officials, and graced by the presence of the noble and gentle in the land, and of our beloved Queen, who sympathises in the early interest the good Prince Albert took in our future home: nurtured erst in poverty, and restricted in usefulness; now possessed of a princely income, and folding within her wide-spread arms the destitute sick and maimed, whose only passport is suffering and want: yielding formerly her pittance of empirical skill and nursing to the few who sought it; now rich in the memory of so many whose labours within her walls have indelibly allied their names with some of the most enduring achievements of medical science; and (shall I not add ?) proud of association with the imperishable name and work of the self-denying and gentle Nightingale:—such, in brief, is the history of this noble institution; and such are the children she has nurtured, who have repaid her fostering care by shedding a lasting lustre on our profession.

And what is the moral to be laid to heart from this history and these names? Shall *we* shrink in timid indolence from sustaining the weight of reputation thus transmitted to us? Shall we plead, with deprecating humility, that "there were giants in those days", and sit down in listless indifference beneath the laurels they planted? Nay, not so. Let each and every associate in the work, with unselfish and untiring energy, devote himself to his allotted task. Let the substantial token of our affection for our Alma Mater, placed within her chapel-walls, be the pledge and symbol of that harmony of action for the common weal which no jarring note of discord shall disturb. Let private advantage and individual preferment ever yield, as in truth and honesty they should, to the fulfilment of the sacred trust to which every officer of this establishment is pledged when he takes office here. Let this be done earnestly, heartily;—I speak, sir, as one who has journeyed through weary years of discouragement, and is permitted, by grace, and not by right, to tread a few steps within the boundary of the promised land; let this, I say, be done earnestly and heartily, and who shall gainsay the confident anticipation, which it may be, perchance, my happiness to witness, though not to share in, that our ancient foundation, both hospital and school, shall emerge from its temporary eclipse, to shine with more than pristine brightness.

And, standing thus on this border-land, once more beside the old familiar river, from whose slimy bed this stately edifice has arisen as if by magic; surrounded, too, by my trusted colleagues and many familiar faces which remind me of our earlier and happy association as pupil and teacher,—Imagination portrays for me, without an effort, the expanding vista of an illustrious future, worthy of such a history and such a habitation.

ST. GEORGE'S HOSPITAL.

DR. JOHN CLARKE, Lecturer on Midwifery, delivered the Inaugural Lecture. Having given a hearty welcome to the students, Dr. Clarke remarked that they were about to be introduced to a profession which had been truly called "Godlike" in its aim and tendency, unselfish and elevating in its practice, with duties great and laborious indeed, but allowing of pleasures which, if rightly esteemed and viewed, were equally elevating, grateful, and attractive—the grand aim and object of which was, or ought to be, the happiness and comfort of mankind. He was always proud to acknowledge the reciprocity of good feeling which had existed between the teachers and pupils of St. George's Hospital, and he trusted that in the future the dangers of lodging-house life might be avoided, giving comfort to the student without expense, freedom without licence, pleasurable enjoyment without dissipation. Great and well nigh insuperable difficulties, he well knew, lay in the way of compassing such an object, but he did not despair of one day seeing it fairly advanced towards accomplishment. The hospital now possessed three hundred and fifty beds, large enough for all practical and scientific educational purposes; not so large as to weary and confuse the student by its size and number of patients. There was a medical school, composed of buildings most admirably adapted to their purpose, and a museum almost equal in number and variety, and quite equal in the skilful arrangement of anatomical preparations, to any other in London. Having adverted to the multiplicity of the subjects with which the student would have to deal, their varied character, and the novelty of the language in which information was conveyed, the numerous examinations which the students would have to undergo, the lecturer said he could well understand all these perplexities, although in his time of studentship examinations were less numerous, and examiners less of a bore than they were at present. He insisted, though, strongly upon a knowledge of the classical languages, without which it was impossible to understand the nomenclature of disease, or the terms commonly used in medical books.

EPIDEMIC AND SPECIFIC CONTAGIOUS DISEASES:

CONSIDERATIONS AS TO THEIR NATURE AND MODE OF ORIGIN.

Being the Introductory Address delivered at University College, October 2nd.

By H. CHARLTON BASTIAN, M.A., M.D., F.R.S.,

Fellow of the Royal College of Physicians; Professor of Pathological Anatomy in University College, London; Physician to University College Hospital; etc.

WE assemble to-day to inaugurate the work of a new session. Some of you are entering upon a new career, though others will have only to find your way again into what is, I trust, an already established routine of work and duty. The occasion, however, on which we now meet differs little in all essential respects from many others which have passed. Words of advice and counsel have been so often uttered to the new-comers, that I feel it would be comparatively useless again to take up such a well-worn theme. This view is further strengthened by the consideration that the able and judicious exhortations of many who have preceded me on similar occasions are still accessible. Without further excuse, then, gentlemen, I shall pass on to topics of another kind.

In medicine, even more than in other less complex sciences, it is well that imperfectly established general doctrines should be, from time to time, tested by the light of more recently acquired facts. Practice necessarily follows along the paths indicated by theory, and therefore it is in many cases all-important, even from a practical point of view, that true theories should be arrived at. The wider the applications of the theory, the greater is the necessity that it should be sound and based upon the best knowledge of the time.

I have determined to lay before you some considerations touching the nature and origin of epidemic and so-called 'specific' infective diseases. You will be impressed with the vast importance of the subject when you learn that nearly one-fourth of the total number of deaths occurring in Great Britain are due to these affections. As the Registrar-General has aptly pointed out: "Diseases of this class distinguish one country from another—one year from another; they have formed epochs in chronology; and, as Niebuhr has shown, have influenced not only the fall of cities, such as Athens and Florence, but of empires; they decimate armies, disable fleets; they take the lives of criminals that justice has not condemned; they redouble the dangers of crowded hospitals; they infest the habitations of the poor, and strike the artizan in his strength down from comfort into helpless poverty: they carry away the infant from the mother's breast, and the old man at the end of life; but their direst eruptions are excessively fatal to men in their prime and vigour of age. They are emphatically the *morbi populares.*"

No labour is too great, then, no pains should be spared, in order to arrive at just conceptions concerning the origin, nature, and mode of distribution of these scourges of humanity. Deeply impressed with the difficulties surrounding these great problems, and with the enormous importance of strengthening the foundations of our knowledge in respect to them, I was induced rather more than two years ago to take up the investigation of some questions which lay at the root of the whole subject. It seemed to me that no real advance could take place in our power of controlling these diseases until certain other great problems had been settled. What is the real cause of fermentation and putrefaction? Can the organisms which are associated with many of these processes arise *de novo?* These were questions the solution of which seemed to be of the utmost importance to the science of medicine, as well as to the cause of science generally. Thus incited, I resolved to study such much-disputed subjects for myself, with the view of arriving at some independent opinion.

As the results of this work—which to many may have seemed almost wholly unprofessional—have tended to strengthen certain views concerning the epidemic and specific diseases, and to make plain some points which were previously involved in obscurity, I think I cannot do better than attempt a somewhat hasty review of facts, which seem to point conclusively to the necessity of entertaining opinions with respect to some of these diseases which have been hitherto almost wholly ignored.

In the consideration of the nature and causes of disease, we have always to keep in mind two principal sets of factors. Each person exists with structural characters and functional properties which, though

in the main similar to those of his fellow men, have nevertheless individual peculiarities more or less marked. These may be inherent or acquired, habitual or occasional. Amongst these individual peculiarites are ranged what time-honoured custom has called the 'predisposing causes' of disease. On the other hand, man, with his individual peculiarities, lives in a world of change, exposed to the incidence of constantly varying external conditions, which, acting upon individual peculiarities, or upon the average human nature, become, in proportion to their deviation from the usual condition of things, so many 'exciting causes' of disease.

These two sets of factors must never be lost sight of. In the majority of cases, both come into operation in the production of the resultant morbid condition, although in others one or other of them alone may seem to be so potent as of itself to determine the morbid manifestation. The person who inherits a tendency to destructive lung-disease may develope this morbid condition under the influence of exciting causes which would scarcely affect another who inherits no similar weakness (predisposing cause). On the other hand, just as contact with boiling water, owing to the exceeding potency of the 'exciting cause', may determine a lesion of the skin in any individual (quite independently of the existence of a 'predisposition'), so may a person who is born with a weak and unstable nervous system becomes insane or epileptic, independently of the influence of any obvious exciting causes.

All diseases are, in fact, due to altered structure or molecular composition, whether visible or invisible, ascertainable or non-ascertainable. They are no longer regarded as entities. They are due to changes of state in some portion of the body, whereby the vital movement in the part is diverted from a normal into an abnormal mode of activity.

The complicated structure of the human body, and the allocation of specific functions to specific parts, necessitates, and has occasioned, a functional correlation and interdependence. Any disturbance of this normal balance of functions of necessity entails a definite sequence of pathological states and actions. A morbid change in an important organ, if it interferes with the function of the part, rarely exists alone. It sets up other associated effects, whereby the disturbed equilibrium of functions is more or less replaced by a new adjustment.

The effects are often well marked, though very variable, when the disease is one in which any notable alteration in the composition of the blood occurs. Supplying the materials of growth for all parts of the body, any changes in the composition of the blood are found, now to affect one organ and now another most profoundly. Before entering, however, upon the consideration of those diseases in which changes in the nature and quality of the blood form the most important condition of the disease, it will be useful to dwell for a time upon some of the more local pathological changes that occur in the more solid tissues of the body. The two sets of phenomena are closely related to one another. Morbid states which are at first purely local may, after a time, produce general diseases; and a general or constitutional disease frequently entails limited lesions in special parts. A wound or a local inflammatory process may lead to thrombosis, gangrene, and blood-poisoning; just as, following a reverse order, various febrile conditions may cause local lesions—now in one organ and now in another.

There are so-called 'specific growths', just as there are 'specific diseases' of a more general or constitutional character. The life-history of such growths as cancer and tubercle is a subject of great intrinsic interest; though the importance of their study is much enhanced by the fact, which I shall strive to make plain, that their modes of origin and distribution within the body are capable of throwing much light upon the origin and distribution of epidemic and specific infectious diseases amongst the community.

