Is consumption contagious? : and can it be transmitted by means of food?

Is Consumption Contagious?

CLAPP

SECOND EDITION

IS

CONSUMPTION CONTAGIOUS?

AND CAN IT BE

TRANSMITTED BY MEANS OF FOOD?

BY

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"A disbelief in the contagion of consumption is highly favorable to the spread of that disease (if it can really be propagated in that mode), inasmuch as in private practice and both civil and military hospitals no measures of prevention are employed." — *Principal Diseases of the Interior Valley of North America. Vol. II. By Daniel Drake, M. D.*

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PREFACE TO THE SECOND EDITION.

DURING the twenty months which have passed since the appearance of the first edition of this book, many communications from readers in different parts of the country, laymen as well as physicians, have been received, in which cases of consumption, probably originating from contagion, were reported, or strong beliefs in the possibility of such origin expressed. Further experiments, not necessary to be here mentioned, but mostly confirming the inoculability of tubercle, have during this time been undertaken. The most important addition to our knowledge on the subject, however, has come from the recent very remarkable researches of Dr. Koch of Berlin, an abstract of which by the celebrated Prof. Tyndall, who appreciates their scientific importance, is quoted in the Appendix on page 179. Most of the prominent medical journals of the world have already described them and admitted their great value.

Although Schüller, Toussaint, Klebs, and Aufrecht had, previously to Koch, announced the discovery with the microscope of peculiar spherical, or actively moving, or rod-shaped micrococci, which they thought were the specific germs of tubercle, and had even tried to cultivate them in albuminous fluid, yet it is now probable that, owing to faulty methods of experimentation, together with the real tubercle bacilli several other minute organisms were also cultivated by them in the same fluid, and were mistaken for these microscopically. Of course the injection into

animals of this fluid, as it contained among other things the true tubercle bacilli, occasioned tuberculosis, as had the injection of crude tubercle by many previous experimenters. It has remained for Koch to prove that his recently described organisms of rods, which are invisible under the microscope by previous methods of observation, are really the *specific parasites of tubercle*, (1) by skilfully isolating them by ingenious methods from other minute organisms as well as from their gross surroundings; (2) by cultivating them, generation after generation, for months on a specially prepared solidified gelatinous substance; and then (3) by invariably producing the disease in animals by the inoculation of the cultivated parasites, and those only; the simultaneous inoculation in other animals of the same culture substance *without* the parasites always resulting negatively.

Koch's experiments have also settled a question over which there has been much controversy. The infectious cattle disease *Perlsucht* (see page 120) is now proved by him to be identical with human tuberculosis. Since our first edition, Dr. Charles Creighton, whose views on this subject are briefly presented on page 138, has given a very detailed and interesting account of them in a book entitled "Bovine Tuberculosis in Man."

Last year Toussaint vaccinated a cow, suffering from advanced tuberculosis, with absolutely pure vaccine lymph. The vesicles progressed normally, and with lymph from them he vaccinated successfully a number of animals. All of these latter animals, however, became tuberculous. (*Medical Times and Gazette*, Sept. 3, 1881.) Does not this suggest the necessity of caution in the selection of heifers on the part of propagators of animal vaccine virus?

One of the chief arguments of these propagators against humanized virus has been the possibility of conveying from one person to another, not only the vaccination, but also some disease. It has been asserted that with virus direct from the cow this is impossible!

As to Koch's discovery of the presence of tubercle bacilli in large numbers in the expectoration of phthisical patients, and of such only, and of their activity retained even for weeks in the dried sputum, much of which in a pulverized form floats in the air, one must be blind indeed not to see the very practical connection of this demonstration with the contagiousness of consumption. Some of the possible reasons why phthisical sputum is not oftener the cause of the disease are given in the quotation from the *Lancet* on page 184. The latter suggestion there made, as to the aptitude of the soil, had been previously brought forward by the world-renowned Prof. Rindfleisch (Virchow's Archiv., 1881, B. 85, p. 71), who believes tuberculosis to be undoubtedly an infectious disease, but one for which mankind has to some extent established a *tolerance*, and which in consequence appears in its original character only in a person where this tolerance does not exist. Good food and a free blood formation, he thinks, give immunity from the outward manifestation of this latently almost universal disease. If these fail, tuberculosis becomes evident.

While contemplating the importance of Koch's great discovery, it is impossible for us not to speculate on the future probability of preventing, by its inoculation in a modified form, that disease which is the greatest scourge of mankind; nor is it merely an idle invasion of the province of dreamland thus to speculate. Pasteur was lionized more than any other

man at the International Medical Congress at London last summer because of his brilliant achievements in preventing anthrax, the fatal splenic fever in cattle, by inoculating its bacillus modified by cultivation, and thus producing the disease in a mild and harmless form. Vaccination itself many now regard as simply inoculation with the bacillus of small-pox, modified by its transmission through the bovine race. The opinion seems to be rapidly gaining ground that the other eruptive fevers, which are most likely due to the growth within the body of minute parasites, generically called bacilli, may perhaps be forestalled at some future time by the designed production of very mild forms of them by the inoculation of their bacilli altered by cultivation. Dr. Koch, in his experiments hitherto, has only cultivated the tubercle bacillus in its original virulence; but of course now many other investigators, who think they can forecast the signs of the times, will attempt with eagerness to cultivate these parasites in diminished intensity, and grapple, full of hope, with one of the greatest problems ever offered to the physician, — the prevention of consumption. Whether consumption can ever be prevented in this way is somewhat doubtful, we grieve to say. The analogy with small-pox is not a perfect one, because one attack of consumption, however mild, surely does not confer immunity from future attacks, but on the contrary rather predisposes the victim to them. However, even if Koch's researches do not facilitate the prevention of the disease by inoculation, they surely will do a great deal towards accomplishing the same object by enforcing on public attention the *contagious nature* of the disease, and by warning people not to breathe in nor eat its germs.

11 Columbus Square, Boston, July, 1882.

INTRODUCTION.

It is not from any desire to create a few hours' "sensation," or to gratify a gaping curiosity, or even to attempt to settle any purely abstract and theoretical discussion, that I have prepared the following pages, but because I have become thoroughly imbued with the great importance of the subject, and feel impelled by an irresistible urging to point out and emphasize, by word and implication, its exceedingly *practical bearings*. Consumption is such a fearfully common and fatal disease all over the world, that few questions can be more practical than those on our title-page, which involve very important methods of its propagation. To be sure, our knowledge is not sufficiently accurate to enable us to give just now, with mathematical exactness, a positive answer to these questions, but it is very doubtful if any thoughtful person can read through these seven chapters without being persuaded that there is "something in it," and that *to a certain extent* at least, and *under certain conditions*, consumption *is* contagious.

Some of the more important practical results to be obtained by a judicious agitation of this subject are

the following: (1.) That no person, particularly if young, should be allowed to sleep in the same bed, or even (if it can possibly be prevented) in the same room with a consumptive. (2.) That no person should be allowed to remain for too long a time in too close or too constant attendance on a consumptive. (3.) That ventilation as perfect as possible should be secured. (4.) That the most rigid inspection of all the meat that comes into our markets, particularly at the slaughter-houses, and of all the cows which furnish us milk, by competent government officials beyond the temptation of bribes, should be insisted on for the public safety.

Of course, it is not to be supposed that these points would be entirely neglected without the agitation of this subject, but it certainly supplies an incentive in their behalf of wonderful power. Those physicians who do not believe in the contagiousness of consumption may, on general principles, advocate the necessity for good air, good food, and good hygienic surroundings, because it seems, on the whole, the proper thing to do; but such general advice as this often comes with very little force, and they frequently fail to insist energetically on these points when disregarded. The friends of the sick person, too, if they are not aware of the danger to which they are exposed, frequently think they cannot, and certainly often will not, carry out these necessary precautions, as almost any physician will testify. But give them a stimulus to vigorous action, convince them of the possible contagiousness of the disease, and the physician's advice will be

prompt and earnest, and the patient's friends will faithfully carry out his instructions to the letter, with a firm purpose and resolution born of an intelligent conviction. It must necessarily be that those who are lulled to sleep in a false security through disbelief in an enemy's presence, are thereby rendered less able to ward off the unforeseen attack.

Some physicians, like the well-known Frenchman Dr. Pidoux, have been known to say: "Well, even if consumption is contagious, we ought not to terrify people by telling them of it, for, if they find it out, they will turn with loathing from their best friends when taken sick, as if they had the leprosy, and consumptives will thus be left to perish miserably in great neglect." Shame on the man who could for a moment entertain such villanous opinions! Does a mother neglect her child because it has such a contagious disease as scarlet fever or diphtheria? Does a wife refuse to provide suitable care for her husband, even if he has such a loathsome affection as the small-pox? Again, is it any kindness to people to lie to them and tell them there is no danger from contagious diseases, when we know there is or may be danger? No; let us be honest with them, and tell them our convictions, our opinions, or even our suspicions, if they rise no higher in the scale, that they may be able intelligently to take due precautions. Even if we run the risk of frightening them unnecessarily, we should not withhold the truth at any hazard. However, as far as our knowledge extends at present, they need not be frightened at all, if they will only take the proper precautions.

In order to arrive at the *truth* as nearly as possible, and to avoid every appearance of distortion, I have tried to present both sides of the argument, derived from all accessible sources, in an impartial manner, often quoting the very words of the authorities drawn upon. If my only object had been to prove contagion, it would have been comparatively easy to work up a one-sided argument in favor of the doctrine, carefully concealing everything that has been or might be said in opposition.

During the last ten years I have noticed quite a number of apparent cases of contagion from husband to wife, or from wife to husband, or from friend to friend. This has been more particularly since I began to look for them, for at first, being rather skeptical, I regarded those cases about which a suspicion might have arisen as merely curious coincidences, and undoubtedly neglected to inquire carefully into others, about which I am now persuaded something might have been learned in reference to the matter now under consideration. I have not included, in Chapter IV., reports of any of my own cases, because I deemed those which I had collected from others (all of them undoubtedly trustworthy sources of information) amply sufficient to illustrate the subject, and also because those which have occurred in my own experience have not, as a rule, seemed to present anything especially interesting to detail beyond the simple fact of contagion. I have not met with any examples of those remarkable series of cases linked together, which characterize several of the reports which I have borrowed from different authorities.

I hope that nothing which may be found in this book will be construed into an argument against the use of two of the most important, healthful, and nutritious foods which we possess, — milk and beef. While, on the one hand, people would be very foolish to allow their timidity to induce them to give up two of the great necessaries of life, on the other hand they would be very culpable, if, in view of the facts already demonstrated by scientific experiments, incomplete though they may as yet be, they did not energetically insist on the most rigid governmental inspection possible.

To those who are convinced of the possible contagion of consumption by statements herein contained, it is of course unnecessary to say anything as to the advisability of marrying a consumptive.

I desire to call attention to the practical instructions given by Dr. Bowditch on page 33. With the exception of his recommendation of the wine, I can heartily indorse them.

Reports of cases or facts bearing on this subject, or opinions from any source, will be thankfully received by the undersigned.

H. C. C.

16 Concord Square, Boston, Nov. 1, 1880.

Is Consumption Contagious?

CHAPTER I.

WHAT WE MEAN BY THE WORDS "CONSUMPTION" AND "CONTAGIOUS."

In entering on the discussion of such a subject as that now under consideration, it will be exceedingly important for us, at the very outset, to state what signification we attach to these words. Time and time again have long and excited wordy wars been waged, utterly profitless in their results, simply because of misunderstandings of the exact meaning of each other's phrases by combatants, whose opinions possibly might not have been very widely different, after all.

Both words have been chosen, because on the whole they seemed to be more expressive and less open to the charge of ambiguity than any others which presented themselves. More particularly was this the case with the word *consumption*, which, not only among physicians, but also among the public at large, is the commonest expression for that dread disease, whose most prominent symptoms are emaciation,

cough, spitting of blood, night-sweats, chills and flushes, etc., and which carries off in most countries from one eighth to one fifth of all those who die. *Phthisis* and *tuberculosis* in the present work are used synonymously with consumption. No one will question this use of the word phthisis, simply because it means precisely the same thing; and even those who follow Niemeyer's pathology, and refuse to acknowledge the primary dependence of cheesy pneumonia or pneumonic phthisis on *tubercles*, will yet admit the frequency of their occurrence in this disease in its last stages, only during which, as a rule, as will be seen in the following pages, contagion is claimed.

Much harder is it to reconcile the various and often conflicting interpretations of the word *contagious*. To show some of the difficulties here to be encountered, a few out of many definitions given by good authorities are quoted.

In Webster's Unabridged Dictionary we read under *Contagion*: —

"CONTAGIOUS, INFECTIOUS. — These words have been used in very diverse senses; but, in general, a *contagious* disease has been considered as one which is caught from another by contact, by the breath, by bodily effluvia, etc.; while an *infectious* disease supposes some entirely different cause acting by a hidden influence, like the miasma of prison ships, or of marshes, and *infecting* the system with disease."

Also, under *Infection*: —

"Medical writers in Europe do not, most of them, recognize any difference between *contagion* and *infec-*

tion. In America the distinction referred to under *contagion* [just quoted] is, to a considerable extent, admitted."

In Bennett's *Practice of Medicine:* —

"By *infection* is understood the power of being propagated through the inhalation of air tainted by the breath or perspiration of the affected person. By *contagion* is understood communication of disease by actual contact."

In *Chambers's Encyclopædia*, we read: —

"*Infection* is distinguished from *contagion* by some medical writers, who would restrict the latter word to the cases in which there must be *contact* of the healthy person with a patient, while they imply the term infectious to diseases which can be conveyed by the atmosphere. The distinction is unimportant." Also:

"*Contagion*, the communication of a disease from the sick to the healthy, either by direct contact of a part affected by the disease, or through the medium of the excretions and exhalations of the body. Some authorities have employed the term *infection* to designate this latter method of communication, and have correspondingly limited the meaning of the word contagion; but no practical end is served by this refinement, and it has, indeed, led to great confusion by obscuring the fact of the communication, which is, when clearly proved by instances, the most important element in the inquiry."

The last edition of Dunglison's Medical Dictionary says: —

"Contagious diseases are produced either by a virus

contagium, capable of causing them by inoculation, as in small-pox, cow-pox, hydrophobia, syphilis, etc., or by *miasmata* proceeding from a sick individual, as in plague, typhus gravior, and in measles and scarlatina. Physicians are, indeed, by no means unanimous in deciding what diseases are contagious and what not. It seems probable that a disease may be contagious under certain circumstances and not under others. . . . *Contagion* and *infection* are generally esteemed synonymous. Frequently, however, the former is applied to diseases not produced by contact, as measles, scarlet fever, etc., while infection is used for those that require positive contact, as itch, syphilis, etc., and conversely. Diseases which cannot be produced in any other way than by contagion, are said to have their origin in *specific contagion*, as small-pox, cow-pox, measles, hydrophobia, syphilis, etc. Those which are produced by contagion, and yet are supposed to be sometimes owing to other causes, are said to arise from *common contagion*."

In Ziemssen's *Cyclopædia of the Practice of Medicine*, in the introduction to the volumes on the "Infectious Diseases," by Professor Liebermeister, of Tübingen, occurs the following definition:—

"Under the name *Infectious Diseases* we group together those affections which we know (or at least believe) must originate through the infection of the system with certain peculiar poisonous matters, and which are mainly distinguished by the fact that they can reproduce themselves, under favoring conditions, to an endless degree."

He subdivides infectious diseases into three classes: *Miasmatic*, *contagious*, and *miasmatic-contagious*. The malarial diseases are purely miasmatic; the purely contagious diseases are measles, scarlet fever, small-pox, cow-pox, typhus fever, diphtheria, glanders, malignant pustule, hydrophobia, virulent ulcers and blenorrhœas, syphilis, pyæmia, and puerperal fever; and the miasmatic-contagious diseases are cholera, typhoid fever, dysentery, and probably also yellow fever and the plague.

In explanation, he says: —

"*Miasm*, in the original and broadest sense, is the name for any material contained in the air that can produce disease. The old writers frequently used the term in this broader sense, when they spoke of the miasm of measles and small-pox, but they also included with it the non-specific noxious matters mingled in the air, as, for example, the products of putrefaction and decomposition, and sometimes even all kinds of injurious gases. Later, the term miasm, being brought into contradistinction with the term contagium, was used in a far narrower sense, and in this narower sense it is now solely used.

"It is usual, now, to speak of *contagium* as a specific excitant of disease, which originates in the organism suffering from the specific disease; while *miasm*, on the other hand, is used of a specific excitant of disease, which propagates itself outside of, and disconnected from, a previously diseased organism. Contagion can be conveyed, by contact, from a diseased person to a sound one, produce the disease in him, and then again

reproduce itself . . . but it is true that *direct* contact is not a necessary condition for the transmission; it can also follow *mediately* from the vaccinator's lancet, from other instruments, from clothing, through third persons, and in many of these diseases *by the air*. . . . Miasm originates from without; taken up into the body, it can call a specific disease into action; but it cannot spread the disease any further by conveying it from a diseased to a sound person."

Neither of these definitions will cover his third class of infectious diseases — the miasmatic-contagious — which, as its name indicates, partakes of each, but belongs strictly to neither of the two preceding classes. Here the germs of disease are originally furnished by the affected body, but before they can reproduce their disease in another person, they have to pass through a distinct stage of development *outside of* the human body, under favorable surroundings, as, for instance, when the discharges which contain them remain some time standing, particularly when in contact with great quantities of organic substances that readily decompose, as in water-closets, sewers, etc.

It may not be out of place here to add that the distinguished author just quoted from, gives it as his opinion that, in the future, phthisis will also be included among the chronic infectious diseases. (Vol. I., page 24.)

