and reacting against the stroke of the right ventricle as the aorta is against the stroke of the left ventricle. To deny this, amounts to the same thing as saying that a certain degree of elasticity reacting against a given pressure is not exactly equivalent to a greater degree of elasticity reacting against a proportionately greater pressure. It may require a cable to lift a ton, but a thread will lift a pound. The resilience of the pulmonary artery is the exact equivalent in force to the resistance offered by the smaller vessels in the lung, and the thickness and strength of its coats are adequate to the tension to which they are subjected. If, at any time, this natural relation between power and resistance become altered from any cause, then the rapidity of recoil must necessarily be altered too. For instance, if the *vis à fronte* in the lungs be heightened or diminished, then the pulmonary artery will be correspondingly more tense or less tense, and react more speedily or less speedily, in the same manner as a spring tightly or slightly wound.

In mitral regurgitation, the blood returning from the lungs meets with an already full auricle, and pulmonary congestion results; the arteries do not discharge their blood with the usual facility, the pulmonary artery becomes distended and its tension at the same time in-