The term 'specific,' as applied to diseases, is confusing, and apt to carry with it a crowd of erroneous notions. Doctrines of 'specificity' have, however, been fashionable in medicine, though they are now growing more and more into disrepute. Thirty or forty years ago, amidst all the jargon concerning homoplastic and heteroplastic, euplastic and cacoplastic growths, would it not have been deemed rank heresy to profess a disbelief in the prevalent notions concerning the unalterable and 'specific' nature of cancer and of tubercle? Here were products altogether peculiar, and not derivable, as it was thought, from the normal tissues of the body—having laws of growth and distribution peculiar to themselves, and an origin which was shrouded in the mystery of a remote past. In view of this doctrine as to the specific and alien nature of the products, how natural was it that an undue stress should have been laid upon the fact that a tendency to the occurrence of such modes of growth may be hereditarily transmitted; how easily explicable is the facile and popular resort to the notion that, where multiple cancerous growths exist, the primary new formation has given rise to a seedling progeny by means of actual cancer 'germs'. Slowly but steadily these views have been undergoing a progressive modification. The anatomical elements of cancer and tubercle are now known to have no special and peculiar characteristics, and they are believed to be as easily derivable from pre-existing tissues as are other non-specific morbid growths. A mere local change in the mode and intensity of pre-existing tissue-changes suffices to engender them. In the case of tubercle, this has been conclusively proved by such experiments as those of Dr. Burdon Sanderson and Dr. Wilson Fox. The latter says:* "M. Villemin's position, that tubercle is a specific disease, producible by tubercle alone, cannot, I think, be held to be true; nor can the method of inoculation be used as a test of the tubercular character of any pathological product; for the four guinea-pigs in whom the vaccine lymph was inoculated, and those inoculated with putrid muscle, and even one beneath whose skin I simply inserted a piece of cotton-thread, and also one of the four in which, following Dr. Sanderson's example, I inserted a seton, presented as intense and typical specimens of the disease as those on whom inoculation had been practised with the most typical grey granulations from the lungs or the meningeal vessels." What has now been (even experimentally) established with regard to tubercle seems also to hold good for such 'malignant' growths as recurrent fibroid, epithelial, and cancerous tumours or infiltrations. Statistics to which Virchow has drawn prominent attention seem to indicate most clearly the potency of 'exciting causes' in giving birth to these growths. Are they not found primarily, with by far the greatest frequency, in situations which are exposed to the action of irritative agencies, either external or internal, normal or abnormal? An amount of irritation which in some persons may lead to chronic inflammation or an hyperplasic overgrowth, will in others suffice to produce one of these so-called 'malignant' growths, even without the aid of any ascertained predisposition. The history of many cases of 'labial cancer', and of that to which chimney-sweeps are liable, speaks almost as plainly concerning the origin of cancer, as the results of experiments on the rodent animals do concerning one of the modes of origin of tubercle.

Is there anything specific in the mode of growth of these products, and in their subsequent distribution within the body of the affected person? Just as an erysipelatous inflammation spreads by gradually inducing a similar morbid action in adjacent parts, so does a cancer or a mass of tubercle grow by a slower extension of the morbid modes of growth. We have no more to do with a kind of implanted something increasing by a multiplicative reproduction in the one case than in the other. In both alike there are deviations from the ordinary modes of growth, which gradually extend to adjacent healthy parts. Neighbouring lymphatic glands become affected in the case of tubercle and cancer-growths, just as they do where simple inflammations exist; and just as the change in the gland in the case of inflammation must be regarded as the result of a mere induced morbid action, rather than as the product of the multiplicative reproduction of a transmitted germ, so is a similar explanation open in the case of cancer and tubercle. Modes of growth which have been primarily induced may be also secondarily induced. The kind of agency, which is at least probably potential where the lymphatic system is concerned, or where particles of morbid growths come into contact with serous† or mucous surfaces, seems almost certainly operative when we come to consider that wider distribution which is occasionally brought about through the vascular system. The potency of the 'exciting causes' are here weakened, and new growths cannot be initiated in distant parts or organs by contact with disseminated particles, unless the 'predisposing causes' are favourable and there is an ability in the part to take on the morbid mode of growth. The action may be similar in kind to that which the transplanted fragment of epidermis exerts upon the ulcerated surface. This becomes covered, not so much by an actual increase of the imported fragment as by the formative changes which its presence incites. A crystal thrown into a mixed solution of saline substances will determine, by its mere presence, the crystallisation of similar materials from the solution; nay, it may determine, in addition, the crystallisation of other products whose modes of aggregation are more or less similar (isomorphous salts). The contact of any number of germinal particles with the tissues of an organ will not produce the formation of a new growth unless the molecular actions (or modes of growth) existing in the part are such as to make the transition an easy one. The mere presence of 'germs', therefore, is not all that is necessary. Cancerous masses may grow into the vena cava, and yet no cancer springs up in the lungs: the stomach may be absolutely infiltrated with cancer, and yet, as I have recently seen, no similar growths may exist in the liver.‡ Detail,

* *On the Artificial Production of Tubercle*, 1868, p. 23.

† See Dr. Sanderson's 'Report on the Communicability of Tubercle by Inoculation' (*Eleventh Report of the Medical Officer of the Privy Council*).

‡ We find, therefore, that in the absence of any apparent predisposition, exciting causes, when potent, are sufficient to determine the occurrence of secondary growths in the same fashion that the primary growth may be determined. As the exciting

however, is needless on such a subject. The distribution of morbid growths throughout the body, as is well known, takes place, if at all, in a manner so irregular in different individuals, as to make the result wholly beyond the possibility of predication. Having to do with a case of syphilis, who would venture to fix upon the internal organs which would become affected? Who can account for all the irregularities observable in the generalisation of tubercle either in man or in the rodent animals? When cancer exists, who will affirm which organ shall be secondarily affected and which not?

The old notions as to the specific nature of cancerous and tubercular products are, therefore, supported neither by the anatomical characters of their growths, by their mode of origin, nor by their mode of distribution; and the known facts concerning the hereditary transmission of a tendency to the formation of such growths is certainly not more explicable in accordance with the old hypothesis than it is by the more modern view. And the history of these local so-called 'specific' growths will be found, as others have in part indicated, to throw much light upon the history of general so-called 'specific' affections, and their mode of distribution through communities, or from individual to individual.*

Epidemic and acute specific diseases have many characters in common; they constitute a family the members of which are united by a certain bond of unity, though at the same time they are in other respects strikingly different from one another. The 'general' character of the symptoms originally gave rise to the notion that these affections were in the main dependent upon changes in the nature and quality of the blood. This view is still the one most commonly entertained and which seems most likely to be true. And seeing that particular sets of symptoms recur with as much definiteness as individual differences of constitution will permit, we have a right to believe that the changes in the blood—however induced and of whatsoever nature they may be—are definite and peculiar for each of these diseases. The successive changes in the blood which are the immediate causes of the phenomena of small-pox, must be quite different from those giving rise to the morbid state known as typhoid-fever. Variable as these several groups of symptoms are amongst themselves in individual cases, yet is there a general resemblance which suffices to maintain the distinctive nature of each affection. In this broad sense they are undoubtedly entitled to rank as 'specific' diseases. They may be presumed to be associated with definite changes in the blood, though we have not a right to infer that such changes of state can only be induced in one way. Many well known chemical changes are capable of being brought about by more than one agency. And just as there is the best reason for believing that cancer or tubercle may be initiated *de novo* by the operation of irritants upon the tissues of certain individuals, and that such growths may subsequently be multiplied within the body by the contact-influence exerted by some of their disseminated particles; so may we suppose, not only that specific substances (contagia) may be capable of initiating specific changes in the blood, but that certain combinations of circumstances may by their action upon the human body entail similar definite changes and states of blood. Having to do with a preverted nutritive activity and mode of growth in a limited area of tissue, cancer or tubercle may make their appearance; whilst, having an altered nutritive activity and set of changes occurring in the blood, this all pervading tissue may lapse into the successive states peculiar to one or other of the specific diseases, and so give rise to the symptoms by which they are characterised. This is by no means a forced analogy. Can cancer or tubercle arise in the individual without any pre-existing 'hereditary taint'? Can the states of blood peculiar to the several specific diseases arise *de novo*, or independently of contagion? These are questions whose import is really similar.†

One of the great and distinguishing peculiarities of these specific diseases is their 'contagiousness.' Although very differently marked in the several affections, this property is as interesting as it is important. The fact of its existence seems always to have had a large share in determining the nature of the general views which have been held concerning these affections. Even in remote periods, by Hippocrates and others, they were commonly compared to processes of fermentation; whilst, since the time of Linnæus, more especially, attention has been often prominently directed to the many apparent similarities existing between the commencement and spread of epidemic diseases, and the "flight, settlement, and propagation of the insect-swarms which inflict blight upon vegetable life."* These analogies were seemingly strengthened by the increased knowledge which gradually arose concerning the various parasitic maladies to which man and the lower animals were liable. Writing in 1839, Sir Henry Holland says in his essay *On the Hypothesis of Insect-Life as a Cause of Disease*, "The question is, what weight we may attach to the opinion that certain diseases, and especially some of epidemic and contagious kind, are derived from minute forms of animal life, existing in the atmosphere under particular circumstances, and capable by application to the lining membranes, or other parts, of acting as a virus on the human body." Now, the fact of the multiplication of the virus within the body was the peculiarity of these diseases which, above all others, caused such an hypothesis to be received with favour. Causes which are specific and which seem capable of self-multiplication—what can such agents be but living things of some kind, plant or animal? This mode of argument was, with many, all powerful. And when, after the discovery of the yeast-plant by Schwann, in 1836, new doctrines concerning fermentation began to prevail, the views of those who believed in the living nature of the specific causes of epidemic diseases were in part strengthened. If all fermentations were initiated by the agency of living organisms, and the specific diseases were comparable to processes of fermentation, then how natural was it that many who were moreover influenced by the other analogies, should be led to imagine that the specific causes of these diseases were also living organisms? Only now, attention became directed to the much lower organisms which are so frequently associated with fermentative and putrefactive changes, instead of to insects "minute beyond the reach of all sense."

Here then is the origin of what in modern times has been termed 'The Germ-Theory of Disease.' Like homœopathy and phrenology, this theory carried with it a kind of simplicity and attractiveness, which insured its acceptability to the minds of many. Now, however, it seems to rest upon foundations only a little more worthy of consideration than those upon which these other theories are based. But, owing to its influence, in combination with the more generally received doctrines concerning the origin of life, there has gradually grown up an unwillingness in the minds of many to believe that these contagious diseases can arise *de novo*. And this being one of those theories which tends to curb inquiry, and to check the possible growth of sanitary knowledge in certain highly important directions, it seems to me necessary to look with scrutinising care to its foundations, not only with the view to the advancement of medical science, but with the direct object of removing all checks which may exist to the growth of sanitary precautions against the origin of these most pestilential affections.

Let us see, then, how far the 'theory' fulfils the conditions which all good theories do fulfil—how far it explains a great number of the phenomena in question, without being irreconcilable with others.

The advocates of the 'germ-theory' have always rested their belief in it, in the main, because they considered that it offered a ready explanation of the increase of the virus of the contagious diseases within the body of the affected person. This increase, they suppose, is not otherwise to be explained. All other considerations brought forward in support of the theory are just as explicable by another supposition. Fully admitting that the occurrence of a process of organic self-reproduction would be a very adequate way of accounting for the increase of the infecting material, we must see whether this mere hypothesis can be reconciled with other characteristics of these affections. In the first place, it may be asked, whether such a process is actually known to constitute the essence of any general diseases. Because, if so, those in which it does occur, ought, in the event of the hypothesis being true, to present a close similarity to the diseases in which such a process is supposed to occur.