From the various and often contradictory definitions just quoted, some of which we feel obliged to characterize as very impractical, to say the least, it will readily be seen that confusion worse confounded almost

necessarily arises from any *ex parte* attempt at scientific refinement through the multiplication of terms and the corresponding limitation of their meaning concerning subjects on which our knowledge is at present no more thorough. The main point which we desire to keep in view in this investigation is, not the proper theoretical exact classification of consumption among diseases, but whether consumption can in any way, under certain conditions (we do not now speak of heredity), be communicated from one person to another. This is the question in which the profession and the people at large are, or ought to be, interested, and no amount of hair-splitting as to the exact meaning of technical terms should be allowed to obscure this really vital question. Therefore we shall use the word *contagious* in its broadest sense; that, indeed, in which it is now *popularly* used almost everywhere, as synonymous with *communicable*, *transmissible*, "*catching*." This definition surely is simplicity itself, and we do not thereby commit ourselves to any particular theory as to the exact mode of its transmission; a procedure which, until we know more about it, is undoubtedly safe.

CHAPTER II.

HISTORICAL AND DESCRIPTIVE.

If the question which forms the subject of this book had been asked of any intelligent physician one hundred years ago, the answer would have come promptly, "Why, yes, from the earliest times medical men and the people generally have believed, almost without exception, that consumption is, to a certain degree at least, contagious." The question was tolerably well settled at that time, and did not admit of much controversy. Any one who is interested in investigating the subject historically to-day, by consulting the medical department of our Boston Public Library, of which we may well be proud, the Boston Athenæum, and such other libraries, public and private, as he may be able to gain access to, and poring over the many musty old volumes which treat of a disease which is so fearfully common and distressing that its literature is necessarily extensive, will find that almost all the celebrated medical writers in the world, down to 1780, have shared this belief. Aristotle (330 B. C.) says that "it makes the breath corrupt and offensive, and those who breathe it suffer." He adds that the Greeks of his day generally believed

in its contagiousness. He asks why consumption, itch, and ophthalmia are common to those who approach near to the persons laboring under these complaints. (*Probl.* l. 7, vii. a.) Hippocrates had previously (400 B. C.) entertained the same ideas. Galen (A. D. 180) says that it is "dangerous to pass the whole day with a consumptive person, and with all people whose diseases generate putrid effluvia." (Galen, iv. 87, 91.) Richard Morton says (*Opera Medica*, Lugduni, 1697. *Phthisiologica*, Lib. II. Cap. 1, p. 27): "A contagious principle also propagates this disease, for, as I have often found by experience, an affected person may poison a bedfellow by a kind of miasm like that of a malignant fever." He adds that phthisis acquired in this way is more difficult to cure, and shorter in duration, than usual.

Valsalva, who was predisposed to consumption, constantly avoided being present at the dissection of the lungs of persons who had died of that disease (Benj. Rush, *Med. Inquiries*, p. 95, Vol. II., 3d ed, Phil., 1809); and Andral says that Valsalva's immortal pupil, Morgagni, avowed that he had never dared to make more than a very few autopsies of persons who had died of phthisis, for fear, as he said, of catching their disease. He clung to this prejudice all his life, and in one of his letters (*De sedibus et causis morborum* (22), A. D. 1761), we read the following sentence:* "Phthisicorum cadavera fugi adolescens, fugio etiam senex."

We learn from Riverius (1656), who gave credence

* As a young man I used to avoid the dead bodies of those who had died of consumption, and now that I am old I still avoid them.

to the facts, that it used to be customary for consumptives to nurse women's milk as a means of cure, and that cases were reported where the nursing women contracted disease in this way. (Drake's *Dis. of the Int. Val. of N. A.*, Vol. II., Phil., 1854.)

Riverius also declared contagion to be the "chiefest" cause of phthisis, "for this disease is infectious. We may observe women to be affected by their husbands, and men by their wives, and all the children to die of the same, not only from infection of their parents' seed, but from the company of him that was first infected."

About a hundred years ago a reaction against this almost universal belief set in; and, as in many other instances in the history of medical opinions, especially those which, from the nature of the case, can never be settled with absolute precision, the pendulum finally swung too far, and after a time the uncertainty and doubts which had slowly arisen, by a gradual transition, finally developed into almost utter scepticism. This was true at least in America and in the northern parts of Europe. The old, well-founded belief, though somewhat modified, never entirely lost its hold in warm latitudes, especially in Southern Europe, and more particularly in Italy, where, to this day, it is widely prevalent. Within comparatively a few years, however, as we shall presently show, the pendulum has begun to swing back again, and probably, before long, more accurate views than ever, based on modern scientific investigations, will be generally accepted.

Briefly to illustrate, we will quote a part of what we

have been able to obtain of the views of prominent writers within a hundred years or so.

The famous Professor Wm. Cullen, M. D., of Edinburgh, says in his *First Lines of the Practice of Physic*, Vol. II. p. 424, 1791:—

"It has been frequently supposed by physicians that phthisis is a contagious disease, and I dare not assert that it never is such; but in many hundred instances of the disease which I have seen there has been hardly one which to me could appear to have arisen from contagion. It is possible that in warmer climates the effects of contagion may be more discernible."

Dr. T. Reid, of London, in 1782, thinks that it may be slightly contagious in extreme cases.

Dr. Benjamin Rush (*Medical Inquiries and Observations, etc.*, Vol. I., Phil., 1789), in the earlier editions of his works, seemed to be fully convinced of the contagiousness of the disease, and in illustration gave an account of its spreading from the white proprietors of an estate among the negroes, who were neither overworked nor particularly anxious. In his third edition, however, printed at Philadelphia in 1809, Vol. II. p. 95, he announced that his views on this subject had changed, and brought up arguments to prove that except in rare cases it is not proper to ascribe the causation of consumption to contagion.

Darwin, in 1793, believed that it was contagious, but only in persons nearly allied.

Dr. Heberden, of London, in 1802 (*Commentarii de morborum historia et curatione*), says:—

"In England we have very little apprehension of the contagious nature of consumption, of which in other countries they are fully persuaded. I have not seen proof enough to say that the breath of a consumptive person is infectious; and yet I have seen too much appearance of it to be sure that it is not; for I have observed several die of consumption in whom infection seemed to be the most probable origin of their illness, from their having been the constant companions or bedfellows of consumptive persons."

Dr. Clark (*Climate and Diseases of the South of France, Italy, etc.*, p. 41) says: "At Rome a gentleman of my acquaintance was refused lodgings from suspicion that he was consumptive; and it was only after a friend's engaging to pay for all the furniture of the lodging, should he die, that he was admitted."

Dr. William Sweetser (*Consumption*, p. 137, Boston, 1836), speaking of Italy, says: "A law once existed there by which the proprietor of the house in which a consumptive patient died could claim payment for his furniture, which was burnt."

Dr. Thomas Young, of London, in 1815, says (*A Practical and Historical Treatise on Consumption*, p. 46):—

"It has been much disputed whether or no consumption is ever capable of being communicated by contagion, and it must be allowed that it would be extremely difficult to produce any strongly demonstrative evidence of the fact. It may often have been observed that the husband or wife of a consumptive person has died consumptive within a few months after that per-

son's death, but it will appear, upon calculation, that this circumstance must happen in London more than once in about four months, before we can deduce from it any proof of the probable connection of the two events. For one, at least, of fifty adults may be expected to die annually, and consequently one of two hundred by consumption, or one out of four hundred in six months; and if we estimate the number of married persons dying annually of consumption at two thousand, we may expect to find five instances of apparent contagion without any real foundation."

The celebrated French physician, Laennec (*Traité de l'auscultation médiate et des maladies des poumons et du cœur* Paris, 1818. 4th ed., 1837, p. 178), expresses his views as follows:—

"Tubercular phthisis for a long time was considered contagious, and it is still so considered by the people, magistrates, and some physicians in certain countries, especially in the southern parts of Europe. In France, at least, it does not appear to be so. We often see, among poor people, a numerous family sleeping in the same room with a consumptive, or a husband sharing the bed of his phthisical wife up to the last moment, without catching the disease. The woollen garments and the mattresses which consumptives have used, which are afterwards burned in some countries, but which are very often not even washed in France, have never, in my experience, seemed to have conveyed the disease to any one. However this may be, prudence and propriety would demand that more precautions should be taken in this respect.

Besides, many facts prove that a disease which is not habitually contagious may become so under certain circumstances."

Prof. Andral, of Paris, consulting physician to the king, etc., etc., who edited the fourth edition of Laennec's work, just quoted, adds in a foot-note, on page 179:—

"Without doubt, the facility of the contagion of pulmonary phthisis has been singularly exaggerated. Yet, is it wise to deny it absolutely and in all cases? Who can affirm, with enough proof to back up his opinion, that a malady which could never be considered as purely local, and which, just in proportion to the progress it makes, very distinctly indicates an infection of the whole economy, is not capable of transmitting itself in cases where patients are brought into very intimate and constant relations (such, for example, as sharing the same bed) with healthy persons, who may thus absorb the miasms which are thrown off both from the pulmonary mucous membrane and from the skin of the sick? All that I can say, without pretending to give a final decision to such an important question, is, that in the course of my practice I have been impressed more than once by seeing women begin to show the first symptoms of a pulmonary phthisis a short time after their husbands, whose beds they had shared up to the last moment, had succumbed to this disease.

"Such a question as this will be always very difficult to decide scientifically, on account of the great frequency of phthisis Some persons will be always

ready to tell of cases where the disease was not acquired in this way after exposure, and will think that they can very easily break the force of such cases as I have referred to by saying that the persons who became phthisical under these circumstances would have become so at any rate. But practically these facts are of sufficient importance to induce persons who have the daily care of consumptives, especially in the last stage of the disease, to take due precautions."

Sir Alexander Crichton, M. D., physician to the Emperor and Dowager Empress of Russia, expresses himself as follows (*Practical Observations on Pulmonary Consumption*, London, 1823, p. 223) : —

"That a healthy person sleeping constantly with one who has an open ulcer in the lungs, and especially where the ulcerated surface is large, or when the ulcers are numerous, and when a vitiated pus is formed, almost infallibly gains the disorder, is a fact which has occurred to me to see so often, that I have no doubt on the subject. It has fallen repeatedly to my lot, particularly among the poor, to see a husband gain the disease from his wife, and many wives gain it from their husbands, under circumstances where there could be no doubt as to the influence of the poison, and no other way of accounting for the disease."

The well-known authority, John Mason Good, M. D. (*The Study of Medicine*, 2d ed., Vol. III. p. 267, London, 1825), says : —

"There is great reason to believe that the disease is in a certain degree contagious. M. Portal, and a few other pathologists of distinction, have doubted or de-

nied that it possesses any such property; but the apparent instances of communication among near relations and close-attentive nurses, and especially between husbands and wives, who have fallen victims to it in succession, are so frequent, that its contagious power has been admitted by most practitioners and in most ages. . . . I have myself been witness to various cases which could not be ascribed to any other cause. . . . The disease, however, is but slightly contagious, admitting it to be so at all, and seems to demand a long and intimate communion, as, for instance, that of sleeping or constantly living in the same room, to render the miasm effective. Yet, in the present state of the question, most judicious practitioners from the time of Galen have thought it right to follow his advice, and to caution attendants upon consumptive patients against the danger of being constantly about their persons through the whole course of the disease."

Trousseau, in 1845, writing a review in *Le Journal de Medicine*, of a work of Dr. Bernardeau, who believed in contagion, says that we ought to thank the author for having brought up again a question perhaps too lightly thrown one side, and hopes that "the communicability of phthisis may become again at least a matter of dispute."

Dr. Daniel Drake (*Principal Diseases of the Interior Valley of North America*, Vol. II., Phil., 1854) thought contagion very probable, and adds: —

"A disbelief in contagion is highly favorable to the spread of that disease (if it can really be propagated in that manner), inasmuch as in private practice, and

both civil and military hospitals, no measures of prevention are employed."

In Copeland's *Dictionary of Practical Medicine*, Vol. III. p. 1228, New York, 1859, we read :--

"Emanations from the lungs and skin of persons in the second or third stages of phthisis are certainly sometimes productive of consumption, more particularly in young persons of a scrofulous diathesis, and in those who are predisposed by other causes, or who are subjected to several concurring influences. The inhalation by the healthy of the emanations from the lungs and skin of the consumptive, and the consequent appearance of the disease in the former, may, as in other cases of infection, be productive of its injurious effects only in the circumstances now stated, but the disease is caused by infection, nevertheless, although the fact is stated loosely by many writers, as one of the propagation of phthisis by *contagion*,* and denied by others, as indeed the infectious nature of nearly every disease has been denied by some, who considered belief in infection to be *credulity*, and scepticism to be a proof of *a strong-minded physician*."

As a rule, during the first half of the present century medical writers of note, in treating of the causes of consumption, which they sometimes discussed in quite an elaborate manner, either entirely ignored the question of contagion as an exploded theory, a superannuated notion, or, if they mentioned it at all, did so only to refute it. As a sample we will quote from

* For the distinctions between "contagion" and "infection," see Chapter I.

one of the most instructive, entertaining, and widely circulated medical books ever written, Sir Thomas Watson's *Lectures on the Principles and Practice of Physic*, delivered at London in 1836–37:—

"Is phthisis *contagious?* No, I verily believe it is not. A diathesis is not communicable from person to person. Neither can the disease be easily (if at all) generated in a sound constitution. Nor is it ever imparted, in my opinion, even by one scrofulous individual to another. Yet in Italy a consumptive patient could not be more dreaded and shunned if he had the plague. And in this country the suspicion will now and then arise that the disease may be infectious. A girl dying of phthisis is nursed by her sister, who afterwards droops and dies of the same complaint. Here the presence of a peculiar diathesis is strongly presumable, but the parties may be different in blood. A wife watches the death-bed of her consumptive husband, and presently sinks herself under consumption, and there may be no traceable or acknowledged example of scrofula in her pedigree. Yet even here the latent diathesis may fairly be presumed to have existed. Very few families are perfectly pure from the strumous intermixture. The predisposition may be slight; it may be dormant for a generation; or, like other inherited peculiarities, it may light capriciously on some individuals only of the kindred. In both the supposed cases there have been other influences at work, more authentic than the alleged contagious property, in calling forth the fatal malady: watching, the want of rest, confinement in the unwholesome

air of a sick-chamber, and, above all, protracted mental anxiety, than which no single cause perhaps has more power to foster and forward the inbred tendency to phthisis. The disorder, I am satisfied, does not spread by contagion. Nevertheless, if consulted on the subject, I should, for obvious reasons, dissuade the occupation of the same bed, or even of the same sleeping-apartment by two persons, one of whom was known to labor under pulmonary consumption."

Dr. Walshe, of London, was such a well-known writer that his opinion also is well worth quoting. In his treatise on *Diseases of the Lungs*, published a few years after Dr. Watson's *Lectures*, he says:—

"I find the death by phthisis of several physicians in France known to have practised much among the consumptive (Bayle, Laennec, Dance, Delaberge) suggestively ascribed to infection (Péreyra, *Phthisie*, p. 81); but the multitudes similarly engaged, who have shown no such symptoms, are forgotten. Curiously enough, of the first three clinical assistants I had at Brompton (Hospital for Consumptives), two died of phthisis, and the third left the establishment with slight hæmoptysis, cough, and chest-uneasiness. The latter is now in perfect health; one of the former had clearly been affected before he came to the hospital. No similar case, as far as I am aware, has occurred during the fifteen years that have elapsed since the opening of the hospital.

"But the strongest argument contagionists adduce is founded on the frequent death by phthisis of the husband of a wife, herself cut off by the disease, or

vice versa. The direct aspiration of organic particles is supposed to transmit the disease. That such deaths do occur is indubitable; but in some instances careful inquiry shows the putative victim of infection was already tuberculous at the time of marriage, or that he sprang of a more or less thoroughly phthisical stock. The converse cases, long since insisted on by Portal, where a husband has had two or even three wives successively destroyed by phthisis, himself wholly escaping, are too frequently lost sight of.

"Again, the circumstances which bring on the disease in one predisposed person are likely enough to bring it on in another; and, it must be remembered, husband and wife are in great measure exposed to anti-hygienic influences of the same classes and of the same activity.

"There is, besides, the statistical argument, which has never, that I am aware of, been seriously examined. What are the chances that a given individual, either actually phthisical or manifestly fated to become so, will unite with a consort similarly conditioned? The answer will vary with locality, walk of life, and, above all, the age of the contracting persons; but the chances would, I feel positive, be considerable, if the union took place within the period of existence at which tubercle is most common. Probably one of seven or eight of young married men has a wife who will one day die of phthisis, according to an estimate made by Dr. John Beddoe. Grant, then, that the male is tuberculous, and I apprehend we have a calculable frequency of this form of double phthisical death quite as high as any

that has been actually observed. If this be actually so, the influence of contagion remains anything but proven.

"But even though the reality of infection be logically unproved, it is wise to segregate the actively phthisical as far as possible; the inspiration by the healthy of the exhalations of the diseased cannot fail to be positively, though not *specifically*, injurious."

Dr. Henry I. Bowditch, of Boston, in an article on the subject in the *Boston Medical and Surgical Journal*, May 26, 1864, narrates eight cases in his own practice, and from them deduces the conclusions which we give below: —

"*First.* — Consumption is not *contagious* * in the usual acceptation of that word.

"*Second.* — It may be *infectious*,* and to this extent only. By long attendance of the closest kind, by inhaling the breath of the phthisical patient, by living in the phthisical atmosphere, so to speak, and in general by a neglect of hygienic laws during such attendance, the health may be undermined and phthisis set in. How far the depressing passions may have an effect, it may be impossible to say; but that they exert an important and deleterious influence, we cannot deny. Doubtless the want of exercise and the neglect of all hygienic laws have an immense influence. But as we see all these influences, except the sup-

* By reference to Chapter I., it will be seen that the distinction between the words *contagion* and *infection* is often a very loose and unimportant one, and that there are a *great many* "usual acceptations" of this distinction. Used as a synonyme of "communicable," Dr. B. would undoubtedly have no hesitation in applying it to consumption.

posed infection, existing often for a long time without producing phthisis (for example, a long attendance on patients ill with other non-tubercular diseases), I do believe that, in some few instances at least, we must admit a degree of real infection in our estimate of the matter. Especially must we take this into consideration in the case of a sister or a brother in a family, where the hereditary tendency to phthisis is strong; for in this case I doubt whether it would be safe to allow the sister to attend as nurse, even with the best moral and physical hygienic influences.

"I am confirmed in this view by two more considerations. We see cancer and other chronic diseases have never even suggested this idea of contagion to the medical profession, at least so strongly as phthisis has. Is not this an argument in favor of the idea of there being something peculiar in tubercle, whereby it is capable, at certain periods of its development, of producing its kind? Second, I deem of some importance the positive statement made to me by a husband, the sole male patient among our cases. He distinctly asserted that the first real cough arose when he was raising his wife in bed, and consequently was making an effort, and drew in a deep breath, inhaling at the moment when his wife was exhaling. The chief part of the air he drew in was loaded with emanations from her lungs. As she was at that time in the last stages of phthisis, those lungs were probably ulcerated, and possibly some irritating matters may have escaped with the expired air, and have been inhaled by the husband.

"While we may feel assured that contagion, as held by Morgagni and the Italians, is, at least in this country, a delusion, we may feel, I think, equally assured that we should warn a wife or a sister, or near female friend, from devoting herself too closely to the attendance upon a consumptive husband, sister, or friend. For the sake of the attendant as well as the sick one, sleeping in the same bed, or even in the same room, should be avoided. If, however, this cannot be prevented, then we should try to eliminate all deleterious influences as much as possible. The room should be thoroughly ventilated and cleansed daily. The attendant should eat regularly and pay especial attention to the digestive system. The diet should be nourishing and simple. It would not be amiss for her to take some wine daily. She should walk daily out of doors, and not confine herself to the sick-chamber week after week without release. In active attendance she should beware of inhaling the breath of the invalid. Disinfecting agents might, with advantage, be used in the spittoons. In a word, every good hygienic influence should be brought to bear upon the attendant. At the same time I would have the community understand that with such care there is scarcely a trace of danger in any case save that of a sister, or a wife whose husband is so miserably exacting or thoughtless as to demand that she should sleep with him during his illness. The danger of the health being injured in these latter cases is great, even if the above hygienic rules be strictly adhered to. The best plan is not to allow a sister or a wife ever to take the

sole charge of the invalid. Whenever it is possible, let a regular hired attendant assume all the harder work, and sleep in the room, if need be, at night, or still better, in an adjacent room. The wife or sister should be in the room only during the day, and even then with proper and regular intervals of removal to the fresh air. These rules it may often be impossible to carry out; still oftener they *will* not be. Nevertheless, I believe our cases fully show that they are simply rational and right, and rules that the contagionist and anti-contagionist ought alike to adopt."

In 1865 Dr. MacCormac published, at London, a book on consumption, in which he strongly advocated the "air re-breathed" theory; that *the* cause of the disease is the breathing over and over again of the air which has already been used by the lungs of the same or some other person or persons, both or all of whom may have been previously healthy. This some have considered the strongest argument against contagion.

Dr. William Budd, in an article on the nature and propagation of phthisis, in the London *Lancet*, Oct. 12, 1867, takes strong ground for contagion. He concludes that "tuberculosis is a true zymotic disease of specific nature, in the same sense as typhoid, scarlet fever, typhus, syphilis, etc., are; and that, like these diseases, tuberculosis never originates spontaneously, but is perpetuated solely by the law of continuous succession."

The evidences of this he finds in,—

(*a*) "Considerations based on the pathology of phthisis, consisting in the evolution and multiplication

in the organism of a specific, morbid matter, with a tendency to elimination, and casting off of the same, like zymotic diseases generally.

(*b*) "Actual instances in which there is evidence to show communication from one to another.

(*c*) "The geographical distribution of phthisis in past and present times, and especially its fatality now in countries which were entirely free from it, when first discovered by Europeans.

(*d*) "Its greater prevalence in low levels and crowded communities, and entire absence, except by importation, at high levels, — the same conditions which govern zymotic diseases.

(*e*) "Its high rate of prevalence in convents, harems, barracks, penitentiaries, etc., *i. e.*, in the same social conditions known to propagate zymotic diseases."

As facts for his statement about geographical distribution (*c*), he adds that when the South Sea Islands were first discovered, there was no phthisis there; but that since the aborigines have come into contact with Europeans, the disease has become so wide-spread as to threaten their extermination. This is a striking contrast, only to be explained, he thinks, by the importation of a new and specifie morbid germ.

He further says that the late Dr. Rush, of Philadelphia, who made accurate inquiries, satisfied himself that there was no phthisis among the American Indians when America was discovered, whereas now it is very common and very fatal among them.

Furthermore, in Africa, everywhere along the seaboard, where the blacks have come into constant and

intimate relations with the whites, there has been a large mortality from the disease; but in the interior, where there has been only occasional contact with a few great travellers, the disease has not been found. Of this fact Dr. Livingston and other African travellers have given Dr. Budd positive assurance.

As carrying out the same line of argument that Dr. Budd suggested (*c*), we quote the following communication from Herbert L. Snow, M. D., London, to the *British Medical Journal* of Aug. 21, 1880, headed "The Contagiousness of Tubercle":—

"Taken in connection with your recent article on Cohnheim's views, the following passages, which I extract from De Quatrefages' *The Human Species*, pp. 428–430, seem to me of special interest to medical men.

"'. . . the strange and fatal influence which the white race seems to exercise upon certain inferior races, whose territory it has invaded. Nowhere is this melancholy phenomenon more striking than in Polynesia.

"'In the Sandwich Islands, Cook calculated the population at 300,000. In 1861 there were but 67,-084. In New Zealand, Cook found 400,000 Maories. In 1858 there were only 56,049 remaining. From a comparison of the estimates of Cook and Forster, it appears that the population of Tahiti must have been at least 240,000. In 1857 the official census gave only 7,212. These facts . . . are universal.

"'Not only does the rate of mortality increase in this unfortunate Polynesian race; there is also a decrease in the number of births. In the Marguesas

Archipelago at Tairo-Hal, M. Jonan saw the population fall in three years from 400 to 250, during which time only three or four births were registered. In the Sandwich Islands, from among eighty women legitimately married, M. Delapelin found that only thirty-nine had children. There were only nineteen children in the principal families of chiefs. In New Zealand, says M. Colenzo, marriages are rarely fertile. The seven principal chiefs of Almiri are without children, with the exception of Te-Hapuku ; but of the four married sons of the latter, three are as yet without a family. Nine out of eleven marriages were here barren.

"'Two naval surgeons, MM. Bourgarel and Beruelfert, have alone been able to throw some light upon this melancholy problem. The former found that tubercles were invariably present in the lungs of bodies submitted to post-mortem examination. The latter tells us that almost all Polynesians suffer from an obstinate cough, and that in eight cases out of ten tuberculosis follows these bronchial catarrhs. *Now phthisis does not appear in the list of diseases drawn up by the old voyagers.* Have we, then, imported it into these islands? Developing in a new region, in a race to whom it was formerly unknown, this disease assumed a more terrible form, with examples of which we are acquainted.'"

Dr. Parkes, in his large work on *Practical Hygiene*, referring to the fact, now conclusively demonstrated, that purulent and epithelial cells are always floating about in the air, where numbers of persons are to-

gether, says: "Considering that the pleuro-pneumonia of cattle is probably propagated through the pus and epithelium cells of the sputa passing into the air-cells of other cattle; that even in man there is some evidence of a pneumonic phthisical disease being contagious (Bryson, *Cases in Mediterranean Fleet*), the floating of these cells in the air is worthy of all attention. It may explain some of those curious instances of phthisis being apparently communicated."

Dr. Julius Peterson, district physician in Copenhagen, after investigating the subject in 1869, was satisfied "that the discrepancies existing in the views of European physicians on the subject are connected with well-marked climatic differences. That a contagious origin of some cases of phthisis cannot, on sufficient grounds, be denied. That phthisis caused by contagion is, in general, of a very dangerous and inflammatory character; that it must justly be considered hazardous to sleep in the non-disinfected bed of a phthisical patient, and to be habitually in too close contact with such a person; that this danger in Denmark seems to be greatest in the warm period of the year." (Aitken.)

Dr. Clymer writes: "That the tuberculous diathesis may be transmitted from the male to the female by the medium of the fœtus, and this even in such a way that children begotten by a second non-tuberculous husband may inherit the diathesis acquired from the first, or tuberculous husband, would seem likely, from the many cases reported by Dr. Perroud, of Bordeaux (*De la Tuberculose*, Paris, 1866), and by Dr. Alexander Harvey,

of Aberdeen. . . . Dr. Villemin suggests that besides the direct transmission, as by cohabitation, consumption may be contracted through indirect means, by clothes, bed-linen, water-closets, the vitiated air of rooms lived in by tuberculous persons, etc. The possible transmissibility of the disease in this manner merits, he thinks, the attention of medical officers of the army. A tuberculotic soldier dies in the hospital, and his clothes are returned to his company and worn by another; may not this, he asks, be one source of phthisis in the army? He is satisfied that the barrack is to the soldier in the production of consumption what the regimental stable is to the horse in the development of farcy, the contagion and transmissibility of which are at length accepted.

"Dr. Jules Guérin (in the discussion on the subject in the French Academy of Medicine) believed that crude tubercle can never be contagious, but that when it is softened, and the ulcerated lung surfaces are exposed to the air, the patient may become a source of infection to those about him, just as the pulmonary lesions he has may affect his own organism by the resorption of purulent and putrid products."

On the other hand, again, Dr. Richard Payne Cotton, senior physician to the Hospital for Consumptives at Brompton, comes out very strongly against contagion. He says, in an article in the *British Medical Journal*, Aug. 31, 1872:—

"The Brompton Consumption Hospital was opened in the year 1846, with ninety beds. Ten years later it was completed; and since that time two hundred

beds have been constantly occupied. We have lost, during that long period, only one nurse from phthisis, and this was a poor creature whose husband had deserted her, and who had long endured, from other causes also, considerable mental anxiety and physical exhaustion. On the other hand, the services of the nurses generally have been unusually prolonged, and I can myself testify to their general health being, as a rule, remarkably good. Of those *now resident*, two have been at duty in the hospital seventeen years, one has resided thirteen years, one eleven years, two ten years, two nine years, one seven years, one four years, two three years, and four two years. The two oldest nurses have lately died of old age and general decay, after having long been superannuated. Each of these had resided in the hospital for upwards of twenty years.

"Of the gallery maids, whose duty it is to be much within the wards, in sweeping and scrubbing the floors, only one has been known to have been affected with phthisis, whilst it is obvious, that during so long a period, the numbers of persons thus employed must have been considerable.

"Our engineer has seen eighteen years' duty within the hospital, and he is now in good health.

"We have had, at different times, three attendants in the *post-mortem* room. The first of these is still living, but in infirm health, the result of intemperance; the second left with spinal disease; the third has been in his office for nearly four years, and is in good health.

"Of the dispensers, one who kept to his post for above ten years is living and well; three who have held office in the dispensary since the year 1867 are also well; and the present senior dispenser has been with us for ten years. Many others have been in this department since the opening of the hospital; but only one has been known to have been consumptive.

"The resident clinical assistants of whom we are able to obtain reliable information amount to seventy-eight in number. Three of these are said to be phthisical, but only one has been known to have died of phthisis. This gentleman I knew intimately. He was always of consumptive appearance, and one of his sisters died of phthisis.

"Our resident medical officer, Mr. Edwards, to whom I am indebted for the statistics I am now giving, has held office for more than twenty-one years, and all those who know him can testify both to his continued health and his undiminished energy and usefulness. The present matron has also been in her office for more than twenty-one years, during which long period her health has been excellent.

"The present secretary has been at his post for fifteen years, and his only predecessor, who is still alive and well, had held the same office for sixteen years. The assistant secretary has been at his duties within the hospital for eighteen years; and a clerk — whose office it is to register the out-patients, and who must, on that account, be exposed to an unusually great extent to phthisical contagion, did such exist — has been at his work for five years. I am happy to add that all of the above are now in excellent health.

"The present chaplain has been with us for twenty-two years, and his two predecessors are still living. Happily, all of them are well.

"Of the staff of physicians and assistant physicians — nineteen in number — one only has been affected with phthisis, and he was a young man of delicate and decidedly consumptive aspect. The period of office which some of us have had is unusually long. Of the present physicians, two have been upon the staff twenty-four years each ; one for seventeen years ; one for thirteen years; one for nine years; and one for five years. The late senior physician, who resigned only three years ago, had been attached to the hospital, and in constant work there from its very foundation. Two of the former physicians who resigned their appointments, and subsequently died of disease quite distinct from phthisis, had held office in the hospital for fifteen and nineteen years respectively.

"I have elsewhere given my experience of the statistics of phthisis, in one of our chief metropolitan general hospitals; and the subsequent observation which I have had only tends to convince me the more that the evidence in this respect is greatly in favor of the Hospital for Consumption.

"With the above facts before us, must it not appear to all believers in the doctrine of phthisis originating in a special and contagious poison, that a residence in the Consumption Hospital, and long-continued working in its wards, is a very good way indeed *not to catch the disease?*"

In the fourth *Annual Report of the State Board*

of Health of Massachusetts, for 1872, presented to the Legislature in January, 1873, and printed at the expense of the State, is an interesting article entitled "Analysis of a Correspondence on some of the Causes or Antecedents of Consumption," by Henry I. Bowditch, M. D., chairman of the Board. Following a plan which he had pursued in other investigations, he prepared a circular and list of twenty questions, to which a monosyllabic answer might be given, although more detailed replies were invited, and sent them to prominent physicians in active practice in various places, but mostly in New England. Two hundred and ten of the physicians so addressed politely filled out their blanks and sent in their returns. One hundred and forty-two lived in Massachusetts, and sixty-eight were scattered through Maine, New Hampshire, Vermont, Rhode Island, Connecticut, New York, Pennsylvania, Illinois, Michigan, London, and Germany. These twenty questions embraced the subjects of heredity, drunkenness, overstudy or overwork, special trades, marriage and child-bearing, contagion, moisture, etc., etc., and the answers to them were supposed to be based on every-day experience rather than on books. Out of the two hundred and ten physicians who sent replies to these questions, twenty-eight, from one reason or another, probably from lack of experience in this direction, or because their attention had never been drawn to it before, did not feel able to answer that on contagion. Of the rest, *one hundred and ten* answered affirmatively, forty-five negatively, and twenty-seven were doubtful.

This is a pretty large majority in the affirmative, when we consider how the vote would probably have stood twenty or more years before. Dr. Bowditch writes: —

"Evidently those who believe in contagion or infection are not so numerous or so sanguine as they are upon some other questions submitted to them. May not the fact of the hitherto great prevalence of the opinion of the non-contagiousness of this disease among English and American practitioners, and our strong belief in the hereditary character of it, have led us all to ignore what may, after all, prove a potent cause, and which we shall recognize on more close inspection? The question is of much importance."

Many physicians, not content with simply the monosyllabic "*yes*" or "*no*," wrote, in addition, something as follows: —

"I am a firm believer that consumption is a contagious disease, much more so than is generally believed. I have in my mind several cases where there was almost positive evidence of contagion." —

"In very many cases, I have the opinion, from my own observation, that consumption is communicable by contagion or infection." —

"I am more inclined than I was at one time to attach importance to the influence of contagion." —

"I am thoroughly convinced that phthisis is frequently caused by contagion, and deserves to be classed with typhoid fever in this respect. I have seen unmistakable evidence where a healthy wife contracted the disease from sleeping with her husband suffering

from that disease, and *vice versa*. So much am I convinced of the truth of this statement, that I always forbid a healthy man from sleeping in the same room with a phthisical patient."

Prof. Ludwig Buhl, of Munich, whose pathological researches have excited so much attention, and whose infection theory of tuberculosis was the "groundwork and foundation of Niemeyer's structure," in his *Twelve Lectures on Inflammation of the Lungs, Tuberculosis and Consumption*, American translation, New York, 1874, p. 117, says:—

"In addition to these points, we must regard as favorable to the [auto-] infection theory the fact that *tuberculosis can be conveyed* from *one human being to another*. I do not mean the inheritance by a child from its parents, but the immediate communication through contagion; both that which takes place from one part of the body to another, and from one individual to another. This is shown in the examples adduced by Rindfleisch (p. 349), where tuberculosis was conveyed over a limited space from the pleura pulmonalis to the pleura costalis; also in a case which I met with, where a true tubercular ulcer on the edge of the tongue communicated itself to the adjacent mucous membrane of the cheek.

"So, also, additions are being constantly made to the cases where the contagion has been conveyed from a husband to the wife, or *vice versa*, though this might perhaps be explained on the same ground as the inheritance from parents. This also agrees with the well-known contagiousness of glanders, which is only a

very pronounced form of miliary tuberculosis, and with the course of general syphilis. This latter is an analogy which we must not overlook, since the histological characteristics of the syphiloma are so similar to those of the lymphoma, as to be readily confounded with them."

Dr. W. H. Webb published an article in the *American Journal of the Medical Science*, for April, 1878, taking strong grounds on the side of contagiousness. While preparing his article, he had written to several distinguished physicians, asking them to favor him with their views on the subject. The following communications were some of the answers which he received: —

Prof. Alfred Stillé writes to Dr. Webb: —

"While Dr. C. T. Williams concludes that the disease is not infectious, the vast experience and sagacity of his father, Dr. C. J. B. Williams, led him to declare that both reason and experience indicate that a noxious influence may pass from a patient in advanced consumption to a healthy person in a close communication, and may produce the same disease. The latter is my own opinion, and I always feel it my duty to advise that a consumptive's bed should be shared by no one."