Now, there are certain general diseases which do undoubtedly depend upon the presence and multiplication of organisms in the blood and throughout the tissues generally. There is the epidemic and highly contagious disease amongst cattle which is known in this country by the name of the 'blood,' and which excites in man that most

causes are weakened, the growths occur only under the influence of a predisposition (hereditary or acquired) or natural aptitude on the part of the tissue to lapse into the morbid mode of growth.

* See Dr. Morris, *On Germinal Matter and the Contact-Theory*, 1867.

† This double mode of causation is perfectly familiar to the chemist. Particular chemical changes may occur under the influence of certain general determining conditions, which at other times (in the absence of these conditions) may be even more easily initiated by a single specific cause. The introduction of a crystalline fragment into a saline solution, and its determination of the crystallisation of all the isomorphous salts contained in the solution, seems to be exactly comparable with the "contagious" origin of diseases. But, under the influence of certain favouring conditions, crystallisation may occur without the contact of a crystalline fragment—the process may be 'spontaneous' in the same sense that the occurrence of the blood-change may be 'spontaneous.'

* Sir H. Holland's *Medical Notes and Reflections*, 2nd edition, 1840, p. 584. On the following page, the same author writes:—"Connected with these facts is the observation, seemingly well attested, that the cholera sometimes spreads in face of a prevailing wind, and where no obvious human communication is present—a circumstance difficult, if indeed possible, to be explained, without recourse to animal life as the cause of the phenomenon. No mere inorganic matter could be so transferred, nor is vegetable life better provided with means for overcoming this obstacle." Whilst on the preceding page, the "animal species" had been admitted to be "minute, beyond the reach of all sense."

dangerous morbid condition known as 'malignant pustule.' The researches of M. Davaine* and others have revealed the fact that this disease is essentially dependent upon the presence and multiplication of living organisms, closely allied to *Vibriones*, in the blood of the animals affected, and that similar organisms are also locally most abundant in the contagiously incited 'malignant pustule' of man. Unless this latter be destroyed in its early stages, the contained organisms spread throughout the body and the disease speedily proves fatal. Of late, moreover, attention has also been called† to Pasteur's researches on the subject of the very fatal epidemic which raged for fifteen years amongst the silkworms of France. This affection, known by the name of *pébrine*, is dependent upon the presence and multiplication of peculiar corpuscular organisms, called *Psorospermiæ*, in all the tissues of the body. Both these general parasitic diseases are highly contagious; both are contagious by means of organisms; and in both the virus does increase by self-multiplication within the body of the animal affected. What more suggestive evidence could there be as to the truth of the 'germ-theory,' say its advocates, than is supplied by the phenomena of these two diseases? Undoubtedly the evidence is irrefragable as to its applicability to these particular diseases; but then comes the question, whether they are comparable with the other affections to which the germ-theory is sought to be applied. And this question must decidedly be answered in the negative. These parasitic diseases are sharply distinguished from the others by the fact of their almost invariable fatality. Creatures or persons once affected in this way are, under ordinary circumstances, thenceforth on the road to more or less immediate death. Happily, however, no fatality of this kind is characteristic of even such highly contagious diseases as scarlet fever and small-pox, or any other of the maladies with which parasitic organisms cannot be shown to be associated.‡ But if living things were really present as causes of these diseases, then most assuredly ought they to conform to that fatal type which is almost inseparable from the notion of a general parasitic disease, and which we find exemplified by the course of *pébrine*, the 'blood,' and 'malignant pustule.'§ The fact then, that the general tendency in the acute specific diseases, is undoubtedly towards recovery rather than towards death, speaks strongly against the resemblance supposed to exist between them and the parasitic affections alluded to, and also against the hypothesis that they are dependent upon the presence of self-multiplying germs within the body. Such germs, when present, would be sure to go on increasing until they brought about the death of their host.

These considerations alone should suffice to inspire grave doubts as to the truth of the 'germ-theory.' And such doubts may be reinforced by many others. Thus, the several affections being distinct from one another, this theory demands a belief in the existence of about twenty different kinds of organisms never known in their mature condition, but whose presence as invisible, non-developing germs is constantly postulated, solely on the ground of the occurrence of certain effects supposed to be otherwise incapable of occurring. That, if existent, they are no mere ordinary germs of known organisms is obvious, because the presence of these has again and again been shown to be incapable of producing the diseases in question. Mr. Forster says,‖ "There is not perhaps on the face of the earth a human creature who lives on coarser fare, or to a civilised people more disgusting, than a Kalmuck Tartar. Raw putrid fish or the flesh of carrion—horses, oxen and camels—is the ordinary food of the Kalmucks, and they are more active and less susceptible of the inclemency of the weather than any race of men I have ever seen."¶ It has, moreover, been frequently demonstrated, that the organisms of ordinary putrefactions may be introduced even into the blood of man and animals without the production of any of these specific diseases.** Yet is the 'Antiseptic System' of treatment (good as it may be, irrespective of the germ-theory on which it has been based) pressed upon our attention on the assumption that the germs of putrefaction and the germs of disease are living organisms similar in nature. The strange persistency with which this view is advocated is not a little surprising when it entails the obvious contradiction that germs which do, under all ordinary circumstances, develop into well known organic forms, should, when concerned in the production of the diseases in question, induce all the effects supposed to depend upon their prodigious growth and multiplication, and yet never develop, never become visible. And whilst *Bacteria* and other organisms with which the unknown disease-germs are compared, flourish and reproduce in the much-vaunted germ-killing carbolised lotions,* still carbolic acid continues to be recommended solely on account of its germ-killing powers, and the theory on which the practice is based is thought to derive support from the results obtained by the use of this agent. Surely no theory could be weaker on which to base a successful method of treatment; and if, as its distinguished originator says,† its general acceptance is principally hindered by the "doubt of its fundamental principle," then I would deliberately say that the blame, if any, cannot fairly be said to lie with those "who have opposed the germ-theory of putrefaction." The 'Antiseptic System' of treatment needs no support from a 'germ-theory;' it can be surely and unassailably based upon the broader physico-chemical doctrines of Liebig.‡

The last blow, however, seems given to the 'germ-theory' of disease, when we are told that the blood and the secretions in sheep-pox are not infective, though this disease is most closely allied to, and even more virulently contagious than, human small-pox. If germs had existed in this general disease, and their multiplication was the cause of it, then most assuredly would they have existed in the blood and in other fluids of the body; and yet, as Dr. Burdon Sanderson tells us,§ "In sheep-pox all the diseased parts are infecting while no result follows from the inoculation either of the blood or of any of the secretions; the liquid expressed from the pulmonary nodules has been found by M. Chauveau to be extremely virulent—certainly not less so than the juice obtained from the pustules." Now, although in other of these diseases the blood does undoubtedly exhibit infective properties, still the ascertained existence of even one exceptional case amongst maladies so contagious as sheep-pox, seems to me absolutely irreconcilable with the truth of the 'germ-theory,' more especially when this theory was started principally to explain the phenomena of such highly contagious diseases.‖

Rejecting the 'germ-theory,' then must we confess our absolute ignorance on the subject (a course always better than the adoption of an untenable theory), or are there facts to guide us to another view as to the nature and origin of the poisons of these infectious diseases?

It surely is a vice in argument to suppose that the increase of the virus within the body in these diseases is only to be accounted for by a

* See *Comp. Rend.*, 1864 and 1865.

† *Nature*, 1870, No. 36, p. 181.

‡ Doubtless there are other general parasitic diseases amongst animals. In almost all the specific diseases to which man is liable, however, I have invariably failed to discover any trace of organisms in the blood. The experience of many other observers has been similar to my own in this respect.

§ See papers by Dr. Wm. Budd, in BRITISH MEDICAL JOURNAL, 1863.

‖ See *Med.-Chirurg. Rev.*, 1854, vol. xiii, where the supposed connection of diseases with processes of putrefaction is ably considered by the late Dr. W. Alison.

¶ The *Bacteria* which are sure to be abundant in such food cannot, therefore, be the much talked-of 'disease-germs.' Such a diet is, of course, by no means recommended, and could probably only be borne in certain climates by persons who lead a very active life. Epidemic diseases are frequently most fatal when they once break out amongst a people whose diet is of this kind. See Dr. Carpenter, in *Med.-Chirurg. Rev.*, 1853, vol. xi, p. 173.)

** See, amongst others, Davaine in *Compt. Rend.*, Aug. 1864, and E. Semmer in Virchow's *Archives*, 1870. Dr. Lionel Beale is well aware of this fact, and he, accordingly, whilst adhering to the "germ-theory," promulgates it under a new form. He says (*Monthly Micros. Jour.*, Oct. 1870, p. 205):—"Concerning the conditions under which these germs are produced, and of the manner in which the *rapidly multiplying matter* acquires its new and marvellous specific powers, we have much to learn, but with vegetable organisms the germs have nothing to do. They have originated in man's organism. Man himself has imposed the conditions favourable to their development. Man alone is responsible for their origin. Human intelligence, energy, and self-sacrifice may succeed in extirpating them, and may discover the means of preventing the origin of new forms not now in existence." This is undoubtedly a very much less objectionable form of the 'germ-theory,' though much additional evidence would be needed before we could accept the view that contagious diseases are due to the rapid multiplication of the contagious particles within the body of the creature affected. The non-contagiousness of the blood is as irreconcilable with this as with the other form of the 'germ-theory.'

* See *Modes of Origin of Lowest Organisms*, 1871, p. 85. And in a recently published paper "On the Relative Powers of Various Substances in Preventing the Generation of Animalcules or the Development of their Germs", Dr. Dougall says, "If, as is alleged, germs are the source of putrefaction, then the strongest preventives *must* be the best antiseptics, and *vice versâ*. Now, as seen in the table, carbolic acid occupies a very mediocre place as a preventive, therefore it is legitimate to conclude that it stands no higher as an antiseptic" (p. 13).

† BRITISH MEDICAL JOURNAL, August 26th, 1871, p 225.

‡ These doctrines do not seem to have been adequately grasped by Prof. Lister. Fragments of organic matter are believed by Liebig to be capable of acting as ferments; he, however, holds that their potency is deteriorated by heat almost as much as are the qualities of living ferments. The experiments with boiled fluids in bent-neck flasks, therefore, upon which Prof. Lister so strongly relies in proof of the germ-theory, prove absolutely nothing as between the two theories of fermentation of Liebig and of Pasteur. Amongst the atmospheric particles there are sure to be dead ferments in the form of mere organic fragments. Now the doubt that previously existed was, as to whether they could initiate fermentation and putrefaction, or whether the presence of living germs was absolutely essential. In the experiments with bent-neck flasks, both fragments and germs must be simultaneously excluded or admitted to the fluids. Professor Lister's readers might suppose that Liebig had no objection to his ferments being boiled, and that the issue lay between the relative efficiency of oxygen and living germs. (See Gerhardt's *Chimie Organique*, t. iv, p. 545.)

§ Report "On the Intimate Pathology of Contagion", in Twelfth Report of Medical Officer of Privy Council.