Prof. J. M. Da Costa writes to Dr. Webb: —

"I have met with a number of instances which seemed to prove the contagiousness of phthisis. I am a believer in this, although I admit the great difficulty of eliminating the law of coincidence in a disease as common as tubercular phthisis. To mention a few of the instances I have met with: —

(1.) "I attended a gentleman of tuberculous family, and himself suffering from very slowly developing consumption, which, in truth, was arrested for a number of years. He thrice married, and lost his three wives by consumption. The third was a woman of splendid physique, and of a very healthy, long-lived family. She was the mother of three children, one of whom is scrofulous.

(2.) "The case of a young woman twenty-six years of age, in whose family the patient assured me there never had been a case of phthisis. She died sixteen months after her husband, who had been a slowly progressing consumptive. She left two children.

(3.) "A singular case in a splendid-looking young woman, who most faithfully nursed a tuberculous husband for nearly two years. She died a year afterwards of phthisis, beginning, apparently, with throat and bronchial irritation. She had, I think, no children.

(4.) "A young woman, who accompanied her husband to Colorado, where he died a year ago of a slow consumption. She is tubercular now; no case of phthisis has been known in the family except that of her mother's aunt. One of the children of the young widow died of a scrofulous affection.

"I might give you many more examples, and I have noticed the fact that they chiefly happen in women."

Dr. J. Solis Cohen, M. D., writes to Dr. Webb: —

"I am strongly impressed with the opinion that phthisis can be contracted, that is to say, is communicable (rather than contagious, in its strict sense) from frequent, continuous contact with the phthisical."

Dr. Edgar Holden, of Newark, N. J., independently of Dr. Webb, and without his knowledge, had been investigating the subject, and published his results in a paper in the next number (July, 1878) of the same journal. He also argued strongly for the contagion theory.

He had sent out about five hundred circulars to good physicians in every county, and almost every important town, in the United States, to which he received two hundred and fifty answers, which showed an almost equal division of opinion. He ascertained that those whose attention was much given to pulmonary diseases quite frequently inclined to a belief in contagion, while those, however eminent as scientific men, who were interested especially in other directions, often doubted it. Speaking more definitely, *one hundred and twenty-six* physicians answered *yes* (seventy-three of whom were emphatic, and gave cases), *seventy-four* answered *no*, and *fifty* were in doubt, or had had too little experience in lung diseases to form an opinion of any kind on the subject.

A few of the replies we quote.

Dr. Todd, of Indianapolis, Ind.: —

"My observation during a period of twenty-four years' active practice, long ago convinced me that such was the case (*i. e.*, the disease is infectious). I have known a number of instances where parties, with no hereditary tendencies or predisposition, have, after long and intimate association with those affected, themselves fallen victims."

Dr. Inglis, of Detroit, an acute observer, after say-

ing that he had not seen indubitable evidence of contagion in his practice, continues : —

"I have, however, in the words of Professor Frantzel, in the Vienna General Hospital, seen such striking cases, explicable in no other way, as to make me believe that the disease is communicable when persons are confined to constant inhalation of the breath of consumptives without sufficient out-door air and exercise."

Referring to the answers to his circulars, Dr. Holden states that, "the instances adduced [by his correspondents] of wives contracting consumption from their husbands, when the careful investigation of family history showed a remarkable freedom from hereditary taint, and the same of husbands contracting the disease from their wives, are too numerous to mention. Equally numerous, also, are the cases where the disease once started in a family appeared to affect one member after another ; and although such cases at once suggest the development of a diathesis under circumstances common to all the family, and therefore not suggestive of contagion, it is by no means certain that they are not valuable in answering the question propounded."

Dr. Holden himself started in practice with the idea that consumption was communicable only from parent to offspring, but has finally been forced to the following conclusions : —

"I can enumerate at least a score of cases which have been watched with this very object in view, and in which the result has been the same. Wives after

husbands, husbands after wives, intimate companions and faithful nurses, who slept in the same bed, or wore the same clothing, have fallen victims. Of course, it is not to be intimated that all, or even a majority, can be affected by contagion any more than in those diseases now indisputably contagious. Malignant scarlet fever, diphtheria, and cerebro-spinal meningitis are braved by faithful nurses and friends, who escape in *more* than a majority of instances. To prove contagion now, is no easier than when [the non-contagiousness of] erysipelas had its defenders, and the records of the surgical wards of the hospitals of Paris, and Berlin, and London, had accumulated a fearful mortality ere the obdurate prejudices of the profession were awakened to a new belief."

The most interesting cases which Dr. Holden quotes, one by Drs. Kittoe, of Galena, Ill., and Bennett, of Danbury, Conn., and another by Dr. McDowell, of Kentucky, will be found in Chapter IV.

In 1879, Dr. de Musgrave Clay, of Paris, published an interesting pamphlet, entitled *Étude sur la contagiosité de la Phthisie Pulmonaire*, to which, also, I here acknowledge my obligations for valuable material.

Other authors will be mentioned here and there, in appropriate places.

CHAPTER III.

CONTAGION AMONG CATTLE.

So far, in the consideration of this subject, we have confined ourselves strictly to its relation to mankind. Although it is not always safe, in the prosecution of scientific inquiries, to draw too close a parallel between the lower animals and man, yet it is very interesting to us, in conducting our investigations into the contagiousness of consumption in the human race, to learn that several *veterinary* surgeons have for a long time insisted that it was propagated by contagion among the *bovine species;* one animal acquiring it either from inhaling the breath of a diseased fellow, or from eating food that has been contaminated by his secretions. Dupont, of Bordeaux, for instance, committed himself to this belief twenty-five years ago, and Cruzel, in his book on diseases of cattle, writes: — *

"Much has been stated with regard to the danger animals incur by being kept in low, narrow, and badly ventilated buildings, and this danger is real; it exists,

* Translated by George Fleming, M. R. V. C. S., in an excellent article in the British and Foreign Medico-Chirurgical Review for October, 1874, to which I am indebted for many facts about tuberculosis in cattle.

and may be remarked every day. But all has not been said with regard to this matter when treating of tubercular phthisis. If in a cramped, low stable, in which several milch cows are lodged, there chances to be one with this disease, and if the tubercles are in a state of ulceration and suppuration, so that the air expired from its lungs is very fetid, then this air inspired immediately by another cow carries into its healthy lungs the tubercular infection. . . . It is in this way that tuberculosis is communicated by the expired air, and the facts which support this opinion are numerous. Those veterinary surgeons who practise in the country may gather them every day, and I may more particularly call their attention to the following circumstance: Two working oxen or cows are brought into the same building, and they eat their forage or ration, or whatever it may be, out of the same rack or manger; lying in the same stable, they respire nose to nose. One is perfectly healthy, at least so far as appearances go; so is the other, except that it coughs from time to time, and its breath is fetid. Soon it is perceived that the other animal, which does not cough, and whose expirations are not fetid, if at first it ate with appetite, shows less desire for food out of the usual receptacle, hangs back from it, and neglects it; and, to make it approach the rack or manger, one is obliged to push it, to excite it with the hand, and even to beat it with a stick. Nevertheless, this animal does not *appear* to be affected with disease, nor even to be indisposed; and if it refuses to remain on the same spot as its comrade, it is because it is desirous

of getting away from its fetid breath. The cow-men can scarcely be deceived; and if by chance there is an opportunity for selling the diseased animal, or if it can be fattened for slaughter, and its companion gets another healthy comrade like itself, then its appetite quickly returns, and no further inconvenience is observed. But if, on the contrary, the situation is not altered by the separation of the two animals, phthisis runs its ordinary course in the one affected, and the other, at first becoming emaciated, is finally attacked by the same disease. After this, if we do not wish to recognize in such a case the effects of contagion, we may attribute them to cohabitation, infection, or anything we like; but we cannot hinder the fact from being present, nevertheless, and to see it reproduced very frequently in the circumstances I have just alluded to."

Fleming goes on to express the views of other veterinarians of celebrity as follows: —

"Viseur,* of Arras, in 1868, endeavored to explain the persistence of the malady in certain establishments, well kept in every respect, but in which the cattle were slowly decimated, and in which hereditary transmission could not be included as a cause of the mortality, as breeding was not carried on in them. In some of these places the malady had lingered for years, and had affected animals of all ages, but not until after they had been a certain time in the sheds, and in contact with the consumptive milch cows."

"Zundel,† one of the most talented of the conti-

* Recueil de Méd. Vétérinaire, 1873, p. 881.

† Recueil de Méd. Vétérinaire, 1874, p. 93.

nental veterinarians, has also commented on the frequent persistence of bovine phthisis in Alsace, in stables well kept and carefully ventilated; and this persistency, he is inclined to believe, can only be attributed to contagion. He alludes to certain establishments in the vicinity of Mulhouse, where, notwithstanding the greatest care in purchasing, and good hygiene and regular feeding, the healthiest animals become affected with the disease after dwelling for a certain time in the stables. Most frequently these are the strong and vigorous Swiss cattle, which, in the words of Zundel, 'do not pay their tribute to acclimatization, as certain people appear disposed to believe, but are in the stables under the influence of a contagion that the owners cannot comprehend, and look upon as a fatality to which they must submit. And yet other animals from the same country, and kept in sheds often less attended to, remain healthy for years.' 'Is not this a proof,' he adds, 'that acclimatization goes for nothing in the mortality attending the first, and that it must be attributed to some circumstance inherent in the places where these accidents occur?'"

"The evidence of M. Grad,* veterinary surgeon at Wasselonne, Alsace, is still more significant and decisive, as it not only furnishes us with clinical observations, but also an experimental fact which adds a great value to the others, while it receives additional weight from them. The observations are guaranteed as perfectly authentic, and were collected during a

* Recueil de Méd. Vétérinaire, 1874, p. 94.

period of twenty-three years' practice as a veterinary surgeon. At the commencement of his professional career, Grad does not appear to have believed in the possibility of tuberculosis being transmissible amongst bovine animals by cohabitation, but the close observance of a series of facts at length confirmed him in the opinion that it was. In order that the circumstances in which the transmission of the disease was most frequently noticed might be understood, this veterinarian describes the arrangement of the cattle-sheds of the farmers; and it would appear that the animals are generally kept in very short stalls, which completely separate the animals from each other, from the rack and manger, to one third the width of the shed. This arrangement, while preventing the cattle from injuring each other with their horns, as they cannot reach one another with their heads, also permits their allowance of food to be given separately. On different occasions, owners had informed Grad that they had lost two, three, and four animals *in the same stall*, from consumption, — marasmus, accompanied by cough At first he did not attach any importance to these declarations, either because he thought the mortality occurring in this way was merely accidental, or was due to hereditariness, which he acknowledges plays such an important part in the production of the malady. However, one day, when visiting the stables of an extensive farmer at Leinheim, he was informed that every year for five years one of the cattle had died from phthisis; 'and what is very curious,' said the farmer, 'this always happens in the same stall.'

"In proof of this, Grad was shown a young cow, which, on examination, offered all the symptoms of tuberculosis; such as excessive emaciation, skin clinging closely to the bones, frequent and feeble cough, etc.

"Its value in this condition was from thirty to forty francs. According to the farmer, the animal had been ten months in the stall, and when first placed therein it was in flourishing health; it was the fifth cow that had fallen into the same state in this stall. Grad's attention could not but be strongly aroused by this report; nevertheless, he expressed his opinion that an hereditary tendency was doubtless the efficient cause in this succession of losses. Such, however, was not the opinion of the farmer, and it was at length agreed that Grad should choose a cow from another stable and put it in his stall, as an experiment. With this object a cow three years old, and in calf, was selected; it had every appearance of perfect health, had been bred on the farm, had never been unwell, had never coughed, and none of its progenitors had been affected with tuberculosis. It was moved into the stall, and remained quite well until after calving, when a short cough was the first symptom observed. This cough increased in frequency, and emaciation gradually set in, with the usual *cortége* of symptoms accompanying tuberculosis distinctly marked; so that in about twelve months the creature was only the shadow of its former self, and was sold for a trifle, like the others which had preceded it in the stall, to the low-class butchers. Grad could no longer resist the evidence in support

of this cause, this being the sixth animal attacked with tuberculosis in the same stall; and he inferred that in all probability the disease had been transmitted by the ingestion of the matters expectorated by the cattle which had previously inhabited the same place.

"On his recommendation, all the woodwork of the stall was removed, the manger and rack were thoroughly disinfected, and the spot left unoccupied for a certain time.

"When the stall was rebuilt, and again occupied by several animals in succession, tuberculosis made no more victims either in it, or in other parts of the stable.

"This observation was made fifteen years ago, and since then Grad's opinion as to the transmissibility of tuberculosis has, he asserts, been amply confirmed; and the examples he could adduce of the conveyance of the disease by inhabiting a *contaminated stall* are numerous, and as patent as that just related. For a number of years he has recommended that the places occupied by phthisical animals should be disinfected; and in no instance in which this was adopted had cases of transmission occurred, no matter how numerous they may have been previously. Grad also mentions that a number of people in his district believe in the possibility of transmission of tuberculosis by cohabitation, and he has no doubt that experience or observation has taught them this. He states that the observations he could report as to the transmissibility of tuberculosis by direct cohabitation — animals standing beside each other — are not so frequent or conclu-

sive, because of the arrangement of the stalls, which completely separate the cattle towards the head; so that the expectorated matters stand but little chance of being ingested by those placed in the stalls adjoining a phthisical cow. In those sheds in which the cattle are not so kept apart, cases of transmission have been noted, but the observations were not sufficiently rigorous to establish a certain conclusion upon. He adds, however, 'In my opinion, this transmission is possible as soon as tuberculosis has set in at an advanced stage, and the expectorated matters are ingested by other cattle.'"

CHAPTER IV.

REPORTS OF ILLUSTRATIVE CASES.

CASE 1.

(Translated from Vialettes, *Thèse de Montpellier*, No. 44, 1866.)

JOHN A. was born at St. Martin's, in London, of consumptive parents. His health was delicate in infancy and youth. Towards the age of puberty he had a slight hemorrhage from the lungs. At the age of about twenty-five he married Antoinette A., and soon after the symptoms of the terrible disease whose germs he had in his system, showed themselves to such an extent as to remind him of his parents' premature end. His sad presentiments were fulfilled. Having struggled against the disease for some months, he died in 1856, greatly emaciated. His wife was assiduous in her care and devotion, and no one would have thought that only a few years later she would be compelled to yield to a similar malady. She was born of healthy parents, and in her youth enjoyed the best of health. Her father died from a brain disease, at the age of fifty-seven, and her mother at the age of seventy-one, from a "catarrhal fever." Of this union there was born a child, who died, when fourteen

months old, from meningitis, the tubercular nature of which was only too probable.

Some time after her child's death, Antoinette was united in second wedlock to Peter R. He was thirty-two years old, and his parents died at an advanced age, — his father in his sixty-ninth, and his mother in her seventy-fifth year. There was not a suspicion of a hereditary taint in the family. He had a sister, who is still living; and their strength, and everything in their appearance, especially the size of their chests, pointed to a freedom from disease in their respiratory organs.

Scarcely two years after marriage Antoinette experienced the first symptoms of the disease of which she died fifteen months later, in 1859. Her husband, Peter R., began, in his turn, to cough and spit, and at the expiration of two years succumbed to a clearly defined consumption.

But the list of victims does not end here, and in this sad experience are facts which appear to me to be most striking. During the last ten months of her illness Antoinette was cared for by one of her nieces, Marguerite, who had lost her father at the age of forty-seven, from the effects of a fall. Her mother, aged sixty-nine years, is still living.

Marguerite had two sisters, one of whom was married about ten years ago, both perfectly healthy. As to herself, of strong constitution, she had successfully weathered all the storms of infancy and youth. We do not want to exaggerate, but believe that if from our villages a type of perfect health was to be chosen,

all eyes would be turned upon her. Moreover, what I am telling is known to everybody in our country towns, for these occurrences are of recent date.

She had been married for eight years to one of the most vigorous of our countrymen, Joseph B., and five children were born from their union. For ten months, like an excellent sister of charity, she was lavish in love and devotion to the poor consumptive. Breathing the same air, often sleeping with her, always in contact with the exhausting sweats which bathed the sufferer, frequently dressed in her clothes, she identified her own existence with that of the sick woman; and when the latter was dead, after closing her eyes, she remained for hours with her forehead resting on the head of her whom she had been in the habit of considering as a mother, and who was still bathed in the cold sweat of the death agony.

Some time after, a paleness, causing from day to day in her friends more uneasiness, succeeded her brilliant complexion. Her appetite diminished, she grew thin, and began to cough, while dull pains extended from the sternum to below the shoulder blades. She, however, making light of her condition, struggled on with her work.

By and by came sharper pains, a complete loss of appetite, a feeling of suffocation at the least fatigue, and a nervous, wearisome cough. Next followed profuse night-sweats, and purulent expectoration, and at length Marguerite died, Dec. 15, 1860, in the last stage of consumption.

Her husband, Joseph B., had arrived at the age of

thirty-seven, without having ever experienced the slightest illness. His great size, very broad shoulders, and extraordinarily large chest, seemed to oppose the idea that a pulmonary consumption could attack so vigorous a constitution. Not wishing to believe in the diagnosis which the physician had made of his wife's sickness, he shared her bed until a week before her death, not leaving it then, he said, because she was consumptive, but that he might fatigue her less.

Nine months after his wife's death, this vigorous man began to cough, then to spit. At first it was viscous matter, which later resembled broken rice. His appetite failed, and he no longer carried to his daily work that degree of strength and energy of which he had been so proud. Difficult breathing and palpitation soon followed, and then, justly frightened, he appealed to my father's experience and sought his care. When he submitted to auscultation, we were sadly struck with the indications of disease which it afforded. Percussion produced a dull sound under the left clavicle, contrasting with the healthy resonance of all the other parts of the chest. Moreover, this dulness corresponded with an aching pain extending from this point to the upper part of the shoulder blade behind. The sick man began to have night-sweats, and when he saw in himself the same symptoms which his wife had shown, he became a prey to the most sinister presentiments. We tried to relieve his mind, and to combat the symptoms which presented themselves, by suitable treatment. But neither our efforts, nor those of Professor Courty,

to whom the sufferer applied, and who requested our colleague, M. Abbott, to place three blisters below the left clavicle, were successful. Having suffered grievously more than a year, Joseph B. died, in the beginning of 1863, greatly emaciated.