‖ Inoculation with the blood of a person suffering from measles has also in several cases failed to reproduce the disease. The different severity of small-pox taken in the ordinary way, and that induced by 'inoculation' of the matter of a small-pox pustule, is also quite inexplicable in accordance with the 'germ-theory,' although both facts are quite reconcilable with the view about to be mentioned.

process of organic reproduction. The power of self-multiplication by division is peculiar to living things, but an actual increase of any substance may be by a process of growth alone, without the aid of self-multiplication. Growth, however, takes place in not-living as well as in living matter; and, fundamentally considered, it means only increase in the quantity of the substance which grows, whether we have to do with the substance of a muscle, with a crystal, or with a complex organic poison. Liebig says: "a substance in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid." And in illustration Sir Thomas Watson writes: "Thus the virus of small-pox (which virus is formed out of the blood) causes such a change within the blood as gives rise to the reproduction of the poison from certain constituents of that fluid: and whilst the process is going on the natural working of the animal economy is disturbed; the person is ill. The transformation is not arrested until the whole of that ingredient in the blood which is susceptible of the decomposition has undergone the metamorphosis."* The specific poison (contagium) does not, however, seem to be immediately reproduced in the blood of the person affected: rather, a set of changes are set up in the blood which ultimately lead to the evolution of such a poison in some parts of the body, either limited or widely distributed; so that, as Mr. Simon says,† "Bowels, skin, kidney, tonsils, are the favourite resorts of the several fever-poisons just us they are the surfaces by which naturally the organic waste of the several tissues is eliminated."‡

There are many organic poisons which undoubtedly produce spreading changes in the blood. Writing from Australia, Prof. Halford says § :—"In fatal cases of snake-poisoning, whether in this colony, India, America, or Africa, it may be stated as a rule, with few exceptions, that the blood loses its power of coagulation and becomes thinner and poorer." After the death of the person "it greedily absorbs oxygen when exposed to the air, and it absorbs it more than unpoisoned blood." Though the precise changes are quite unknown, its constitution is obviously profoundly modified. ‖ The rapidity with which the symptoms are produced in the case of snake-bite do not in the least prevent our comparing the effects of snake-poison with those of the contagious zymotic diseases. In some of these the effects have been even more rapidly produced. Speaking of 'the Black Death,' which raged in the fifteenth century, Hecker tells us that, "Many were struck as if by lightning, and died upon the spot, and this more frequently among the young and strong than the old." Again, Dr. Aitken says: "When the cholera reached Muscat, instances are given in which only ten minutes elapsed from the first apparent seizure before life was extinct; whilst instances of death taking place from cholera-poison in two, three, or more hours, are well known to be extremely common.

> "Its effect
> Holds such an enmity with blood of man
> That, swift as quicksilver, it courses through
> The natural gates and alleys of the body."

The action of known poisons, whether animal or other,¶ upon the blood and system generally, may therefore be compared with those unknown poisons of the zymotic diseases. The great difference is this. The changes in the blood induced by snake-poison are not such as to terminate in the elaboration of a similar poison in any part of the body of the person bitten, whilst the bite of a mad dog does lead to changes which culminate in the reproduction of the hydrophobic poison; and similarly with those of scarlet-fever or small-pox—contact with these poisons entails changes which result in an enormous production of similar poisons. There is probably no fundamental difference between the two sets of cases. The malarial *miasm* of intermittent fever, and the poisonous state of the blood which leads to the production of rheumatic fever,* as a rule produce effects which are more strictly comparable with those of snake-poison, though there is reason to believe that these diseases may merge into other affections which are admitted to be contagious, as when intermittent or remittent fevers develop in warmer climates under the aggravated form of contagious yellow fever. In this way may the gulf be bridged which seems to separate the effects in snake-bite from those of hydrophobia. As Liebig pointed out, what occurs in the former case may be compared to the action of yeast upon a simple solution of sugar, and in the latter to the action of the same ferment upon a solution of sugar which also contains nitrogenous materials at the expense of which the ferment itself may grow. Thus, then, just as the presence of a crystalline fragment may determine the synthesis of its elements† from a solution in which they are contained, and as the living ferment may bring about that much more complex synthesis which occurs during its growth, so may an organic poison having an intermediate molecular complexity by its contact with the fluids or mucous surfaces of the body, be enabled to determine a series of changes leading to the synthesis of a similar poison.‡

If we find that amongst this class of general or specific diseases some are non-contagious, others only slightly so, whilst the remainder present increasing degrees of contagiousness; that diseases, which sometimes or under some conditions are non-contagious, under others become contagious; and lastly, if we find that even the virulently contagious poisons of some diseases are undoubtedly capable of arising *de novo*, then have we certain reason for the supposition, that the contagiousness or non-contagiousness of particular general diseases is a quasi-accidental feature, and that there is no real difference in kind between the poison of a serpent which does not occasion the production of a similar venom, and the poison of a mad dog which does seem capable of undergoing self-multiplication.§

Let us take a brief survey of some of the facts which are known concerning these specific infective diseases.

Glanders is an affection which is in many respects analogous to syphilis, and is almost, if not quite, as highly contagious a malady. Both these diseases, too, form extremely interesting links between such specific tissue affections as cancer and tubercle, and such infective blood-diseases as small-pox and scarlet fever. Like the former, they are apt to involve the presence of morbid growths scattered in different parts of the body, though, like the latter, they are commonly spread by contagion from individual to individual. However little we may

* Ch. Robin says, in his *Végétaux Parasites*, 1853, p. 376:—"On a confondu un phénomène grossier et physique de transport de végétal d'un sol sur un autre plus ou moins favorable avec la question de contagion. Celle-ci est au contraire caractérisée par une modification moléculaire lente des substances organiques se propageant de proche en proche, sous l'influence du contact d'autres substances organiques présentant déjà elles-mêmes une modification analogue. S'il y a quelque chose de contagieux dans cette transplantation, c'est la putrefaction des substances azotées qu'on transporte, et elles déterminent dans les mucus sains un altération analogue à celle qu'elles ont éprouvée. Mais il n'y a rien là qui appartienne en propre au végétal et doive lui être attribué." See also pp. 307-309.

† Lectures on Pathology.

‡ A similar view has been advocated on more than one occasion by Dr. B. W. Richardson. He says (*Medical Times and Gazette*, November 5th, 1870, p. 539):—"A person suffering from a communicable disease is poisonous precisely as a cobra di capello is poisonous—that is to say, he is producing by secretion an organic poison, which, if it comes into contact in the right way with a healthy person, will reproduce disease." See also *Trans. of Epidem. Soc.*, vol. i.

§ On the Treatment of Snake-bite.

‖ Dr. Richardson has ascertained that, unlike vaccine lymph, the snake-poison becomes weakened by dilution; and similar observations have been made by others. The 'particulate' nature of the poison in vaccine lymph, which has been demonstrated by the skilful experiments of Chauveau and Sanderson, is a condition in which it very probably exists in many other contagia.

¶ That such effects are in no way necessarily dependent upon the fact that the poison may contain living elements, we may imagine from the influence of prussic acid, morphia, etc. Nay, more; I have had frequent personal experience of the fact that a spasmodic and catarrhal affection somewhat resembling hay-fever may be produced by emanations from certain Nematoid worms, even after they had been preserved for two or three years in spirits of wine, and macerated for a time in calcic chloride (see *Philosoph. Transact.*, 1866, p. 583). Effects somewhat similar, though not so lasting, are produced upon some persons by the smell of powdered ipecacuanha.

* I agree with Dr. Richardson in thinking that this affection really belongs to the zymotic class of diseases. Dengue seems to be a slightly contagious affection somewhat intermediate between rheumatic and scarlet fever. The 'sweating sickness' of the middle ages was considered to be an aggravated epidemic form of rheumatic fever, and so also with the various forms of 'miliary fever.' The contagiousness of these diseases, according to Hecker, seemed to vary in different epidemics.

† Which, as Prof. Graham showed, really exist separately in the solution, since they are separable by dialysis.

‡ Sir Thomas Watson says, in explanation of Liebig's doctrine, "In order, then, that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system which results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's wort) should be present in the blood, and this ingredient must have a definite relation to the given poison. And he subsequently adds, (*Principles and Practice of Physic*, vol. ii, p. 790):—"This theory of Liebig's offers, then, an intelligible explanation of the curious facts that certain contagious disorders furnish a protection, temporary or permanent, against their own return; that they have a tolerably definite period of incubation, and run, for the most part, a definite course; that some persons are less susceptible than others of the influence of these animal poisons, or not susceptible at all; and that the same individual may be capable of taking a contagious disease at one time, and not at another." The same facts, it may be observed, are almost inexplicable in accordance with any rational rendering of the 'germ-theory.'

§ In snake-bite the symptoms are due to the effects of an habitually poisonous secretion which has a most rapid and deadly action; whilst hydrophobia is due to the effects of an occasional quality of the salivary secretion. This occasional quality, characteristic of rabies, is generally admitted to arise independently in the dog, and yet the poisonous salivary secretion sets up a similar disease in other dogs which may be bitten. Nay, more, this affection at times prevails in an epidemic fashion. Dr. Gavin Milroy says (*Transactions of the Epidemiological Society*, vol. i, p. 173): "Hillary, in his work on *Barbadoes*, described *rabies* as common in the West Indies. Moseley, having never seen a case of it for a series of years, doubted the correctness of the statement; but, in 1783, it unexpectedly broke out with violence at Hispaniola, and also in Jamaica, where it prevailed from June to the following March. Dogs were seized with it that had no communication with others, and some dogs not brought on shore went mad in the harbours of the island. *On Tropical Diseases*, 1803." There are those, however, who still doubt whether rabies is capable of arising *de novo*. (See Art. in Reynolds's *System of Medicine*, vol. i.)

know concerning the actual origin of syphilis, no doubt seems to remain in the minds of most of those who have studied the question, as to the possibility of producing glanders in the horse. After referring to the highly contagious nature of the affection, Dr. Gavin Milroy says, on this subject: "It is also very generally admitted that glanders is a generable as well as a propagable disease; and that it is extremely apt, especially in some seasons, to develope itself in foul, unventilated stables, or (as was often the case during the continental war) in the filthy between-decks of crowded transports."*

Here too may be mentioned such affections as purulent ophthalmia, gonorrhœa, croup, and diphtheria—the two former at least yielding local secretions which are virulently contagious, although assuredly they are not necessarily produced by specific infecting agents. The secretions of croup are only slightly contagious, though those of diphtheria often exhibit this quality to a more marked degree. Yet, even this last is generally regarded as an aggravated form of angina, which is apt to prevail occasionally as an epidemic affection.†