CASE 2.

(Vialettes, *Thèse de Montpellier*, No. 44, 1866.)

Louise D. was born at Fayet, in the department of Aveyron. Her father and mother are still alive, the former being seventy-eight, the latter seventy-two years of age, and still with the unimpaired health they have always enjoyed.

There is in our patient's family, she says, a remarkable tendency to longevity. The idea of a pre-existent diathesis in their ancestors was consequently unthought of. She had eleven married brothers and sisters, all of whom inherited the vigorous constitution which seemed to be the portion of the family. The seventh of the twelve children, she experienced, in her youth, none of the maladies so common to early childhood; no swelling of the glands, showing a scrofulous tendency, no obstinate catarrh, bronchial ulceration, or sudden and frequent inflammations, indicating a predisposition to this disease. Arrived at the age of puberty, the menses appeared with no trouble or pain, and afterwards continued so.

At the age of twenty-six years, she was married to a young man of Fayet, whose parents had died of consumption. Her husband had inherited the germs

of the disease, and some time after threatening symptoms appeared. Under the influence of suitable treatment and diet, the case improved, and the tuberculization was, for a time, arrested; but one year after, in consequence of a cold, he had an ominous cough, followed by several hemorrhages. Then came all the sad symptoms of consumption: emaciation, short breath, night-sweats, hectic fever, etc. A diarrhœa which could not be stopped, giving us an idea of the general diffusion of tubercles, was the sign of his fast-approaching end.

The young wife, not wishing to add to the sufferer's uneasiness by any proceeding which would seem singular to him, shared his bed until the very last, contrary to the physician's advice. A year passed, and her health, as flourishing as ever, seemed to set at naught the doctor's apprehensions.

She then married a young countryman, obviously consumptive, who, having gone through all the stages, fell a victim to the disease five years after his marriage.

Towards the third year of her second marriage, her health, hitherto so robust, began to change; at first her menses were less copious, and in the intervals she had leucorrhœa. Then loss of both appetite and strength followed, and she complained of want of breath upon going up stairs rapidily.

In the mean time she became *enciente*, and in proper time was safely delivered of a child. Her pregnancy seemed to modify the spmptoms.

Her color and flesh returned, and the invalid nursed

her child without exhaustion, thus seeming to justify, in this instance, Morton's usually untrue observation that feeble women are often strengthened by the performance of the arduous duties of maternity.

But two years after, at the approach of winter, her cough reappeared, accompanied by bloody expectoration, ejected with a characteristic *hem*. Her appetite lessened, and she lost her breath on the least fatigue. Auscultation showed later a cavity under each clavicle.

Menstruation, now ceasing entirely, now very slight, became more and more irregular, and was finally replaced by an exhaustive leucorrhœa.

One night we were suddenly called to the patient, who had such a fit of suffocation that we believed her to be dying. The fit ended by a fearful hemorrhage.

From that time she lost strength, and, after languishing three months, died early in November, 1863.

CASE 3.

(Bergeret, *Annales d'hygiène et de médecine légale*, 1867.)

A young girl seventeen years of age, of Martigny-les-Arsures, came from a robust family, consisting of a mother, father, son, and two other daughters. She left her parents at the end of July, to work as a day-laborer in the harvest-field in the commune of Chamblay.

Chance led her into the service of a family in which there was a young girl suffering from pulmonary consumption. Whence came this sickness? Father, mother, brothers, and sisters all enjoyed perfect health.

One of the brothers had given up farming for the calling of a *mariner*. This is what those men are called in the country who pass their lives in guiding down the Loire, the Doubs, the Saône, and the Rhone, rafts loaded with pine logs for the construction of the maritime arsenal at Toulon. Some time before the young girl became sick, her brother had taken her to Lyons to visit her friends. Among these was a young woman who was wasted by consumption in an advanced stage, and with whom she remained for some weeks. Three months after her return she experienced the forerunning symptoms of consumption. The malady was already far advanced when the young harvester from Martigny came to the house. She had so much sympathy for the sufferer, that the latter's parents offered her higher wages than she would get in the field, to remain with and care for their daughter night and day. Tempted by this offer, the young woman remained, and for more than a month never left the invalid's bedside. Soon after her return to her family, she was seized with a dry cough. Her menses ceased, she had a hemorrhage from the lungs, and, after it all, the usual signs of pulmonary dissolution. She was cared for by her younger sister, a stout girl, who was a perfect type of a healthy, vigorous peasant. The young consumptive was buried a few months later. Her sister immediately thereafter began to experience the same symptoms, and, in spite of her strong constitution, was very rapidly carried off.

Consumption was arrested in this family, because I

ordered the second patient to be isolated from the others, advised the brother and sister not to take care of her, and made the mother, a woman sixty years of age, do so. She slept in an adjoining room, and carefully followed my directions to avoid breathing the air polluted by the emanations from the patient's lungs. Moreover, the sufferer spat into a closed vase, kept day and night by the chimney fire, so that the air was constantly renewed.

CASE 4.

(Bergeret, *loc. cit.*)

This family, like the former, comprised a father, mother, son, and two daughters. The eldest daughter, like many of our peasant girls, had gone to Lyons in search of employment, and obtained a situation in a large commercial house.

After remaining two years in Lyons, she returned to her family a consumptive. Her father was a robust man, and her mother had the appearance of a powerful virago. The other two children were in flourishing health. The sufferer was cared for by her mother, who was seized with a wasting cough a short time after her daughter's death. She soon had cavities in the upper part of the lungs. Her husband cared for her chiefly, and he in turn died of the disease. His son, who soothed his last days, took to his bed within the year, never to rise again. After her brother's death, the younger sister, the sole survivor of the family, who had not left him during his illness, followed her

sister's example, and went to Lyons. There she married and became a mother, but while nursing her child she bled at the lungs, and a short time after, counting upon the healthfulness of her native air, returned to the village. But the disease in her case was already in the second stage, and it was not long before she rejoined the other members of her family.

CASE 5.

(Bergeret, *loc. cit.*)

Another family of farmers, in the village of Montmalin, canton of Arbois, suffered the same fate beneath my very eyes, with slightly differing circumstances. The family consisted of a father, mother, and three sons, whose constitutions were of the very best. Pulmonary consumption had never attacked the ancestors of either parent. The eldest son was drafted, and became a soldier in the garrison at Strasburg. During a night watch he took cold, and had an attack of rheumatism, for which he went to the hospital. By chance he was placed in the fever ward between two consumptives, whose coughing and spitting, he afterwards said, made his stay in the hospital very disagreeable. His rheumatism cured, he again entered the service, but in a few months experienced a dry cough, and began to grow thin. He went back to the hospital, from which he afterwards received a discharge as a convalescent.

I visited him on his return to the village, and found him consumptive. His mother cared for him, and

became consumptive; the two younger sons and the father himself all followed in the same path.

The father was cared for by a kind neighbor, who watched faithfully by his bedside. She in turn died, a victim to her devotion, and in the course of two years her husband followed her to the grave. They left young children only, who were cared for by relatives, and did not live in an atmosphere impregnated with tuberculous emanations. During the husband's illness the frightened neighbors made but short visits to him, merely giving him the most necessary attention, and the evil went no further. (This family had taken none of the precautions advised by Dr. Bergeret.)

CASE 6.

(Bergeret, *loc. cit.*)

A wealthy land-owner living in the country blindly threw himself into the revolutionary movement of 1848 He frequented clubs, talked excitedly all day, and drank freely at banquets and patriotic reunions. A hemorrhage which he could not arrest came on. Purulent expectoration soon followed; he lingered long, tried, in vain, the waters of the Pyrenees, and finally succumbed to the disease. He was fifty years old, and bore in his organization no hereditary taint of the malady.

His father lived to eighty-four, and his mother to seventy-five years of age. He left a widow forty-eight

years old, who was very stout, and whose parents were both octogenarians, with broad chests, and all the attributes of a powerful organization. This woman had cared for her husband faithfully, sleeping in the same room even to the last, and seldom leaving him. Three years after her husband's death she died of the same disease, having suffered for a long time. The malady accomplished its work of destruction even in this vigorous constitution.

CASE 7.

(Touchard, *Thèse de Paris*, 1860, p. 37.)

A woman died of tubercular consumption of the third stage, having slept with her husband to the last.

The latter, of an originally sound constitution, and belonging to a family in which there had never been consumption, took for a second wife a woman of an equally strong constitution and of healthy parents. After eighteen months of wedlock he yielded to a pulmonary consumption of the worst kind. The second wife continued to sleep with him until his death. A short time after she married again, but, two years after this second marriage, died of consumption. Her second husband, perfectly robust, and belonging to a family which had never known an instance of consumption, yielded to this affection some time after the death of his wife. This happened at Haynin, in Belgium.

CASE 8.

(Dr. L. G. Bryhn, in *Norsk Magaz. für Lägevidensk and Nordiskt Mediciniskt Arkiv.*, Band XII. Translated in *London Medical Record*, July 15, 1880.)

A phthisical man married a woman of healthy family. The man died. The woman became phthisical, as did also her sister, who resided in the house during the man's illness. The latter married a man of great strength and of sound family. He, too, was attacked, and also his sister's daughter, who resided some time in the house. One of their children died of tubercular meningitis, two had signs of pulmonary tubercle, and one was free. The girl who served the first man's wife became tuberculous, went home, and her sister was affected by her. Both their parents had lived to a great age, and tuberculosis had never before shown itself in the family.

CASE 9.

(Hermann Weber,* *On the Communicability of Consumption from Husband to Wife*, in *Clinical Society's Transactions*, 1874, Vol. VII.)

J. had seen his mother, two brothers, and a sister die of pulmonary consumption, and had himself, on two occasions, had hemorrhages when twenty and twenty-one years old. He became a sailor, and was apparently in good health when he left home, in his

*As I could not get access to the original in the following cases of Weber's, they have been translated back again into English from a French translation, and may, therefore, not correspond word for word.

twenty-fifth year. He married at the age of twenty-seven.

He married four wives:

First, A woman belonging to a perfectly healthy family; she enjoyed excellent health until her third pregnancy, when she began to cough and grow thin. She died of consumption after her third confinement.

Second, At the end of a year he remarried, the second wife having every appearance of health; but at the end of a year of conjugal life she began to cough and to spit blood, and soon died of quick consumption.

Third, The third wife belonged to an exceptionally healthy family, consisting of a father, mother, four brothers, and two sisters, all living and in good health.

When married, she was twenty-five years old, and continued to have good health until her second pregnancy, when she began to cough and to have feverish turns. She had two hemorrhages; and when I saw her, seven weeks after her second confinement, she showed extensive lesions in the upper part of the lungs; also hectic fever and profuse sweats. A month later she was taken with severe hemorrhage, and died shortly after, eight months from the appearance of the first symptoms.

The autopsy revealed signs of pneumonic and tuberculous consumption combined, to make use of an expression employed by the late Dr. Addison, who visited the patient with me.

Fourth, The fourth wife, whom I also attended, had not a sign of consumption in her family, and at the

time of her marriage was twenty-three years old, and in the enjoyment of perfect health. About thirteen months later, three months after her first confinement, which had resulted happily, she began to cough, and had a little fever. Then very clearly defined signs appeared; first at the upper part of the right, then of the left lung; moreover, she coughed up blood, and had a slight pleuritic effusion.

She experienced some relief during a voyage which she took to Melbourne, but, on her arrival there, had a severe hemorrhage, and died a short time after her return to England, nine months after the appearance of the disease. The autopsy showed extensive pneumonic and tubercular lesions in both lungs, as well as tubercles in the intestines, spleen, and liver.

At two different times, in 1854 and 1857, after the third wife's death, and during the illness of the fourth, I had occasion to examine J. His general health was excellent, and he assured me that he did not cough, and merely expectorated a little mucus in the morning. The upper part of the left thorax was flattened, and percussion showed it to be less resonant than the right; inspiration was less distinct; expiration was prolonged, and from time to time râles were heard. He did not remarry, not wishing to expose his choice to "certain death." He was healthy, and continued to do active duty as a sailor until 1869, when he was forced to keep his bed for some months, on account of a severe fracture; he then began to cough.

The upper part of the right lung, which had until then been healthy, became diseased, and consumption

developed in the usual way, and caused the patient's death in 1871. The autopsy showed cicatrization where the disease had first seized him, and also more recent lesions.

CASE 10.

(Hermann Weber, *loc. cit.*)

W. belonged to a consumptive family; he had lost his father and two sisters by that disease, and had himself had hemorrhages, and other pulmonary symptoms, at the age of nineteen, but he had in three successive winters made voyages to Venice, and was so much improved that he considered himself well. When twenty-six, he married.

First, His first wife was young, in excellent health, and belonged to a remarkably robust family. She began to cough towards the end of her first pregnancy, had a hemorrhage after her confinement, and died of consumption four months later.

Second, He again married, in 1852, a young girl of twenty-one, perfectly healthy, but not without a hereditary taint. She continued in good health during her first pregnancy, and to the middle of the second, when she had several attacks of inflammation of the lungs, followed by two hemorrhages, from which she never recovered, dying of "quick consumption" three months after her second accouchement.

Third, After a rather short interval, he again married. The woman was young and stout; her father and mother were healthy, as were her four sisters and her brother. About three months after her second

confinement she was attacked by "inflammation of the lungs," from which she never entirely recovered. When I saw her for the first time, seven months after the appearance of the "inflammation," both lungs were affected, the right in a greater degree than the left; she had two severe attacks of fever every day, frequent sweats, diarrhœa, and considerable emaciation. Tubercular meningitis set in a short time after my first visit to the patient, and she died eight or nine months after the appearance of the first symptoms.

The autopsy showed extensive lesions on the right side, and a general miliary tuberculosis. The uterus was healthy.

The husband's general health appeared good; he could walk for long distances, but, when he remained quietly at home, suffered from dyspepsia. The regions above and under the clavicle on the right side were flattened, and, when struck there, the sound was dull. The respiratory murmur was almost entirely wanting, and once in a while dry râles were heard.

The patient was hypochondriacal, and filled with remorse for several months after his wife's death, but he rallied, and lived much in the open air. He was stricken with typhoid fever in Germany, followed by pleuro-pneumonia on the right side. He died of consumption six months after the fever.

CASE II.

(Hermann Weber, *loc. cit.*)

Z. (no information about his family) had two hemorrhages about the age of nineteen; was ill, at this

time, for several months, but was entirely cured, he said, for he had exposed himself to all kinds of weather without experiencing any ill effects. At the age of twenty-three he was married.

His first wife was a healthy and robust woman, twenty years old, with no predisposition in the family to consumption ; but, after the birth of her first child, she became ill, and in five months died of "quick consumption."

About two years after he married a strong woman, twenty-seven years old, belonging to a remarkably healthy family. Eighteen months after her marriage, three months after her first accouchement, she began to cough and to grow thin. The cough never entirely ceased. Two months later she again became *enciente*, and when I saw her for the first time, she was in the eighth month of her pregnancy, and was very thin. It was a type of consumption in the last stage, the greater part of the left lung being affected, but not to such an extent as the right; she had night-sweats, diarrhœa, and hectic fever. A month later she was confined, and lived but three weeks after it. The autopsy showed lesions from recent subacute consumption, to wit, infiltration of a gray color and of different consistencies ; cavities recently and irregularly formed, with no lining ; and yellow and gray tubercles scattered about.

Z, the husband, considered himself well, but nevertheless showed some flattening in the upper part of the left side. The resonance on percussion was plainly imperfect, as well in the regions above and under the

clavicle, as in the corresponding places behind. In the same places inspiration was less distinct, and expiration was prolonged. Six years later, after his return to Germany, in 1865, he again had severe hemorrhages, and died in 1866. I have no information about the autopsy.

CASE 12.

(Hardy, *Bulletins de l'Academie de médecine*, 1868, p. 348.)

This is about a young man belonging to one of the most distinguished medical families in Paris, who had contracted a pulmonary consumption under the influence of a cold, damp climate. After the malady had disclosed itself, he returned to Paris, and brought with him his wife, a perfectly healthy woman. He died after about a year's suffering, and a short time after his wife showed symptoms of the same disease, which eventually ended her life.

In this instance, what right have we to explain the coincidence in the diseases of husband and wife by a similarity of surroundings, as noted by M. Behier? The husband's illness was contracted while in a foreign land; on his return to France his wife was well, and remained so for several months, and it was only some time after that the first symptoms appeared. In no member of the family had there been an instance of this disease, and therefore it might be safe to say that this was a case of contagion.

CASE 13.

(Castan, *Montpellier Médical*, 1869, Tom. 22, No. 2, p. 111.)

A butcher named G., forty years of age, died of consumption at the close of 1865. The disease had been slow, and had followed the usual course. Its last stages had been characterized by the ordinary symptoms, — weakening sweats, abundant expectoration, incessant diarrhœa, etc. His mother-in-law, the widow B., sixty years old, a woman of sanguine temperament, who had always enjoyed good health, and whose family had no hereditary taint, lived with him in a sufficiently large and well-ventilated house. She soon showed signs of tuberculization, and died of consumption in December, 1866. She had cared for her son-in-law with more assiduity and perseverance than had her daughter, as the latter took charge of her husband's business, and was away from the sick-room nearly all day. This, we believe, explains how the mother-in-law, instead of the wife, contracted the disease. The latter remarried, and continued to enjoy perfect health. G. had no children.

CASE 14.

(Castan, *loc. cit.*)

On the 1st of September, 1866, a man in the second stage of pulmonary consumption came to consult me. He said that his wife had died after a long illness, during which she had coughed, expectorated, and sweated much, and that he had cared for her and

shared her bed to the last. The hygienic surroundings of the couple were good, according to the patient's story, and this could be easily believed, from his social position. He always had good health, had no hereditary taint, no scrofulous symptoms, and had not indulged in excess of any kind, that might occasion his disease. Therefore the only cause which appears reasonable to us is contagion.