Turning now to the infective diseases of a more general character, we find a group of the utmost importance to the surgeon and to the obstetrician—between the members of which there is the closest alliance and even interchangeability—and concerning the possibility of whose *de novo* origin no surgeon or physician can entertain any reasonable doubt. These are erysipelas, puerperal fever, pyæmia, and hospital gangrene—fearful affections, but all only too easily producible.‡ Not to mention idiopathic erysipelas, which is also a contagious affection,§ how frequently does an ordinary inflammation assume an erysipelatous character in certain individuals—more especially in those who are the subjects of renal disease: and yet hospital gangrene, pyæmia and puerperal fever are but different modes in which this morbid process repeats itself in certain constitutions and under certain conditions. How easily erysipelas is set up in some persons by the mere contact of a wounded surface with the fluids of a dead body, is well known; and how fatal and frequent may be the attacks of puerperal fever due to the same cause has been fully established by melancholy experience at the Vienna Lying-In Hospital. Yet that such effects are in no way attributable to, or comparable with, ordinary processes of putrefaction is also a matter of absolute certainty. Again, we know that in certain cases where symptoms of poisoning result from eating mackerel or some shell-fish, these effects are not due to the putrescence of such articles of food. And similarly in reference to the many cases in which symptoms of poisoning have been produced in Germany by sausages, we learn from Liebig that "the sausages are poisonous only *at a particular stage of decay*, and cease to be so when putrefaction is advanced so far that sulphuretted hydrogen is evolved; the central part being often poisonous whilst the surface is wholesome." There seems every reason to believe that in the changes which may take place in these substances, short of actual putrefaction, a "peculiar poisonous principle is evolved." And so in certain cases, where an *unhealthy process* of suppuration occurs, poisonous products may be generated in a wound, whose absorption into the system is capable of bringing about those general symptoms of blood-poisoning which are characteristic of puerperal fever or of pyæmia.‖

If we refer, now, to the diseases which are most frequently endemic or epidemic in nature, we find them presenting very different degrees of contagiousness. The communicability of some of these affections seems to vary in different epidemics, and also, even during the same epidemic, in different places. Independently of this individual variability, however, the diseases, looked at as a series, present remarkably different degrees of contagiousness. In some this property seems to be absent, whilst in others it presents a most sure and deadly virulence.

Ordinary intermittent and remittent fevers, are, like rheumatic fever, endemic rather than epidemic, and may, as we know only too well, be developed in almost any individual, and especially in a new comer who ventures into a malarious district. All attempts to connect malaria with the presence of organisms have signally failed; these fevers, indeed, prevail in the most variable sites, and are by no means restricted to marshy districts. Dr. Fergusson says: "The first time I saw intermittent and remittent fever become epidemic in an army was in 1794, when, after a very dry and hot summer, our troops, in the month of August, took up an encampment at Rosendaal in South Holland. The soil was a level plain of sand with perfectly dry surface, where no vegetation existed or *could* exist, but stunted heath-plants. On digging, it was universally found percolated with water to within a few inches of the surface, which, so far from being at all putrid, was perfectly potable in all the wells of the camp." These diseases, under all ordinary circumstances, are most certainly not contagious, and yet all the best authorities on the subject are agreed that yellow fever, which is capable of being propagated by contagion *in circumstances favourable to its extension*, is but an aggravated form of remittent fever, as it occurs in warm countries.* This gradual conversion of a non-contagious into a contagious form of disease, combined with the limitations as to the nature and degree of its contagiousness, which the widest experience compels us to accept, are facts of the utmost importance for those who seek to learn the nature and origin of the contagious influence. And, as almost similar limitations have to be accepted with regard to the contagiousness of cholera and dysentery, it is of the greatest importance to ascertain the nature of these limitations. Facts abound, and speak most plainly to those who will read them dispassionately. Referring to the prevalence of yellow fever on the coast of Brazil, Dr. Mc Kinlay† wrote:—"Almost every person who joined the *Vestal* during the prevalence of fever was affected by it; but no person leaving her, under the disease, communicated it to another, in another place." That is, as he afterwards explained, so long as the affected persons went to a healthy place in which the disease was not prevailing.

Facts of this kind are most notorious; and, when an epidemic of yellow fever occurs on land, it has often been found that there are boundaries at no great distance from the tainted district where the disease has not, and to which it will not, spread.‡ The value of migration from the affected region is a matter of history, and the circumstances which have revealed it have all the value of experiments conducted upon a large scale. "During the epidemic of 1800, at Cadiz, 14,000 persons left that city when the disease became suspected. These people fled to the country, where they remained free from the epidemic; while of the 57,499 who remained, 48,520 were attacked, of whom 6,884 lost their lives." And, again we read§:—"It was calculated that from Barcelona, in 1821, about 80,000 persons fled; and, except some who departed with the disease already upon them, or who were on the eve of being attacked, all remained exempt from the reigning malady." But, when individuals from an infected district pass into a region where conditions prevail which are favourable to its spread, or which are themselves capable of engendering typhus or other fevers, then yellow fever appears to be a contagious disease. A good illustration of this is supplied by Sir Gilbert Blane.‖ He says:

"On the 16th of May, 1795, the *Thetis* and *Hussar* frigates captured two French armed ships from Guadaloupe, on the coast of America. One of these had the yellow fever on board; and, out of fourteen men sent from the *Hussar* to take care of her, nine died of this fever before she reached Halifax, on the 28th of the same month, and the five others were sent to the hospital sick of the same distemper." So far, there is nothing whatever unusual; but what follows is a good example of the

* *Transactions of Epidemiological Society*, vol. i, p. 175. The same author adds, however: "The converse of the proposition is happily no less true; experience having abundantly shown that its development may be controlled even to absolute prevention by the same simple sanitary rules, the observance of which has banished from our jails and workhouses the disease to which I shall next refer, viz., typhus."

† After referring to the exaggerated notions which were at one time entertained with regard to the contagiousness of diphtheria, Mr. J. Netten Radciiffe (*Trans. of Epidem. Soc.*, vol. i, p. 332) says:—"Subsequent observation has shown, moreover, that contagion plays but a very limited part in the epidemic extension of diphtheria. . . . The times of occurrence of the forerunners of the epidemic, the scattered and disconnected centres of manifestation, and the slow growth, extending over a period of several years, would seem to point to developing causes, slowly originating over the whole, or the greater portion of the surface of the kingdom, and culminating more rapidly in the southern than in the northern districts." Mr. Radcliffe adds, "If we would successfully study the etiology of the epidemic, we cannot disconnect that study from the observation of allied affections prevailing contemporaneously." An examination of the statistics relating to the prevalence, during the same period of scarlet fever, croup, thrush, quinsey, and laryngitis lead to the conclusion that "*all the affections allied to diphtheria prevailed epidemically contemporaneously with diphtheria.*"

‡ Sir William Jenner says (*Practical Medicine of To-day*, 1869, p. 56), "We know that the zymotic element which produces contagious pyæmia may be generated in the frame of man *de novo*. A most important problem to be solved is that of the spontaneous origin of other zymotic diseases."

§ Sir Thomas Watson's *Practice of Physic*, vol. ii, p. 917.

‖ Just as contact with particular compounds (*e.g.*, cadaveric poison) seems to favour the production of such poisonous compounds in a wound, so may the presence of carbolic acid tend to hinder those poison-generating changes which are otherwise apt to occur in some wounds. The success which attends the use of carbolic acid may, therefore, be quite independent of its germ-killing powers, or even of its ability to arrest putrefactive processes in general. It has been shown, indeed, to act quite differently with different fermentable fluids. (*Modes of Origin of Lowest Organisms*, 1871, pp. 81-85, and Dr. Dougal's pamphlet, p. 6.)

* "On Marsh Fever", in *Edinburgh Philosophical Transactions*, vol. ix, p. 274. And yet, concerning this disease, Dr. Milroy says:—"That yellow fever is constantly making its appearance, at intervals more or less distant, in various tropical countries, quite independently of any suspicion of antecedent importation, just as malignant cholera does in Hindostan, does not admit of doubt. In some seasons, from causes which we have hitherto failed to discover, it exhibits a much greater diffusion and migratory power than in other seasons. . . . Malignant cholera is much more diffusible and migratory than yellow fever; few regions of the world have escaped its assault."

† *Monthly Journal of Medical Science*, November 1852, p. 425.

‡ See *Med. Chir. Rev.*, 1854, vol. xiij, p. 338.

§ *Second Report on Quarantine*, etc., p. 202.

‖ *Diseases of Seamen*, p. 606.

kind of testimony which exists as to the occasional contagiousness of the disease. "Part of the prisoners," we are told, "were removed on board the *Hussar*, and, though care was taken to select those seemingly in perfect health, the disease *spread rapidly in that ship* (formerly healthy),* so that near one-third of the whole crew was more or less affected by it." Now, these facts which are recorded concerning yellow fever, are very comparable with what would have to be stated concerning dysentery. This also is "a disease liable to be engendered at any time by foul, damp air, and the use of bad food and drink, and which, at first, shows little, if any, power of communicability, but which, as cases multiply, and when the sick and the well are congregated together, unquestionably acquires contagious properties".† The same power of arising *de novo*, and the same absence of contagiousness, except under the influence of favouring circumstances, seems to distinguish the direst of our modern epidemics—cholera. As Dr. Gavin Milroy says: "The whole history of the disease proves that contagion plays a very small and subordinate part in its diffusion; and nowhere has the attempt to exclude it by barring intercourse with places already affected succeeded in protecting a country from its invasion." Out of the area in which it habitually exists as an endemic disease, malignant cholera does not seem to be directly generable "by any known or appreciable conditions of local insalubrity, however much these conditions may favour its development or aggravate its intensity when it is once present, or is close at hand." The spread of the disease from its endemic site seems undoubtedly to be influenced by obscure atmospheric or other unknown conditions, comprised under the term 'epidemic influence'. Sir William Jenner asks: "What is the specific cause-relation between cholera and choleraic diarrhœa, and between severe summer diarrhœa and choleraic diarrhœa? Is cholera, in the form of choleraic diarrhœa, always amongst us?" And Mr. Macnamara, in part, replies from Calcutta that "cholerine is simply a modified form of Asiatic cholera, and is capable of engendering this more deadly form of the disease in other people by means of the dejecta." He says, also: "I know that several of the leading practitioners in this part of India are of opinion that cholera is 'a something generated in the bodies of those attacked by it, quite independently of all external influences.‡'"

Turning, now, to such affections as influenza and parotitis, these also are diseases which present various degrees of contagiousness, and are frequently epidemic in their mode of onset. Both are believed to be capable of arising *de novo*§, although the spread of influenza is undoubtedly promoted by unknown 'epidemic influences.' Sir Thomas Watson says: "The visitation is a great deal too sudden and too widely spread to be capable of explanation" by mere contagion. He adds: "It has been observed to occur also at the same time on land, and on board different ships, which have had no communication with the shore nor with each other.‖"

If, however, we direct our attention to such affections as typhoid fever, relapsing fever, typhus, the plague, and cerebro-spinal meningitis, we meet with a group in which different degrees of contagiousness are presented, but concerning the origin of which *de novo*, or, independently of contagion, there can now be little doubt. Although this is a doctrine which has long been supported by many who have paid most attention to these diseases, it has of late years been much enforced and strengthened by the investigations of Dr. Murchison. The contagiousness of typhoid or enteric fever is very low; and, as Dr. Murchison says, "although enteric fever is, under certain circumstances, communicable, a large number of cases commence under circumstances which appear to exclude every possible source of contagion. The truth of this observation is almost universally admitted; and it is, therefore, necessary to search for some other cause of the disease than contagion". An enormous amount of evidence tends to show that emanations from sewage and from *some forms* of putrefying matter are capable of exciting the disease in those who are favourably predisposed, although in other cases it seems to be more directly communicated by means of drinking water contaminated by sewage containing the dejections from a typhoid patient.*

Relapsing fever and typhus present many points of resemblance: both are essentially epidemic diseases; both are undoubtedly contagious. They generally occur during seasons of great scarcity, and they prevail more widely amongst the poorest class of the population. Overcrowding and defective ventilation, especially when associated with bad and insufficient food, "not only favour the propagation of typhus, by concentrating the emanations from the sick, but appear to be capable of generating the poison *de novo*." After alluding to the mode in which epidemics commence, Dr. Murchison adds: "I would allude in particular to an epidemic of true typhus which occurred in 1843, at Broulhac, an elevated village in the Canton de Puy, in France. Most of the inhabitants were in a state bordering on starvation; and the first cases were traced to a house where there was overcrowding and no ventilation. It is impossible to conceive that the disease was imported, inasmuch as true typhus was not prevalent at the time in any other part of France.†" With regard to relapsing fever, on the other hand, it has been shown‡ that this (which is essentially *the* famine fever) is more dependent upon extreme starvation than upon overcrowding. Although it is not always easy to separate these two causes, it has been ascertained that in mixed epidemics of typhus and relapsing fever, relapsing fever is most prevalent towards the commencement, and typhus towards the close, of the outbreak. Then, again, we know that relapsing fever is not confined to large towns, but that it also decimates the starving inhabitants of country places.

Cerebro-spinal meningitis is believed by many to be only a modified form of typhus§, though this is more certainly the case with the plague, in which the typhus poison is evolved in its severest form. Undoubtedly contagious, though formerly believed to be infectious, in the very highest degree, Dr. Gavin Milroy says: "The whole history of medical opinion on the subject of the plague affords one of the most remarkable instances on record of fanciful speculation taking the place of sober and careful inquiry." And then he adds: "That the plague has frequently become developed *de novo*, and quite independently of any antecedent infection, cannot be doubted. The recent outbreak at Bengazi, on the Barbary coast, only confirms previous testimony; and as this outbreak occurred after many years' disappearance of the pestilence in that place, as well as throughout Egypt and Turkey generally, no other interpretation is possible. Then, as on many other occasions, the disease sprung up

* That is, free from yellow fever.

† Dr. Gavin Milroy, *loc. cit.*, p. 176.

‡ *A Treatise on Asiatic Cholera*, 1870, p. 327. It is only fair to add, however, that Mr. Macnamara does not give his assent to this view. He is a firm believer in the communicability of cholera. He admits that 'sporadic cholera' is easily generable *de novo*, and that 'cholerine', from which it is often quite indistinguishable, is capable of giving rise in others to malignant cholera; and yet he wishes to maintain the distinctiveness of the latter form of the disease. Other affections also exhibit different degrees of contagiousness, and it would seem to us that 'sporadic cholera', which is easily generable in certain parts of India, cannot be really distinct from 'cholerine.'

§ Dr. Morris says (*Germinal Matter and the Contact-Theory*, 1867, p. 70):—"A curious contagious disease is recorded by Huxley to have arisen on board the surveying vessel *Rattlesnake*, characterised by glandular and diffuse cellular inflammation, by common and phlegmonous erysipelas, and by *mumps*."

‖ *Principles and Practice of Physic*, vol. ii, p. 43; where examples are given. On this subject, also, Dr. Gavin Milroy says: "It has been confidently stated that every known visitation of the epidemic in the Faroe Islands has been preceded by the arrival of a vessel or vessels from Denmark, when it was prevailing there. But such a statement must not be too readily received; as it is well known that other islands, equally distant from any continent, has been visited, quite independently of arrivals therefrom." See also the articles by Dr. Parkes on this disease in Reynolds's *System of Medicine*, vol. i.

* Referring to the views of Dr. W. Budd and others as to the disease being propagated only by sewage which is contaminated by typhoid stools, Dr. Murchison says:—"Admitting fully that this view offers the best explanation of those cases where the fever is propagated by the sick, many, if not most, of the facts adduced in its support are explicable on the theory of spontaneous generation, while in the others the mode of transmission is less clearly established than might be desired. On the one hand, facts are adduced to show that the disease is contagious; and, on the other, cases are mentioned to demonstrate the intimate connection between its origin and bad drainage. The evidence, however, is still insufficient to prove that the stools of the sick have constituted the medium of communication. This conclusion, it seems to me, has been jumped at from the unwillingness to admit that a communicable disease can ever have a spontaneous origin. But, in the second place, there are many facts which show that enteric fever often arises from bad drainage, independently of any transmission from the sick. As long as the current flows freely through a drain, there is little danger of the emanations from it giving rise to enteric fever. The danger arises when the drain becomes *choked up*, when the sewage stagnates and ferments." The dejections from a typhoid patient being remarkably prone to undergo decomposition, Dr. Murchison adds: "It is possible that the stools of enteric fever are more prone than ordinary sewage to the *peculiar* fermentation by which the poison is produced, and that even in certain cases the fermentation may have commenced before their discharge from the bowels. In this way, enteric fever may occasionally be propagated by the stools, but even then it seems more probable that the poison is always the result of *decomposition*, than that it is derivable from a specific eruption like that of small-pox." ("On the Causes of Continued Fevers," in *Lond. Med. Rev.*, 1863.)

† Dr. Murchison very aptly remarks:—"It has been the custom with many writers to refer epidemics of typhus to some subtle 'epidemic influence'; and thus when a failure of the crops has been followed by typhus, both of these disasters have been ascribed to a common atmospheric cause. But of such atmospheric influences, capable of producing typhus, we know nothing; their very existence is doubtful, and the employment of the term has too often had the effect of cloaking human ignorance, or of stifling the search after truth. If typhus be due to any 'epidemic influence', why does this influence select large towns and spare the country districts? Why does it fall upon large towns in exact proportion to the degree of privation and overcrowding among the poor?" (*loc. cit.*) Still, although the prevalence of typhus fever may be in great part accounted for without resorting to unknown 'epidemic influences', it must not be supposed that there are no unknown cosmical influences which have to do with the outbreak and spread of various epidemic diseases. Let us rather admit that which seems so probable, and live in the hope that we may one day ascertain more concerning their nature.

‡ See *Continued Fevers of Great Britain*.

§ Doubts, however, are entertained on this subject. (See Mr. Radcliffe's article in Reynolds's *System of Medicine*, vol. ii.)

amongst want, wretchedness, and squalor, and its true nature was not recognised for many weeks, in consequence of its close resemblance to ordinary typhus, to which it seems to be nearly allied."*

Now the remaining members of the group of specific infective diseases are varicella, hooping-cough, measles, scarlet fever, and small-pox. The knowledge which we possess concerning the mode of origin of these otherwise than by infection is almost *nil*. They differ amongst themselves, it is true, as regards their degree of infectiousness; but, as others have suggested, they are probably more strictly dependent upon individual states than upon external conditions, and, consequently, are more baffling to those who attempt to fathom their causes. Measles, scarlet fever, and small-pox, are undoubtedly amongst the most contagious of diseases, and, therefore, the chances are always strongly in favour of their contagious origin in any given case. But should this satisfy us? Should we be content to say that even measles, scarlet fever, and small-pox, are propagable only by means of contagion, and cannot arise *de novo*†? Are they not strictly comparable with many other general infectious diseases which undoubtedly arise 'spontaneously'? Do we not see amongst those which may so arise that the degree of contagiousness is altogether variable? Does not this seem gradually to increase in each affection, as the off-cast particles have tendencies to undergo molecular change which are more and more capable of initiating chemical actions of a spreading character in the blood or mucous surfaces of ordinary individuals? And does not the diminishing contagiousness of different diseases seem to be due to the fact that off-cast particles are less and less capable of acting upon the healthy fluids and mucous surfaces of the body, but require that these should be altered, now by one set of agencies affecting the general health, and now by another, before such particles can initiate those changes which lead to the evolution of one or other of the specific poisons within the body? Hooping-cough, measles, scarlet fever, and small-pox, would in this case be merely the last terms of a series, differing from the other members simply in degree, but not in kind—and therefore as capable of being generated *de novo* as either of the others, although much more capable than they are of being disseminated by means of contagion.

If we reject this notion, what remains for us? The germ-theory is quite untenable—the analogy which has been thought to exist between the causes and nature of certain diseases and the specific and unalterable characters of living organisms is erroneous in both its aspects. And even if the diseases are *now* only propagable by contagion, just as the higher living things are propagable by reproduction, they must nevertheless have originated once; and, if once, why not now? Or, declining to admit even so much, shall we refuse to bear our own burdens? Shall we shift the difficulty, and suppose that the poisons of syphilis, measles, scarlet fever, small-pox, and other diseases, have been evolved amidst the unknown conditions obtaining upon the surface of an unknown world, whose disruption has scattered them broadcast, and conveyed them to us, with other never-dying germs, upon the verdant surface of a "moss-grown fragment"? With such alternatives, surely our choice cannot be doubtful.

If we turn to a sober survey of the facts which lie before us concerning the infective diseases as a class, our difficulties will be much diminished: simple and obvious conclusions will appear. *(See Table.)*

In the first place, we find a group of diseases due to the presence upon or within the body of parasitic organisms. These are partly local and partly general affections, the latter being intensely contagious, and on that account frequently confounded with other general infectious diseases in which living organisms do not occur. These general parasitic diseases are propagated by the presence and multiplication of living units, whilst those of the next great class are not.

* *Transactions of Epidemiological Society*, vol. v, p. 174. This is generally the rule with regard to epidemics. They occur mostly at times when other ordinary or non-specific affections to which they are most closely related are prevalent. And during the period of their decline, the more virulent epidemic forms of the affection again seem to lapse into more ordinary forms of disease.

† It seems to me that at present the facts are looked at much too exclusively from one point of view. It is fully admitted by many persons that during epidemics, more especially, a large number of cases of small-pox occur, even in isolated situations, in which it is quite impossible to obtain any evidence of contagion. When we consider, further, that the disease is epidemic at times, and then almost dies out, although multitudes remain who might be infected, we must admit that something besides contagion is undoubtedly operative in facilitating its spread during these times, and therefore we may assume it to be possible that this 'epidemic influence' of itself might, in certain persons, suffice to engender the disease without contagion. Dr. Gavin Milroy (*loc. cit.*) says: "This most interesting subject has not been investigated with that patient and searching care which all physical problems demand. The prevailing negative belief rests on merely presumptive grounds, rather than on sifting inquiry. That outbreaks of measles, hooping cough, etc., have been observed in various remote islands, and at distant intervals of time, without any traceable connexion with previous cases, either in the country itself or amongst recent arrivals, can scarcely be doubted. Hillary particularly alludes to his having noticed such occurrences in Barbadoes; and the medical history of other West India islands would afford, I believe, similar evidence." See also Hecker's *Epidemics*, pp. 215-218.