We lost sight of this man after two months' treatment.

CASE 15.

(Seux (*père*), *Le Marseille Médical*, April 20, 1869, No. 4, p. 310.)

In Mme. ———'s family were both consumption and scrofula. She was married at the age of twenty-seven, and afterwards had hemorrhages several times, though never very seriously, during a period of twenty years. When forty-five years old, in connection with her blood-spitting she had a slight cough, which, however, rapidly disappeared; but when she was fifty years old, the cough, accompanied by characteristic expectoration, returned, and became chronic. As I was called to the patient about this time, and cared for her until her death, I can give undeniable proofs of all the physical signs which showed a tuberculous softening at the upper part of the lungs.

The disease was exceedingly slow in its progress for several years. Still, all this time she became gradually thinner and thinner, coughed more and more, and frequently had profuse night-sweats.

Such was her condition when her husband, a man of fifty-five, who had always shared his wife's chamber and bed, began to have hemorrhage. This man was of sanguine temperament, and had never had the slightest tendency to chest trouble. There were no consumptives among his ancestors, several of whom had died of apoplexy. I must add that he was a man of great sobriety, and found no pleasure but in the companionship of his family.

The first hemorrhage was preceded and followed by remarkable emaciation, and auscultation showed unmistakable signs of tuberculization After lingering thirteen or fourteen months, he yielded to that form of consumption characterized by the sudden appearance of fever, the rapidity of emaciation, and the comparatively sudden death. What a contrast to the slow progress of the same disease in his wife! In her case the malady progressed slowly, but surely, and took her off only after a most protracted siege. Her illness had begun about forty years before her death, and she survived her husband thirteen years.

I should add that the couple lived in the greatest ease, and in the best hygienic conditions.

CASE 16.

(Gros, de Luzy, Vievre. Quoted in *La Thèse du Dr. Compin*, Paris, 1870.)

A long time ago I became convinced of the truth of the thesis you support; and if every physician in our country (where there seems to exist a feeling of

the necessity of a husband and wife's sleeping together to the very end) would thoroughly search his memory, he would find some case similar to that I am about to relate. Although this happened eight or ten years ago, it struck me so forcibly that I remember it as if it were but yesterday.

In March, 1861, I was called to a place twelve kilometres out of Luzy, to the wife of a man named L., a private watchman, forty years of age, and remarkable for his large size and wonderfully athletic constitution. His wife, a woman of from thirty to thirty-five years of age, had been sick for several months. She was much emaciated, had an enormous cavity at the top of the left lung, and showed all the symptoms of consumption in the last stages. I warned the husband (who was not at all deceived as to his wife's condition) to cease all cohabitation with her, if he had not already done so.

The woman died a short time after. Six months rolled on, and the widower, in the full strength of his years, took to himself a young wife, who appeared healthy. After a year I was again called to L.'s. "His wife had been happily confined," they said, "but the child was dead, and since that the mother coughs, and has fits of suffocation." An examination showed her disease to be quick consumption, and the second wife soon rejoined the first.

This is the first chapter. I said that L. was of robust constitution and remarkable for his strength. So, when eight or ten months after his second bereavement I was called to visit him, I was startled at

the change in his appearance. A short time after his second wife's death, L. several times had a dry cough and chills. Although he knew that he was growing thin, yet, trusting in his strength, he was but little anxious, and took no care of himself. But when he first began to cough he remembered the past, and his first words on my arrival were, "Doctor, I have contracted this disease from my wives." This was, unfortunately, too true. Strong as he was, the malady conquered him not long after. This is the second part of the drama. You may well suppose that in such a case as this I learned the full particulars of the constitutions of L. and his ancestors. There was nothing to authorize a suspicion of hereditary consumption. The man's first sickness was also his last.

I shall never consider this case merely as a coincidence. If I expressed my entire conviction, it would be that the second wife, entering the contaminated house, sleeping in the bed, and using the under-clothes of the first victim, had taken from them the germs of the disease.

I have practised medicine for thirty years, and have seen many similar cases, but in no chain of events has this theory been carried out so logically, so to speak.

For my part, I am strongly convinced that, *under certain circumstances*, pulmonary consumption is contagious.

CASE 17.

(J. Bernard, *Thèse de Montpellier*, 1872, No. 46.)

P. J., a man forty years old, from C., a *valet de chambre*, for four months assiduously cared for his

master, who died of consumption. There was nothing in J.'s family to give a suspicion of a constitutional taint; his father, seventy years of age, was actively engaged in farm work, and his mother and brothers were equally healthy.

After his master's death, J. had a kind of sickness to which it is hard to give an exact name. He lost strength and appetite, and often coughed. He was obliged to leave off working, and return to his family to regain his health. Taking the physician's advice, he went to Montbrun to try the waters, where he arrived Aug. 14, 1870.

The patient was thin and pale, had but little appetite and restless sleep. His cough was frequent, with hardly any expectoration. Percussion showed a slight dulness under the left clavicle, still more over the upper part of the lung behind, on the same side. There were moist râles both in front and behind.

Mineral waters (as a drink) and gaseous inhalations were prescribed. The patient derived no benefit from this treatment, spitting blood worse than ever, which obliged him to desist. We have never seen him since.

CASE 18.

(Chamontin, *Thèse de Montpellier*, 1874, No. 22.)

A confectioner named Martin, fifty-one years old, came into our hospital June 27, 1873. He said that he had coughed and expectorated for a long time; he was thin, had some fever, night-sweats, and neuralgic pains in the back. In the upper third of the left lung

we found dulness on percussion, also dry and moist râles.

On the right side the trouble was farther advanced; here there were coarse bubbling and gurgling râles. His expectoration was yellow, thick, abundant, and muco-purulent, with a predominance of pus.

Everything showed a clearly defined tubercular formation, in different stages of evolution.

The patient had complained noticeably for only about four years. He had led a regular life under the most healthful conditions. He had *absolutely no* hereditary tendency to disease; his trade was neither laborious nor unhealthy. His father died of softening of the brain at the hospital of Montdevergnes; his mother is still living, at an advanced age.

In his most remote ancestors we found nothing worthy of suspicion. He had lost his wife seven years before from a chest disease, which, from the description with which he furnished me, I felt pretty sure was consumption.

His mother-in-law, a young woman, also died, he said, from a chest trouble, which seemed to him the same as that of his wife, in its course and symptoms.

I thus found in the latter an unmistakable hereditary taint, developing into actual disease, which accounts for the death of six children, four of whom were carried away at an early age by meningitis, incontestably tubercular in form. Of the other two, one succumbed, at the age of eighteen, to a pulmonary tuberculosis; the other died, when thirteen years old, in the wounded citizens' ward, where he had been admitted on the

score of many abscesses. My duties as interne in this ward gave me the opportunity of proving the existence of tubercles in his lungs, some far developed. The abscesses were owing to caries of the spine.

The following facts are proved: that our patient had no sickly ancestors; that he had been very healthy for the first forty years of his life; that he had a strong constitution. His habits had been regular; he had not contracted from excesses or privations the tuberculous affection which appeared at a comparatively advanced age, and, a noticeable fact, it appeared only after the death of his wife, who was thoroughly tainted with the disease, and with whom he had always cohabited.

If this is not a case of contagion, what other hypothesis will better explain it?

CASE 19.

(Chamontin, *Thèse de Montpellier*, 1874, No. 22.)

A farmer named Melchoir, forty-two years old, entered our hospital July 15, 1873. He had coughed for nearly a year, and his voice was almost gone; he was a man of large size, and was well built.

Auscultation showed crackling in the upper part of the lungs; percussion revealed dulness at the same points. There was a slight hectic fever, characteristic purulent expectoration, and night-sweats; the larynx, or at least the glottis, was filled with tubercles.

What were this man's antecedents? In what hygienic conditions did he live? His father died sud-

denly twenty-four years ago, of pneumonia; at least that was the name given to the malady by the physician. His mother, also, died suddenly of apoplexy in our hospital, this year, 1874, at a very advanced age. She was very strong, and of a ruddy complexion.

He has two brothers, who are strong and healthy; like them, he used to live in the fields, and was engaged in agricultural employment in the suburbs. He had never before had any chest trouble. Then whence came this disease? We charge it to the score of contagion.

Our patient's wife entered the hospital June 13, 1873, and died July 10, of the same year. This woman, who was small in stature, and whose father had died of consumption, had coughed for seven years; her last confinement had hastened the end, and we were convinced that she was in the last stages of consumption. The existence of cavities, of moist râles located in the upper part of the lungs, of purulent expectoration, the retarded progress of the disease during the later pregnancies, and its acceleration afterwards, and the symptoms generally, were all signs which made us agree on that diagnosis.

So we see that our patient was strong, had no hereditary taint, was born in the country, had lived in accordance with the best hygienic laws, and had labored at an occupation irreproachable for its kinds and conditions of work. On the other hand, he had cohabited for eighteen years with a consumptive wife, and that as intimately during the evolution of the diathesis as before. Have we not a right, therefore, to suspect contagion?

CASE 20.

(Edgar Holden, M. D., of Newark, N. J., in the *American Journal of the Medical Sciences*, July, 1878, p. 145.)

Dr. E. D. Kittoe, of Galena, Ill., makes the following interesting statement: "I have frequently noticed the occurrence of death from this cause in persons who had cared for and nursed friends who had died of consumption, but who before doing so showed no tendency to it, and in whom there was no evidence of hereditary taint. For a marked case see the following: The late Gen J. A. R. was born and brought up near this city; his parents are both living at this time, remarkably robust and healthy; no case of tubercular disease had ever been known to have affected the ancestors of either father or mother. Gen. R. was a remarkably strong and vigorous man, both mentally and physically, prior to his marriage. He married a Miss S., from Utica. By this union he had three children. Miss S. came of a consumptive family, and the disease developed soon after marriage. The general was a very kind and attentive husband and nurse, and gave himself up almost entirely to that duty, staying often in a close room with her, up to her death. After this, he was almost at once placed upon the staff of Gen. Grant. I observed soon that he had a slight cough, and in the summer of 1863, after the siege of Corinth, he had fistula in ano, for which the usual operation was performed. He continued to have a cough from that time forward. On the occasion of

Gen. Grant being ordered to supersede Rosencrans at Chattanooga, in crossing Waldron bridge we were exposed to a heavy rain all day, and, on arriving at Chattanooga, the general's cough was very bad, with sanio-purulent expectoration. When he left with Gen. Grant for the Potomac, he was very thin, but his indomitable energy kept him up. At the close of the war he was in a well-defined tubercular condition, and gradually gave way till he died."

Dr. Holden says, that "a singular fact is here discovered. This history of Gen. R. is, by a remarkable coincidence, taken up by another physician, of another State; and, entirely without the knowledge of Dr. Kittoe, is thus elaborated by Dr. Bennett, of Danbury, Conn.: "About three years before Gen. R. died, he again married, — a Miss H., none of whose family have any tendency to consumption. He died of consumption. The second wife also died, in a year or eighteen months after, of the same disease. She bore him two children of feeble constitution, who died in a day or two after birth. The family believe that the disease was transmitted from the first wife to the husband, and by him to the second wife."

CASE 21.

(By Dr. L. MacDowell, of Flemingsburg, Ky., in Dr. Holden's article in the *American Journal of the Medical Sciences*, July, 1878.)

I knew a man who had all the external appearance of a tubercular diathesis, who married a healthy girl of eighteen. In three years (after the birth of her

second child) she died of tuberculosis. He married again, in a year and a half, an exceedingly robust woman, of a family without taint. In less than two years she died of unmistakable pulmonary consumption. The third time he married a healthy woman. He himself died, in one year after his third marriage, of consumption, and his widow followed him in six months, with the same disease.

CASE 22.

(Lawson Tait, F. R. C. S., of Birmingham, England, in the *American Journal of the Medical Sciences*, October, 1871.)

About three years ago, there came one day to consult me a pretty, flaxen-haired, blue-eyed girl of about eighteen, whose face alone it was enough to look at to designate pulmonary consumption. She told me her mother had died of it, and so had one brother. As is the wont of women in affliction, she brought a friend with her to help her through the ordeal of the visit, a perfect contrast to herself, tall, stout, and strong, the very picture of health, a handsome Irish brunette, born near Sligo. The two worked together at one of our Yorkshire wool mills ; and it would seem as if the strong contrast there was between them had caused their close friendship. They lodged together, and until their visit to me had slept together. As is my custom, I at once insisted on the patient having a room to herself, and after my injunction this was strictly carried out. I need not detail the case of the first girl. Suffice it to say that she passed through

several attacks of softening, during each of which she was attended carefully, but not closely, by her friend, who, during the time when the patient was unable to work, earned the support of both, and after those attacks she recovered completely, married, and is now the mother of two children. Her devoted friend had a different and sadder fate; for only five weeks after her first visit to me in the capacity of companion to my patient, she herself came with the dyspeptic symptoms which usher in the first stage of phthisis. During the time she was nursing her friend the physical signs of incipient consumption were manifested, and, despite all my endeavors, it ran an almost unchecked course in rather less than three months. Long before her death, the conditions of the two girls were reversed: the patient had become the nurse, and the nurse had taken the patient's bed; the former, whose fate I had regarded as decided, recovered; the latter, who really sacrificed herself for her friend, died. It was strange and most interesting to me to find that the survivor felt keenly that she had given the disease to her companion, and was the unconscious cause of her death. In the case of the girl who died, I was fortunately able to get the family history very completely, and eliminate any difficulty there might have remained as to a family taint. Her father and mother, and several brothers and sisters, were alive, and all strong. She had a grandfather and two grandmothers alive, and no instance had occurred in the family of death from chest disease, or any of the usually allied affections. Nor in her own history was

there any point which could be indicated as one of likely explanation for the phthisis. The whole evidence, negative certainly, but none the less valuable on that account, pointed to the conclusion that it was a case of phthisis derived from contagion.

CASE 23.

(Dr. Henry I. Bowditch, in *Boston Medical and Surgical Journal*, May 26, 1864.)

Miss S. sprung from a family in which no trace of consumption is ever known to have existed. She was living as a farmer's daughter on a most healthy site, and enjoyed most strong and robust health, when she went to attend an invalid friend, to whom she was most tenderly attached. The friendship was mutual. The invalid sought for and received the closest attention on the part of her young companion. They slept in adjacent rooms at night, and the strong one acted as nurse at all hours, day and night. This attendance commenced in July, 1853, and continued till June, 1854. During all this period the invalid did not wish any one else to do aught for her, and her young companion often lifted her when the patient was too feeble to raise herself. During one of these efforts Miss S. felt that she had "strained herself." Ever after she had an uncomfortable "coldness" at the epigastrium, and a tendency to chilliness, with, subsequently, in the spring of 1855, some dyspepsia. In the following months her parents noticed her rather feeble condition, but did not regard her as seri-

ously ill. She was treated as a dyspeptic, and considered herself as such. She had scarlet fever in the spring. In August, 1855, cough set in, *i. e.*, after twelve months' invalidism, and the cough continued uninterruptedly till death. Diarrhœa commenced in March, 1856. She died of phthisis, July 27, 1856, nearly two years after her friend's decease. I saw her once in the spring of 1856. She then had the signs of anomalous tuberculosis, *i. e.*, crepitus, a pure tubercular respiration, and solidification of the lower two thirds of the right lung. She also had had two abscesses under the arms, a fews days before my visit.

CASE 24.

(Reich, in *Berliner Klinische Wochenschrift*, Sept. 18, 1878. *Reynolds's System of Medicine*, Am. Ed., Vol. II. p. 117.)

The only two midwives practising at Neuenburg, a healthy little town of 1,300 inhabitants in 1875, were R. and S. Of these, the woman S. was undoubtedly the subject of phthisis, with abundant puriform expectoration. In the first case described, Dr. Reich extracted the child by turning. While his attention was engaged with the mother, he noticed that, owing to some difficulty in the child's breathing, the nurse S. sucked the mucus from the infant's mouth, and also endeavored to promote respiration by blowing into its mouth. For the first three weeks the child progressed well, but then its health failed, and within three months of its birth it died of well-marked tubercular meningitis, initiated by symptoms of bronchial catarrh. In May and June following, two more chil-

dren died of the same disease. These three cases had been attended by the nurse S. Dr. Reich's attention being thus attracted, he found, on investigation, that between April 4, 1875, and May 10, 1876, seven children, in addition to the above three, had died (all within the first year) of tubercular meningitis, although in no case was there any history of hereditary tuberculosis; that all these cases had been attended by the woman S., while of all the cases attended by the other midwife, R., not one had died of this disease, nor had any manifested in any way indications of any tubercular form of disease. The duration of the illness varied from eight days to three weeks; whereas of the ninety-two children who died in their first year during the nine years from 1866 to 1874, only two died of tubercular meningitis; and similarly, among the twelve infants who died in 1877, there was only one such case, and its parents were tuberculous. The midwife S. herself died of phthisis in July, 1876.

It was ascertained that S. had been frequently in the habit of sucking mucus from the mouth of infants, and also of caressing and kissing them.

CASE 25.

(Quoted by D. S. Booth, M. D., of Sparta, Ill., in *Transactions* of the Southern Illinois Medical Association for the year 1879.)

Dr. S. D. Burgess reports the following very interesting series of cases, all growing out of one case. Mr. H. H., of a consumptive lineage, married a Miss

R., of a remarkably healthy family, and noted for their longevity. Mr. H. was in the incipient stage of consumption at the time of his union with Miss R. After his marriage he continued to gradually grow worse, and he finally died at the expiration of about eighteen months. His wife was his constant companion and nurse, and some three months before his death the premonitory symptoms of phthisis manifested themselves, and some twelve months after his death she died. One child was born of the marriage of Mr. H. and Miss R. The child lived about one year, and died of tubercular meningitis. Mrs. H. was closely nursed by a single sister, who, some time prior to the death of Mrs. H., gave unmistakable evidence of commencing tuberculosis; and some twelve or eighteen months after the death of her sister she also died. The single sister was nursed by a younger brother, and, strange to relate, he only survived his last sister's death a few months, dying of the same disease. The remainder of Mrs. H.'s family lived at a distance, and were seldom at her house during the sickness of any of those above recorded, so that they were not under the contaminating influence of the infectious material, and escaped, as not one of the same has ever had any disease belonging to this class. The father and mother were living, and in good health, some six years ago, aged between eighty and ninety years. I look upon this concatenation of cases as a striking evidence of the contagiousness of consumption, and any attempt to enlarge upon it would only tend to impair it.