PARASITIC DISEASES AFFECTING:

- *Many of them capable of arising "de novo."*
  - External (cutaneous) surface.
  - Internal (mucous) surfaces.
  - Closed (serous) cavities.
  - Tissues of organs or parts. (*Psorospermiæ, Cysticerci Nematoids, etc.*)
  - Blood. (*Bacteridia* in 'Malignant Pustule', *Psorospermiæ* in '*pébrine*', etc.)

  → Caused and propagated by the presence and self-multiplication of living units.

COMMUNICABLE DISEASES.

TISSUE DISEASES.

A. *Diseases of Internal Formed Tissues and of Mucous Membranes.*

- *All inoculable and capable of arising "de novo."*
  - Fibro-plastic growths.
  - Cancerous growths.
  - Tubercular growths.
  - Glanders.
  - Syphilis.
  - Gonorrhœa.
  - Purulent ophthalmia.
  - Diphtheria and Croup.

  → *Principally sporadic.*

B. *Diseases of the Blood (principally).*

- *All contagious and capable of arising "de novo."*
  - Erysipelas.
  - Puerperal fever.
  - Surgical fever.
  - Pyæmia.
  - Hospital gangrene.
  - Rabies.
- Rheumatic fever.
  - *a.* Dengue.
  - *b.* Sweating sickness.

  → *Principally Endemic.*
- Intermittent fever.
  - *a.* Remittent fever.
  - *b.* Yellow fever.
- *Contagiousness either absent, little marked, or more or less virulent; all probably capable of arising "de novo."*
  - Summer diarrhœa.
  - *a.* Choleraic diarrhœa.
  - *b.* Cholera.
  - Dysentery.
  - Influenza.
  - Mumps.
  - Relapsing fever.
  - Typhoid fever.
  - Typhus fever.
  - *a.* Cerebro-spinal meningitis?
  - *b.* Plague.
  - Varicella.
  - Hooping cough.
  - Measles.
  - Scarlet fever.
  - Small-pox.

  → *Often Epidemic.*

→ Caused and propagated by chemico-physical agencies, and not by the multiplication of living units.

The tendency in the former is towards death; the tendency in the latter towards recovery. The non-parasitic infective or specific diseases are also partly local and partly general affections. The local affections are closely allied to other morbid states, such as cancer and tubercle, with which they are not usually classed. Many of these local diseases tend to become general diseases. Similar morbid growths spring up in various parts of the body, and the blood itself becomes affected. They are also more or less apt to spread from individual to individual. All are capable of being generated *de novo*. Such local affections are united by the closest bonds of similarity to the more general zymotic diseases, amongst which all degrees of contagiousness are manifested. The members of the whole series, however, are intimately related to one another; and their mode of propagation is essentially similar, even though the readiness with which contagion occurs is variable. Very many of them are undoubtedly generable *de novo;* and the others are probably also capable of arising 'spontaneously', though the proof of this, on account of their highly contagious nature, is difficult to establish.

All these latter diseases, therefore, are dependent upon local perverted modes of growth, or upon chemical changes of a definite, though unknown, character taking place in the blood—partly under the influence of general causes, and partly owing to the initiation of chemical changes induced by contact-action of contagious particles or fluids. As with diseases in general, so with these, two sets of factors are frequently concerned in their production. There are the 'predisposing causes' pertaining to the condition and tendencies (either inherited or acquired) of the individual, and there are the 'exciting causes' or external influences (usual or unusual) at the time operative upon this individual. The combined influence of these causes of disease are often called into play in the production of the infective malady, just as much as they are influential in the origination of non-infective diseases. But predisposing causes may, in conjunction with ordinary external agencies, suffice in some cases; just as, in other cases, the exciting cause or causes may be capable of initiating the affections in the average healthy individual, without the aid of any predisposition.

Unless we entertain opinions of this kind, facts which are admitted by all seem quite incapable of being explained, whether they have

reference to the 'generalisation' of morbid growths within the body, or to the spread of infectious diseases amongst the community. Cancerous particles in the circulation are inoperative in certain individuals, or in many parts of other individuals, however numerously they may exist. Contact with the contagia of ophthalmia or diphtheria will excite the disease in some persons and not in others. Yellow fever and cholera are 'contagious' only when certain favouring conditions are present to facilitate the operation of the specific poisons of these diseases. Rabies cannot be communicated to certain dogs. Professor Gamgee* mentions a case in which a pointer did not contract the disease although it was bitten seventeen times by mad dogs. And, even the most contagious affections—those in which the poison is usually sufficiently potent to act upon the average individual—does not seem capable of being communicated to some persons. Do we not see individuals fully exposed to the contagion of measles, scarlet fever, and small-pox, and yet fail to contract the disease? Facts of this kind are familiar to all medical men. Sir Thomas Watson has referred to the case of "an old woman who for years had been in the habit of going from village to village as a nurse; and of nursing a great number of persons labouring under small-pox, which she had never had, and against which she (naturally enough) believed herself proof": but, he adds, "at length she was taken ill, and died of small-pox in the eighty-fourth year of her age." Again, he says: "In 1845, a lady with whom I am acquainted went through an attack of measles, that disease being prevalent in the village where she was then residing. She had never had the measles previously; yet she had long before personally tended eleven of her twelve children when ill of the same complaint."†

Such facts are quite inexplicable in accordance with the vital or 'germ-theory' of causation of these diseases,‡ though they become much more easy to understand in accordance with the views which have just been laid before you. They are, further, thoroughly harmonious with the results of experiments made by myself and others with reference to the causes of fermentation. These have led me to reject, as too narrow and exclusive, the 'vital theory' of Pasteur, and to adopt the broader physico-chemical doctrines of Liebig, which appear to be harmonious with all the facts. In endeavouring to explain the initiation of fermentation in any particular fluid which has been boiled, we have also to consider the influence of intrinsic tendencies in the fluid, in combination with the exciting or external agencies to which it is subjected. In some cases, the intrinsic tendencies may of themselves be potent enough to initiate the process; whilst in other instances the mere contact-action of an unheated organic fragment combines with weaker inherent tendencies to incite the fermentative process.§ Fermentations may be associated with the presence of organisms, or they may occur independently. The ordinary zymotic diseases are comparable with fermentations of the latter class; and their several contagia act after the fashion of the mere dead organic fragment upon the fermentable fluid.‖

Some *boiled* fluids are quite incapable by themselves of initiating a fermentative process; but this tells no more against the positive abilities of other fluids, than does the fact that certain diseases are unable to spring up amongst a particular community, tell against the circumstance that they do so arise amongst other communities where a number of unhygienic surroundings, previously absent, are also operative in producing the result.*

Amongst the 'exciting causes' of disease, there must be many which are to us at present utterly obscure. More especially is this the case with epidemic diseases. There are, undoubtedly, 'epidemic influences' concerning which we know scarcely anything, but whose existence is only too surely attested by the history of the great epidemic and epizootic affections. As Fleming says, in his *Animal Plagues*, "it has been a matter of common observation from the earliest times, and our history will testify to its accuracy, that wide-spread pestilence in plants, and murrain in animals, have frequently either preceded, accompanied, or followed closely on those visitations which caused mortality and mourning in the habitations of men; showing an identity of causation or affinity, which strongly tempts the inquirer to solve the secret of their joint production."† 'Causes' of this kind, however obscure, are undoubtedly none the less real. Whilst we may hope, therefore, that increasing knowledge will ultimately enable us to throw more light upon their nature, we may at least feel assured that the efficacy of these 'causes' may be increased or diminished by us at will. 'Exciting causes' of all ordinary severity require to be supplemented by the action of 'predisposing causes' existing in the individual himself before disease can be generated. Although we are comparatively powerless to rectify mere individual idiosyncrasies, of the very nature and existence of which we may be ignorant, still these constitute a mere fractional part of the predisposing causes which favour the spread of epidemic affections. These are, in the main, produced in the individual by the operation of the more general exciting causes of disease, such as bad or insufficient food, bad water, and impure air; or they are dependent upon more special causes, such as depressing emotions, excessive muscular exercise, or the occurrence of any unusual amount of degenerative changes within the body. As Dr. Carpenter pointed out nearly twenty years ago, in a very able article on the 'Predisposing Causes of Epidemics'‡, these causes are reducible to one or other of three categories: "1, those which tend to introduce into the system decomposing matter that has been generated in some external source; 2, those which occasion an increased production of decomposing matter in the system itself; and 3, those which obstruct the elimination of the decomposing matter, normally or excessively generated within the system, or abnormally introduced into it from without." Now, the common characteristic here is that "any one of these causes will tend to produce *an accumulation of disintegrating azotised compounds, in a state of change, in the circulating current*"; and observation seems to tell us that either of the causes leading to such a result may, when potent, suffice to assist the spread of epidemic diseases, though two or more in combination lead to much more certain results. Much has been done to diminish the prevalence of these conditions,

* In Reynolds's *System of Medicine*, vol. i, p. 717.

† *Principles and Practice of Physic*, vol. ii, p. 782.

‡ The lowest kinds of living germs with which the hypothetical disease-germs are compared, will live and flourish in various media, and will infallibly set up fermentative changes in fluids which differ very considerably from one another. The contagiousness of general parasitic diseases also manifests itself equally well upon persons of all ages, and at all times.

§ Look, again, at the great moral epidemics which were so prevalent in the middle ages, and which in their most marked form have extended almost to our own times. Here, also, we have a changed mode of action in certain parts of the body, brought about partly by 'predisposing', and partly by 'exciting causes.' We may read in Hecker (*Epidemics of the Middle Ages*, p. 142) as follows: "In a Methodist chapel in Redruth, a man, during divine service, cried out with a loud voice, 'What shall I do to be saved?' at the same time manifesting the greatest uneasiness and solicitude respecting the condition of his soul. Some other members of the congregation followed his example, cried out in the same form of words, and seemed shortly after to suffer the most excruciating bodily pain. This strange occurrence was soon publicly known; and hundreds of people who had come thither, either attracted by curiosity or by a desire, from other motives, to see the sufferers, fell into the same state. The chapel remained open for some days and nights, and from that point the new disorder spread itself, with the rapidity of lightning, over the neighbouring towns of Camborne, Helston, Truro, Penryn, and Falmouth, as well as over the villages in the vicinity. Whilst thus advancing, it decreased in some measure at the place where it had first appeared, and *it confined itself throughout to the Methodist chapel.* It was only by the *words which have been mentioned* [contagia] that it was excited, and it seized none but *people of the lowest education.* Those who were attacked betrayed the greatest anguish, and fell into convulsions. . . . According to a moderate computation, 4000 people were, within a very short time, affected with this convulsive malady." The various signs and symptoms of the malady are then described

‖ It is, however, quite conceivable that, in certain cases, the changes in the blood might, in the last stages of the disease, assume such a character as to lead to the evolution of *Bacteria* in this fluid. This change, which *may* occasionally occur during life, does undoubtedly occur very soon after death, in some diseases. In two cases, one of rheumatic fever and one of typhoid fever, in which the temperature had gone up to 108-110 deg. Fahr. a few hours before death, I found the vessels of the brain and of other parts of the body containing myriads of *Bacteria* even within forty hours after death, and whilst the temperature of the air had not been over 65 deg. Fahr. The blood was blackish and fluid, the organs were much blood-stained, and in addition to other marks of putrefaction, bubbles of gas were abundant in the meshes of the pia mater. The blood of such, and of other similar patients examined during life, have never revealed to me the least trace of *Bacteria.* Dr. Burdon Sanderson, moreover, has ascertained that the blood and other fluids of the body do not generally exhibit any zymotic tendencies (see *Thirteenth Report of Medical Officer of Privy Council*). Some of the *Bacteria* which were found after death, I believe to have been evolved *de novo*, whilst others were descendants of those which had so arisen, in the putrescent blood. No other view seems to me to be so tenable as this. The fluids in a pyæmic abscess may occasionally be on the road towards similar results, and, even if no *Bacteria* exist, such fluids might exhibit 'zymotic' properties.

* There is, however, a great tendency to draw such conclusions; just as there is a tendency with others to conclude that *Bacteria* do not arise *de novo* because there is no evidence of such an occurrence when dealing with Pasteur's solution or a few other fluids, different from those in which the process is stated to occur. Let any person, for instance, repeat Dr. Sanderson's thirteenth experiment (*Thirteenth Report of the Medical Officer of the Privy Council*) with a strong infusion of hay or turnip, rather than with Pasteur's fluid, and then such results will occur that, from Dr. Sanderson's data, he will have no option but to admit that *Bacteria* do arise *de novo.* It is surprising that such an experiment was not tried in the face of all that has been said concerning the productivity of such fluids. The real laws by which contagion is regulated can never be adequately understood unless one knows whether the *contagia* with which one is concerned can, under any circumstances, arise *de novo.* This seems to me to be the point which should be first ascertained.

† If additional reasons were needed to enforce the vast importance of the fullest knowledge concerning these diseases, they are not wanting. The same author writes:—"The losses from only two exotic bovine maladies ('contagious pleuropneumonia', and the so-called 'foot-and-mouth disease') have been estimated to amount, during the thirty years that have elapsed since our ports were thrown open to foreign cattle, to 5,549,780 head, roughly valued at £83,616,834. The late invasion of 'cattle plague', which was suppressed within two years of its introduction, has been calculated to have caused a money loss of from five to eight millions of pounds."

‡ *British and Foreign Medico-Chirurgical Review*, 1853, vol. xi, p. 175.

which act only too surely upon the individual in arousing the 'predisposing' causes of disease, though far more still remains to be done. Happily, however, public attention is now becoming slowly aroused to the importance of pure air, pure water, efficient drainage, and wholesome food, as instruments for maintaining the health of the community.

Let us not be blinded, however, by any narrow or exclusive theories which would teach us that epidemic and infective diseases cannot arise *de novo*. Let us, instructed by a broader survey of the facts, assign no such limits to natural possibilities, and not lightly accept theories which lead to supineness, when we ought to be stimulated to exertion. Whilst accepting to the full all doctrines which inculcate the necessity of diminishing the chances of contagion by every available means, let us, full of hope, diligently seek also for the causes which engender even the most contagious of diseases. Prevention of disease is *the* grand end and aim of medicine; if, then, we have learned from the sad lessons of experience that scarlet fever and small-pox are virulently contagious diseases; if, even in ninety-nine cases out of a hundred, or even in a still larger ratio, both of these diseases are acquired by contagion, then is it all the more important that we should strive to ascertain what are the invariable and immediately antecedent sets of conditions, or states of system, which suffice actually to engender these maladies. In such cases knowledge and power are most frequently convertible terms. Next to typhus fever, the most fatal of the infective diseases which occur in this country are scarlet fever,* small-pox, measles, and whooping-cough. The ravages of typhus in our crowded cities and in our jails have been enormously curtailed, not so much because of its diminished spread by contagion, but rather because we have learned what are the causes which engender it, and are therefore better able to prevent its occurrence. Let us strive, then, to acquire a similar knowledge concerning scarlet fever, small-pox, measles, whooping-cough, and other contagious diseases, and so endeavour, in the most efficient manner possible, to check the ravages of these *morbi populares*.

Time will not permit me even to allude to the many other interesting and important problems which still remain to be solved in reference to these diseases. What I have said, however, will, I hope, suffice to inspire you with a sense of the great difficulty of the problems which you will subsequently have to face; and, therefore, to make you feel the urgent need for diligent, patient, and honest work all through your career—without which you will not be able conscientiously to accept the high responsibilities that will subsequently devolve upon you as practitioners of medicine, and without which no real advance in knowledge can ever be made.

* Mr. J. Netten Radcliffe says (Ranking's '*Abstract*', vol. xli, 1865): "The Registrar-General's returns of scarlet fever for the whole of England, include two periods of five and sixteen years respectively. The first period extends from 1838 to 1842, and the second from 1847 to 1862 inclusive. The total number of deaths registered from the disease in the twenty-one years was 310,720; the annual average mortality for the whole series of years was 14,796." . . . "The history of the progress of scarlet fever in the metropolis differs from that of the entire kingdom in this, that it shows a great augmentation of the mortality from the disease in the last quarter of a century. The annual average mortality from the malady in London during the past twenty-six years was 83 per 100,000 population. The average varied from 32 in 1841 to no less than 174 in 1863. In the quinquennium 1839-43 the annual average was 78; in the quinquennium 1844-48 it increased to 88; in the quinquennium 1859-63 it advanced to 115. The death-rate of 1863 (174) was more than double the annual average of the twenty-six years, 1838-64."

UNIVERSITY OF CAMBRIDGE.

ANATOMY AND PHYSIOLOGY.—Professor Humphry gives notice of lectures and practical teaching as follows. Lectures on Anatomy and Physiology in the new Museums, on Tuesdays, Thursdays, and Saturdays, at 1 P.M., commencing on Saturday, October 21st.—Lectures on Practical Anatomy on Mondays, Wednesdays, and Fridays, at 6 P.M., commencing on Monday, October 16th. These, together with the Lectures on Anatomy and Physiology, constitute the course for M.B. and M.C., and for the Royal College of Surgeons.—Microscopical Demonstrations on alternate Tuesdays, at 6 P.M., commencing on Tuesday, October 31st.—Practical Histology on Saturdays, at 11.30 A.M., commencing October 28th. This, in conjunction with the Practical Physiology by Dr. Michael Foster, constitutes a course of Practical Physiology.—Superintendence of Dissections daily.—Instruction on Practical Anatomy will be continued in the Christmas vacation.—The Downing Professor of Medicine (Dr. Fisher), or his deputy (Dr. Latham), will deliver a course of lectures on Materia Medica and Therapeutics during the ensuing Michaelmas and Lent terms. The lectures during the Michaelmas term will be delivered in Downing College, on Tuesdays, Wednesdays, Thursdays, and Saturdays, at 9 A.M., commencing on Tuesday, October 24th; and during the Lent term on Tuesdays and Saturdays. Fee for the course, £3 : 3. The Museum of Materia Medica at Downing College is open daily to all students of medicine.

ABSTRACTS OF INTRODUCTORY ADDRESSES

DELIVERED AT

THE METROPOLITAN AND PROVINCIAL SCHOOLS,

On OCTOBER 2nd and 3rd, 1871.

ST. MARY'S HOSPITAL.

THE introductory Address was delivered by Dr. ALFRED MEADOWS, Physician-Accoucheur to the Hospital and Lecturer on Midwifery.

The lecturer commenced by defending the system of the delivery of introductory addresses, which, like other institutions, had become subjected to the "ruthless scrutiny of the nineteenth century", and had indeed been tried and condemned at one of the oldest of the metropolitan schools. He thought that "one obvious use of these introductory lectures is the tendency which they have to keep alive that bond of Freemasonry which ought to exist between all the members of a profession such as ours, and notably between the pupils and teachers of each medical school. It is almost the only occasion in the academical year on which, being free from other collegiate duties, we, teachers and pupils of all years, meet together for the special purpose of being together, and for the mutual interchange of thought with thought."

Referring to the changes which had taken place in the school during the past year, Dr. Meadows said: "By the mere lapse of time (we may indeed be thankful it is from no other cause), we have to regret the partial loss of three of our most distinguished colleagues—Dr. Sibson, Dr. Tyler Smith, and Mr. Lane. I say partial loss, because their names are still on our staff, their hearts are yet in the place and in the work, and their spirit still energises the labours of their successors. We have also to regret the loss of Dr. Russell as Lecturer on Chemistry, and of Dr. Payne as Lecturer on Pathological Anatomy. But we can generously afford to congratulate the schools of St. Bartholomew and St. Thomas, which have gained their services, while we welcome their successors—Dr. Wright to the Chair of Chemistry, and Dr. Reginald Stocker as Medical Tutor and Pathologist, an appointment now for the first time made in this school. In the latter, we believe that a colleague has been found to whom you will feel more than ordinarily grateful, if you will use him as he would wish you to do."

The lecturer explained the duty of the office of medical tutor, to which Dr. Stocker had been lately appointed, to be that of assisting the students in their studies—"to help them to use their eyes, their ears, their hands, and above all, their understandings". The instruction conveyed by the medical tutor was, in a measure, a substitute for that gained by apprenticeship—the nearly complete discontinuance of which Dr. Meadows regretted. He referred also to the lately instituted scholarships in Natural Science; remarking that there was an obvious fitness in the foundation of such scholarships in a medical school, inasmuch as no one could doubt the enormous value to the practitioner of medicine of a thorough acquaintance with natural science, and at no time could this be so well acquired as at the commencement of medical study.

Leaving matters of local interest, and turning to the question of the selection of a subject for discourse, Dr. Meadows referred briefly to the relations of medicine to science in general, to society, to politics, and to theology. Regarding its social aspects, he observed: "It may be said, in truth, that society itself could not exist independently of the profession of medicine. In political life, also, the medical profession is yearly growing in influence and importance; and no politician can, with any hope of success, aim at influencing the government of the country if he be indifferent to the value of medical opinion in public affairs. Indeed, I have the best reasons for knowing that the political leaders of both sides of the house would gladly welcome a much larger number of medical men to the House of Commons than at present belong to it."

After a few more remarks, the lecturer proceeded to comment on the relations of science to religion. "We hear a good deal nowadays, and some people seem to take a special delight in talking about the conflict between science and religion. Now, I am not one of those who believe that true science is, or can be in any way or degree, opposed, either in theory or in fact, to what is called revelation. I have too strong a faith in the latter, and too high a love for the former, ever to believe in such a possibility, or to entertain the shadow of a doubt as to their complete oneness in all essentials. It has always appeared to me a simple impossibility that true science and revelation should ever