CHAPTER V.

DEDUCTIONS.

It is only fair to state that the twenty-five cases in the preceding chapter were not taken at random, but were carefully selected from among those which can be found on record scattered through the annals of medical literature, because they seemed, most of them, to present rather strong and convincing evidence of contagion. While it would hardly be proper, therefore, to lay too much stress on statistics compiled from such selected cases, yet, as the principle of selection was to a considerable extent determined by the question of heredity, which is to most people the greatest obstacle to a belief in the contagiousness of consumption, a few facts and reflections suggested by them on other points, at least, may not be unprofitable.

Sex. — The twenty-five cases described in Chapter IV. make mention of sixty-six persons who became consumptive, probably through contagion. Ten of these (Case 24) were infants, whose sex was not reported. Excluding these, therefore, and confining ourselves to those whose sex we know, we find that thirty-six out of the remaining fifty-six were females.

This seems to be in accord with the common verdict; for it has been pretty generally observed by those who have looked into the subject, that this sex is apt to preponderate in such cases.

It is a matter of no little surprise, that Dr. Bowditch, who certainly favors the idea of contagion (see Chap. II., p. 33), should find in this circumstance such a stumbling-block. In the *Boston Medical and Surgical Journal*, Vol. LXX. p. 337, he says:—

"The strongest argument I know against contagion is the fact that, considering my facts at least, woman is more liable than man to be taken. Why this, if there be not something at any rate more important than contagion to explain the phenomena of the case? The really contagious disease spares neither age nor sex. There must be something, therefore, more than mere contagion."

It would seem that the simplest of all possible explanations ought to be sufficient to meet this objection. Woman is oftener taken, because she is more exposed. Except among the Amazons, more men than women are killed in war, because they are more exposed. When a man is consumptive, his wife is his natural nurse, and ministers to his wants day and night, week after week, and month after month, with that love, utter devotion, and reckless disregard of self, which we all know to be woman's commonest attributes. On the other hand, if it is the wife who is first taken sick, the husband, obliged to attend to his daily occupation away from the house, then commits the main charge of the invalid to some one else, even

if he, to some extent, attends her nights. At any rate, he is not subjected to such close and constant exposure.

As far as we can judge at present, consumption is different from the common contagious diseases, measles, scarlet fever, etc , in that, while exposure to the latter for the slightest possible duration of time may suffice to cause their reproduction, consumption can, as a rule, be acquired, in the opinion of contagionists, only after a close exposure, prolonged for weeks, months, or even years. We know that the susceptibility to the common contagious diseases differs greatly not only in different individuals, but also in the same individual at different times ; and even if it should be proved, in the future, that the origin of many cases of consumption, now otherwise explained, is due to a comparatively slight exposure to the disease, yet it stands to reason, and is rendered probable by analogy, that, on account of this difference in susceptibility, the more times one is exposed and the more closely, the more chance is there of the disease being acquired.

In our cases, twenty wives were contagioned by their husbands, while only thirteen husbands were contagioned by their wives. Of these twenty women fourteen were stated to be affected soon after childbirth. This relation of child-bearing to the development of the disease has frequently been noticed, and has been explained by some on the supposition that the germs of the disease, inherited by the *fœtus in utero*, have, through its medium before birth, been con-

veyed to the mother. If this is true (as it undoubtedly is with syphilis), it may help to explain the preponderance of women.

But it is not alone in the marital relations that this excess of the female sex is noticed. In our cases we find notes of four females who were supposed to be contagioned by intimate female friends, one girl by her mistress, one woman by her uncle's wife, two by male friends, one mother-in-law by her son-in-law, one sister by her brother, one mother by her daughter, one mother by her son, one niece by her aunt, three sets of sisters by their sisters. (In five out of seven cases of blood relationship it is distinctly stated that there was no hereditary influence.) On the other hand, we find one soldier reported contagioned by his fellow-soldiers, one valet by his master, one son by his father, one son by his mother or brother, and one brother by his sister. (In two out of three cases of relationship it was distinctly stated that there was no hereditary transmission.) Sixteen females against five males here, most of them acting as nurses.

Hereditary Transmission. — Of the sixty-six cases of probable contagion mentioned in Chapter IV., fifty-four were distinctly stated to be free from any hereditary taint, one was not free from it, and of the remaining ten, almost all of whom were stated to be remarkably strong and vigorous previously, probably some would have been found to be likewise free, if their family histories could have been obtained. As already remarked, however, these figures undoubtedly represent rather more than the average exemption, because the

cases were selected mainly with reference to this very point. That consumption can be transmitted from parent to offspring is one of the best-established facts in medicine, and is not likely to be called in question ; but that the agency of heredity in the production of the disease has been greatly exaggerated, not only by people generally, but also by physicians, is equally true. This question of heredity is really the greatest obstacle in the way of the acceptance by most persons of the doctrine of contagion, although when looked at in the light of Cohnheim's recent researches (see p. 172), it harmonizes perfectly with this doctrine.

It is so easy to be satisfied with such a simple explanation as heredity, and then to dismiss the subject. If the parents give no evidence of the disease, forthwith the grandparents, great-grandparents, aunts, uncles, and indeed all the numerous ramifications of the ancestral tree are invoked, until traces of the horrid monster are dragged to the light. If put to this crucial test, few of us could escape. We should all be found contaminated, as by "original sin." This explanation really savors far more of mysticism than the doctrine of contagion itself. In fact, statistics show that heredity is powerless to explain more than a small proportion of the existing cases of consumption. Dr. Walshe, of London, one of the highest authorities, wrote in his work on *Diseases of the Lungs:* "The final conclusion flowing from this analysis of the family history of four hundred and forty-six persons is, *that phthisis in the adult hospital population of this country is, to a slight amount only, a disease demonstrably derived from parents.*

Lebert, after careful inquiry, found heredity in only about one sixth of the cases investigated by him. Others have reached similar results.

If, therefore, hereditary transmission will explain only a small proportion of the cases of consumption daily met with, it necessarily follows that the majority of such cases must be in some way acquired. Is it strange, then, that, with the evidence which we have at present, we should be induced to believe that contagion, in one way or another, has an influence in the production of some at least, if not all, of these acquired cases ?

At what Stage is Consumption most Contagious ? — From the records of our cases it is not easy to determine exactly the stage of the disease in the person who communicates it to another, but it will not be far from the mark to count thirty-eight in the last stage, fifteen in the second (many of them bordering on the last), and seven or eight in the earlier stages. From time immemorial it has been the custom for contagionists to consider the disease communicable only, or at any rate mainly, when far advanced, and to call it comparatively innocuous in the beginning. Whether the incipient disease will ever be proved to be dangerous to any extent remains to be seen.

Age. — From our cases it will be seen that no age is exempt. Infants, persons of sixty years and over, as well as most intermediate ages, are represented, although the majority are certainly between fifteen and thirty. It is, therefore, advisable that the care of consumptives be not imposed on young people any

more than is absolutely necessary. From Dr. Reich's case it will be seen that the disease in infants is apt to assume the form of tubercular meningitis.

Period of Incubation. — The period from the time of exposure to the disease, when the germs are received into the system to the time when its first manifestations appear, must always necessarily, from the very nature of things, remain inexact. In our cases it is often set down as a few weeks or months, or a "short time." Sometimes, however, it apparently extends over years.

This possibly long period of duration proves a bugbear to the opposers of contagion, who perhaps forget that the period of incubation differs considerably in different diseases ; that of measles, for instance, being generally about two weeks, that of scarlet fever being on an average five or six days (although the variations from this average are considerable), while that of hydrophobia lasts for one, two, or three months, the minimum period being eight days and the maximum *one or more years.* In Reynolds's *System of Medicine* a case is reported, said to be authentic, where a man who had been confined in prison came down with the latter disease. although the only time he was ever bitten was *seven years* before. Everybody knows that when consumption is inherited, the germs of the disease, although born with the child, nevertheless generally do not spring forth into obvious development for fifteen or twenty years or more. If, then, they can lie dormant so long when received into the system by inheritance, why should it not be possible for them to

lurk there undeveloped for a much less time, if received by contagion? Why should the period of incubation in consumption necessarily be moulded after the fashion of that of the short eruptive fevers?

Negative Facts. — It seems strange that intelligent men should rely to any great extent on negative facts in a question of this kind. And yet the illustrious Laennec, having some doubt in the matter, bolsters up his doubt by referring to the numerous instances of large families sleeping in the same room with a consumptive, or husbands sleeping with their sick wives, *without* catching the disease; and the well-known Portal emphasizes the fact that often a consumptive man may live successively with two or three wives, without poisoning them. The method of catching hydrophobia by means of the saliva of rabid animals introduced by the bite is universally known, and Raynaud, by a large number of experiments, demonstrated that the saliva of hydrophobic human patients, when artificially inoculated in animals, will produce the disease in them; and yet within a few months a Russian named Kowalewsky flatly contradicted Raynaud, because, forsooth, he inoculated three dogs with saliva from hydrophobic patients, and failed to produce the disease, and then, emboldened by his negative success, actually allowed himself to be kissed by several hydrophobic soldiers, triumphantly remarking that he was not hydrophobic yet. To clinch his demonstration, he added the instance of an imbecile in a hospital, who swallowed a piece of food after it had been chewed and rejected by a patient afflicted with hydrophobia, and yet escaped the disease.

Even if the story *is* worn threadbare, it is so apt in this connection that we cannot resist the temptation to enforce our point by it. We refer to that of the Hibernian, who, when accused of stealing a pig, resolutely protesting his innocence in spite of the fact staring him in the face that there were three men ready to testify that they saw him steal the pig, triumphantly exclaimed, "But sure, your Honor, I can bring here *six* men who *did n't* see me steal it!"

If we should take as our criterion the numerous cases of persons who are often exposed to scarlet fever *without* catching it, by a parity of reasoning we might easily prove that *that* disease was not contagious. Of a thousand acorns on a mighty oak, perhaps only one will reproduce another tree. Will this failure of the nine hundred and ninety-nine entitle us to deny the possibility of the perpetuation of the oak life? One positive fact, properly vouched for, may at times be worth more than a thousand negative facts.

Exhaustion from Nursing. — Attempts have very often been made to explain away many of these cases of apparent contagion by asserting that sufficient cause for the disease may be found in the bodily fatigue, loss of sleep, anxiety, and other depressing influences which lower the vital powers. There is not the least doubt but that these are extremely favorable *conditions* for developing the disease, if it already exists in germ in the subject, but we doubt the non-existence of something more important than these influences behind. Now and then, an indiscriminating supporter of contagion will throw a stumbling-block in the way

of others' belief in it, as did at least one correspondent of the Massachusetts State Board of Health (Fourth Annual Report, p. 361), by attributing the origin of the disease to contagion in a case which everybody could see really might have come as a heritage ; here, as we allow, the constant and assiduous watching fanned the pre-existent spark into a flame.

Until comparatively recent times it was believed that typhoid fever was brought on by overwork, anxiety, and other debilitating causes ; but now the almost universally accepted idea is that no amount of exhaustion will bring it on, if the *specific germs* of the disease have not first entered into the animal economy. It may bring on nervous prostration and other troubles, but not typhoid fever.

How have the opponents of the views which seem to us so probable hitherto explained the fact that, after excessive fatigue and anxiety, one person will be taken sick with consumption, one with typhoid fever, one with nervous prostration, etc., instead of all having the same disease, the cause being apparently the same? They have said that the predispositions of these persons must have varied; beyond that they do not pretend to know anything. Now it certainly clears up the subject to some extent, and enables us to take one step in advance towards the unknowable, if we can further explain this difference by the very plausible supposition that some diseases have specific germs, which, when introduced into the system under favorable conditions, will produce the same disease and no other.

Poor Hygienic Surroundings. — Some of the opponents of the doctrine of contagion contend that many of these cases should rather be attributed to bad drainage, poor air, moisture, etc. We regret that the information on this important point in our cases is not more full, but we certainly find it recorded in some of them that all of such influences were of the most favorable kind, and it is fair to assume that the same might have been added in others, if it had not been carelessly overlooked. Moisture of the soil, as Dr. Bowditch has clearly shown, undoubtedly tends to increase the number of cases of consumption; but, according to our theory, this should not be regarded as a direct *cause*, but rather, like the exhaustion from nursing just spoken of, as a very favorable *condition*. Moist ground, with this disease, seems to be the "good ground" of the parable, into which if the seeds fall they shall spring up and bring forth fruit, while if they had fallen by the wayside, or among thorns, or in stony places, no matter how good seeds they might have been, they would never have reached maturity.

The Rarity of acknowledged Cases of Contagion. — This has very often been urged as an objection to the doctrine. It is argued that, if the disease is really contagious, well-marked instances ought to meet us on every hand, instead of being so scarce compared to the whole number. To this, in the first place, it is answered that, after all, such cases are not so rare as it might seem. To be sure, the *recorded* cases are rare enough, but these must necessarily form a very small proportion of the observed cases. Many physi-

cians, either because they think they have no special aptitude for writing, or because they consider themselves "too busy," or for other reasons, fail to report cases, of the contagion in which they are fully convinced. As a proof of this we might instance the generally monosyllabic replies to the circulars of Drs. Bowditch, Holden, and others, asking for special information on this point, to say nothing of the laziness, or something else, of those who failed to reply even in this way. Again, as we have already shown, for many years there has been such utter scepticism on the subject, that undoubtedly many physicians have refused to investigate cases with unbiassed and receptive minds, considering, as Copland says, "belief in infection to be *credulity*, and scepticism to be a proof of a *strong-minded physician*." Again, one of the results of this scepticism has been the ignoring of the whole subject in the common text-books and lectures at medical schools to such an extent, that some physicians of the present day, to the writer's knowledge, although otherwise perhaps well informed, have never heard of such a thing as consumption's being contagious. Considering all these facts, then, it is not at all strange that but few well-marked cases have been reported, in proportion to the very much larger number which must necessarily have existed.

Furthermore, a contagious origin need not necessarily be limited to those cases which may seem to us *very obviously* to have arisen in this manner. Everybody now believes that measles is a contagious disease, and it is altogether probable that no new case

ever arises that has not been started by the specific germs of the disease which have come from another case of measles. As an illustration of this truth, we might refer to the epidemic in the Faroe Islands, in 1846, where, on account of their isolation, not a single case had occurred for sixty-five years previously. In April of this year, however, a workman arrived from Copenhagen, who had been exposed there to measles. In three days he became sick, and his two most intimate friends, who attended him, came down with the disease two weeks afterwards. From these cases the disease spread from house to house, until 6,000, out of a total population of 7,782, had been affected by it. No age was spared, and very few escaped excepting those who were protected because they had had the disease more than sixty-five years before. Here, of course, the contagious origin of the disease was very obvious. On the other hand, everybody knows of plenty of cases of measles, concerning which nobody, in spite of diligent inquiry, can tell where the disease was "caught"; and yet these same baffled inquirers are generally convinced that it must have been caught *somewhere*. And so with consumption. If the views (announced within the last few months) of the distinguished German pathologist, Cohnheim, who has justly earned a world-wide reputation for his profundity, are correct, every case must be derived from specific germs, either inherited or inhaled, or ingested with food, or taken into the system in some other way. But, even if we do not accept these extreme views, we certainly ought to be willing to ac-

knowledge, from the reasoning of analogy at least, that many more cases than are now suspected to have a contagious origin may be started in that way. We have hitherto been in the habit of attributing to contagion only those cases where there has been a rather long and close exposure, etc., etc., but perhaps these conditions are not really necessary. Perhaps some sensitive persons may imbibe the germs in a much easier way.

To the second part of the objection now under consideration, namely, that, if consumption is contagious, the emanations from so many independent foci ought to depopulate the earth, or at any rate, the disease ought to be far more common than it actually is, we answer, first, that we should think that a disease which kills from one eighth to one fifth of those that die, to say nothing of those who have it and recover, each case of which by some is considered to be produced either by inheritance or by contagion, is common and widespread enough to satisfy the most exacting; and secondly, whether we accept this extreme view, or acknowledge that only a few cases are produced by contagion, we answer that no *more* are attacked, probably because they have not the susceptibility to the disease. Why it is that the susceptibility to the different contagious diseases differs in different persons no one has yet arisen to explain satisfactorily, but the fact still remains unquestioned. Measles, small-pox without vaccination, and whooping-cough claim as victims almost everybody; while, on the other hand, scarlet fever, less exorbitant in its de-

mands, is satisfied with perhaps half as much. Hydrophobia lays its iron grasp, according to some authorities, on only five per cent of those bitten by the rabid animal, and even those authorities who go to the other extreme do not claim more than fifty-five per cent. These diseases are indisputably contagious. Why this difference? We cloak our ignorance by saying that the susceptibility varies, which is merely another method of stating the fact that the poison finds food for its sustenance in one case which it is unable to obtain in another. An analogy we find in botany and agriculture. Some plants thrive on one soil, but refuse to grow in another.

For this reason, even if the number of recognized cases of consumption produced by contagion *is* quite small, we need not on that account abandon the doctrine, but admit that it may be contagious *to a slight extent* and under certain conditions.

Even this admission may be of very great practical value. Deaths from the use of chloroform by inhalation, as compared with the whole number of cases in which it is used, are exceedingly rare; and yet they are numerous enough to induce the great majority of physicians throughout New England (and now, we are glad to add, of a constantly increasing number throughout the world) to abandon its habitual use, and to confine themselves to sulphuric ether, which is much less satisfactory, on all other accounts than that of safety.

Coincidence. — Closely related to the latter objection, and, in fact, springing from it, is that advocated

by Young (p. 24), Walshe (p. 32), and others, that the acknowledged cases are so few in number that they must be recognized as merely coincidences, "happening by chance." Without stopping to call in question this mode of expression, and to deny that anything can "happen by chance," on the ground that exceptional cases are merely examples of the operation of imperfectly understood laws, we acknowledge that this is one of the most forcible objections to the doctrine of contagion. Still, every now and then we meet with cases in which it is hard to see merely a "coincidence"; as, for example, that (Case 9) in which a man apparently communicated the disease to four wives, or those two cases (Nos. 10 and 21) in which the disease was similarly communicated to three wives, or the much greater number of cases in which two wives have fallen victims.

The Contagium.—Dr. Cotton's forcible statement (p. 41), of the immunity which for many years the attendants and employés of the Brompton Hospital for Consumptives have enjoyed, has been regarded by many as a perfectly conclusive argument against the contagiousness of consumption. If, they say, under such extremely aggravating circumstances, no more who were constantly with the sick came down, it is simply impossible for the disease to be communicated at all.

To show how weak this argument really is, we have only to read the able Prof. Liebermeister's similar statement (made, however, for another purpose), in

regard to typhoid fever, in Ziemssen's Cyclopædia (Vol. I. p. 46) : —

"Physicians and nurses, who take care of such patients, are no more frequently attacked with the disease than are persons who have never seen such cases. Up to the year 1865, I have never seen in the hospitals which I visited (Griefswald, Berlin, Tübingen) a single hospital patient, physician, or nurse attacked with typhoid fever, although such cases are placed in the general wards. Other observers have had the same experience. According to Murchison, during a period of fourteen and a half years in the London Fever Hospital, 2,506 patients with typhoid fever were treated, and, during that time, only eight cases originated in the hospital."

And yet typhoid fever is now regarded as a specific, communicable disease, many even holding that it never originates spontaneously, but that one case must necessarily derive its origin from another case of the same disease. How do we explain this apparent discrepancy ? Typhoid fever is catching, and yet those who are constantly over persons sick with it, if only a few simple precautions are taken, need not contract the disease. Let us briefly clear up this mystery, in the hope, not necessarily that precisely the same explanation will apply to the contagion of consumption, but that we may be encouraged in our expectation of finding, ultimately, some explanation of the latter as satisfactory as that of the former.

For many years there were frequent and excited discussions among physicians as to the contagious-

ness of typhoid fever, chiefly because the distinction between it and typhus fever was not recognized. Those who lived where the latter mostly prevailed, naturally clung to a firm belief in its extreme contagiousness; while those who lived in places where typhoid was more common, influenced by such negative hospital evidence as we have quoted from Liebermeister and Murchison, failed to see the justice of their opponents' views. It was clearly a case of the gold and silver shield over again. Finally, the distinction between the two diseases was clearly recognized, typhus fever was justly put down as extremely communicable immediately from person to person, while typhoid was acknowledged to be not in this way communicable. Typhoid now became universally regarded as a "filth" disease. It was thought to be produced by the inhalation of gases arising from the decomposition of organic substances, especially of fecal masses, and above all of human excrement, or by drinking water contaminated by such decomposing elements. Imperfect sewers, bad drainage, wells poisoned by too great proximity to privies, were thus most constantly blamed for the introduction of the disease; and, indeed, in many cases, after careful investigation, there seemed to be a close connection, as that between cause and effect.

This discovery certainly was a very important step in the attempt to solve the problem, but it did not go far enough to reveal the whole truth.

Many other really specific diseases, such as yellow fever, diphtheria, cholera, the plague, etc., etc., were

likewise classed as "filth" dieases, and exactly the same causes for them were found in the same decomposition of organic material. Now, why in one case should typhoid fever be produced and in another case one of these other diseases? Again, daily observation teaches us that the decomposition of excrement and other organic substances is not of itself sufficient to produce typhoid fever. A little painstaking search would easily disclose multitudes of people in our cities and country towns who are constantly living in an atmosphere impregnated with foul gases from sewers or privies, and even in spite of this, among such people typhoid may be unknown.

Towns or parts of towns with bad sewerage are not on that account necessarily troubled with the disease, but let a single case of typhoid be introduced, and under circumstances so favorable for its propagation, it may spread like wildfire.

"We are, therefore," says Liebermeister, "forced to the conclusion that besides external conditions favorable to the development of the typhoid poison, something else is necessary. Numerous facts render it more than probable that this something necessary is the specific poison itself. In other words, the poison of typhoid fever does not originate in decomposing substances, but finds in them a favorable ground for its growth and multiplication. The most convincing experiences show that typhoid fever never originates in any unusual amount of decomposing matter, nor from any circumstances favorable to decomposition, but is always preceded by the introduction of a case of

the same disease. In most large cities typhoid fever is endemic, and more or less cases are constantly seen, so that in them the result of the introduction of the disease can hardly be traced. In small places, however, it sometimes happens that after a long immunity from the disease a single case will be introduced which gives rise to an extensive epidemic, and for years after this there will be single cases or repeated epidemics."

A great many such instances have been recorded, and they are not only exceedingly interesting, but very conclusive that typhoid is a specific disease, and produced, not by bad drainage, or by excessive fatigue or anxiety, but only by means of its own specific germs, which can produce nothing else; that these germs are contained in the excrement of the patient, but that when first passed they are for a short time innocuous and incapable of communicating the disease to nurses, physicians, or other attendants; that in order to become active, they have to go through a certain stage of development outside of the body, which is best accomplished in connection with decomposition; that in this form they may retain their vitality for a long time, — this latter circumstance explaining the origin of apparently isolated cases which were formerly supposed to have arisen spontaneously; and that excessive fatigue, anxiety, bad air, etc., are very favorable *conditions* for the propagation of the disease, but not in the true sense of the word *causes*.

From this exceedingly brief explanation of the way in which typhoid fever has been found to be propagated, it can readily be understood how the apparent

discrepancy which we spoke of a short time ago can be cleared up. A very large number of cases may be treated in hospitals or private families without spreading the disease, if only the precautions are taken to remove immediately all the discharges of the patients *while they are fresh* (also adding some disinfectant for greater security), to change frequently the underclothing and bed linen, and to secure perfect drainage. If, on the other hand, the discharges containing the germs of the disease are allowed to remain in the sick-room or house until the germs have passed through their necessary steps of development outside of the body, or if, without disinfection, they are cast into a common privy, or if, through defective drainage, in some way or other they permeate the soil and pass into and pollute a well, stream, or other source of water supply, they are liable to be inhaled or drank; and, if the new subject is susceptible, to reproduce themselves with great activity.

Different contagious diseases have different methods of propagating themselves. We do not expect to apply to consumption the same explanation which we have just given of typhoid fever, the logic of which is now pretty generally admitted by physicians to be incontestable; but to show by the reasoning of analogy that, Liebermeister's and Murchison's hospital experiences, which, without further developments, would have been widely quoted as a conclusive argument against the contagiousness of typhoid fever, having now been explained away in such a manner as not to conflict with this doctrine, it is not very presumptuous

for us to expect that Cotton's Brompton Hospital argument in regard to consumption may some time be disposed of in a similar manner. Of one thing, at any rate, we can be certain : that the Brompton Hospital has a great safeguard in its perfect system of *ventilation.* Fresh air is always one of the best disinfectants, and the very important deduction of its necessity in the treatment of every case of consumption is perfectly obvious. Furthermore, all patients in that hospital are obliged to *sleep alone,* and thus a danger so often met with in private life is eliminated.

As to the exact nature of the specific *contagium* of consumption, and the limitations to its modes of operation, we must confess ourselves as yet ignorant. It is altogether probable, however, that it finds an entrance into the system under favorable conditions by inhalation into the lungs with the inspired air, by absorption through the skin, or by ingestion with the food or drink. Whether a partially abraded surface is necessary for the effective lodgment of the germ* is not determined. We close this section by quoting from one of the most widely known and able microscopists of the world, — Dr. Lionel S. Beale, of England : —

" Living tubercular germs will not be considered as very closely related to the contagious particles which are the active agents in the propagation of contagious fevers. There is, however, reason to think

* For an interesting abstract of the present state of opinion with regard to the germ theory of disease, see Dr. Bailey's twenty-page essay in the *New England Medical Gazette* for September, 1880.

that particles of living, growing tubercle exist sufficiently minute to be supported by the atmosphere and carried long distances, while there are many facts considered by many sufficiently conclusive to justify the opinion that tubercular disease of the lungs is at least in some instances contagious." — *Disease Germs*, p. 152.

Suggestive. Almost all the opponents of contagion, after stating their unbelief, and perhaps also their reasons for it, end by saying, "Although we do not believe consumption to be contagious, still prudence would dictate that no one be allowed to sleep with or near a consumptive, or to remain too constantly in close attendance on him, and that the air of the invalid's room be kept as fresh as possible."

Does not this admission of itself indicate a little weakening in their position, a slight apprehension that possibly there may be more in the matter than they like to allow?

CHAPTER VI.

IS IT POSSIBLE FOR TUBERCULOSIS TO BE TRANSMITTED BY MEANS OF FOOD?

THIS question of the possibility of transmitting tuberculosis to man by means of food becomes to us very practical, and of the most vital importance, when we consider the frequent occurrence of the disease among cows and oxen. It is now pretty generally agreed that the "pearl disease" in cows — *Perlsucht*, as the Germans call it (from the grape-shaped appearance of the tubercular masses) — is absolutely identical with tuberculosis in man; one of the greatest authorities for this belief being the learned Prof. Schüppel, of Tübingen, who has made a special and very thorough study of the subject; its most illustrious opponent being Prof. Virchow, of Berlin. Undoubtedly the bovine species is affected to a far greater extent than any of our other domesticated animals, although statistics of the exact percentage of the disease in it have never been gathered, and in all likelihood never will be, until all slaughter-houses are subject to public inspection in this regard.

We are forced to admit, from abundant evidence, that such animals are more often affected than is gen-

erally thought; and that quite a large percentage of cows which have been kept in badly constructed sheds and milked freely, are affected with the disease. We are told that often the amount of tubercular matter found is astonishing, the heart and lungs of the cow sometimes weighing seventy, eighty, and even ninety pounds. The spleen, liver, kidneys, intestines, and different glands are also often filled with tubercle, and rarely even the muscles. The butcher, after a while becoming acquainted with the characteristics of these masses naturally takes pains to remove them from the carcass while dressing it, either through the belief that after they are removed the flesh remains uninjured, or else on account of self-interest and reluctance to the pecuniary loss which a disclosure of the real nature of the meat would certainly necessitate. Many persons who would not be guilty of such conduct, nevertheless have no hesitation whatever in selling milk from cows affected with the same disease.

If the pearl disease in cattle, which everybody agrees is not at all uncommon, *is* identical with human tuberculosis, and if it can be proved that consumption can be acquired by eating the flesh and viscera and milk of animals so diseased, will not this explain the causation of a great many cases of phthisis among us whose origin is now inexplicable? For, universal as the belief in heredity is, it most assuredly will not account for more than a comparatively small proportion of the actual cases, most of them undoubtedly being in some way acquired.

Although there is still a difference of opinion on

this point, and sufficient experiments have not, as yet, been made to enable us to form *exact* conclusions, yet, from what we shall be able presently to advance, it will readily be seen that we are approaching a subject of vast importance to mankind. It is, of course, needless even to hint at the enormous consumption of beef in the world, as well as of the viscera of oxen and cows. Even if, as experiments seem to indicate, that derived from tuberculous animals may to a certain extent be rendered innocuous by the process of cooking, still we have left to reflect upon the widespread use of milk, the greater part of which is never cooked, and therefore, when it comes from tuberculous cows (if certain experiments be confirmed), carries in it the germs of a deadly disease, which may find a lodgment and grow to maturity in any of us or our children, if they fall into a congenial soil.

Prof. Lafosse,* of the Toulouse Veterinary School, states that excessive and long-continued lactation has a powerful influence in the production of tubercle in cattle. On the contrary, those cows which are employed in agriculture (a very common custom in France), as well as those kept for breeding purposes, which give milk for only one or two months, generally escape the pearl disease.

When we consider how universal the use of milk is, especially by young children, who often almost entirely live on it, and when we also consider this tendency of milch cows to become tuberculous, it certainly seems very incumbent upon us to put forth our best endeav-

* Traité de Pathologie Vétérinaire, Vol. I. p. 432.

ors to ascertain the truth or falsity of this theory of transmission of tuberculosis by food. If true, perhaps it also accounts for the comparatively greater frequency of tubercular disease in the *abdominal* regions (*tabes mesenterica*) of young children, where it first finds an entrance into the system, rather than in their lungs.

As an aid towards forming an opinion on this subject, we will now review the very interesting experiments of Prof. Chauveau,* of the Lyons Veterinary School, undertaken with the express object of determining whether or not it is possible to acquire tuberculosis from food containing tuberculous material introduced into the stomach. For some years he had conducted similar experiments with a view to ascertain the modes of action of the contagious principles of other virulent diseases, and had discovered that some of them could act as readily through the organs of digestion as in any other way. Glanders, for instance, was reproduced in this manner, and vaccination was as successful when thus performed as by scarifying the arm. This seemed to furnish the means for explaining the ofttimes obscure transmission of zymotic diseases. These experiments of Chauveau had excited a good deal of attention in the scientific world, but those we are about to relate are still more interesting, because more practical in their results.

Firmly persuaded that, if tuberculosis was contagious at all, it ought to be more readily propagated

* Aug. 11, 1880, the University of Cambridge, England, bestowed on Prof. Chauveau the well-merited honorary degree of LL. D.

when taken with the food than in any other way, because thus a much larger quantity could be introduced, he instituted a series of experiments to prove or disprove his theory. With a desire to have the result of his experiments conform as nearly as possible to the ideal results of similar tests on man, he selected the bovine species, on account of its frequent natural tendency to the disease. On this very account, he was compelled to exercise remarkable caution, lest the animals selected should be already affected, and therefore he chose calves born and bred outside of all those influences which are usually supposed to favor the development of the disease. For a "control" experiment he decided to place under the same conditions exactly (with the exception of not feeding them with tuberculous material) one or more animals of the same species.

Sept. 18, 1868, four calves, from six to twelve months old, after careful examination pronounced to be perfectly healthy, and coming from a locality where phthisis was unknown, were purchased. Each of three of them, numbered 1, 2, and 3, respectively, received, on the day after its arrival, about an ounce of tuberculous matter from an old phthisical cow, in one of the Lyons slaughter-houses. This material was pounded up in a mortar, mixed with water, and then given from a bottle in small quantities at a time. The calves had previously refused to drink water into which the virus had been introduced. The other calf, numbered 4, received none of this material, but was kept for purposes of comparison.

On Sept. 23, No. 2 (six months old) had a fœtid diarrhœa, but soon recovered. On Oct. 1, it seemed in good health, likewise No. 3, of the same age. At this time No. 1 (a year old) seemed to have failed in flesh and strength, and its respirations were increased in frequency. On Oct. 5 and 7, Nos. 1 and 3 were made to swallow some tubercular material from the lungs of a cow not so far advanced in the disease. By Oct. 9, No. 1 was reduced to a remarkable degree of emaciation, and coughed badly morning and evening, especially after drinking, and its coat became very rough. Soon after, No. 2's coat assumed the same appearance, and it coughed and lost flesh, still, however, preserving its appetite. No. 3 remained in good health until Oct. 25, after which, however, the disease progressed with such fearful rapidity, that in eight days it could scarcely be recognized. It coughed constantly, and the left submaxillary gland was as large as a goose's egg.

During all this time No. 4, who, except that he had not been made to swallow any tubercular material, had been treated exactly as the rest, *remained in perfect health.* At the beginning of the experiment he was the puniest of all, but after fifty-two days, on Nov. 10, when it was determined to bring it to a close, his healthy condition was in striking contrast to the utterly miserable appearance of his unfortunate companions.

Nos. 2 and 3 were now killed, and No. 1 was reserved to kill at a later date, when the disease should be more advanced. In No. 3 the autopsy revealed

very extensive tuberculosis, especially in the intestines and mesenteric glands, the latter weighing over four pounds. The tubercles throughout the intestines were nearly all ulcerated, and were particularly numerous in Peyer's glands. In the *true* stomach some were found, but not in its other compartments. Through the lungs were scattered about forty crude tubercles, varying in size from a pea to a filbert, and gray granulations on the pleura were seen. The bronchial and mediastinal glands had grown quite large with disease. Besides, there were tubercular ulcerations of the larynx and pharynx, and infiltrations of the cervical and submaxillary glands.

The autopsy of No. 2 revealed similar results. These were certainly remarkable exemplifications of the theory, especially when we consider that glandular tuberculosis is more common than the other forms in early life.

As a counter test, Chauveau stated that facts prove that calves may be made to swallow common pus in quite large quantities with impunity, thus showing that the results just described were not due to purulent infection.

Chauveau thinks now it is no longer possible to doubt the virulence of tuberculosis, and adds: "It now appears proved that the identity of tuberculosis with the other virulent diseases is so complete and so absolute that we must either recognize its virulency or deny the existence of virulence altogether. There is no middle place in this dilemma."

Subsequent experiments (human tubercle being

used in some) only confirmed Chauveau in his views; nor was he slow to see the practical application of them. On the one hand, creatures drinking from the same ponds or troughs, or confined in the same stable, or eating side by side in the pasture, are constantly liable to swallow the sputa or other secretions of their comrades, which, if phthisical, may thus infect them. On the other hand, mankind may easily acquire the disease by eating the flesh and organs of phthisical animals, which are every day sold in the public markets without suitable governmental inspection, or by drinking the milk of diseased cows, there being at present no restrictions on the sale of such milk. He does not consider the flesh itself as virulent, except when it contains tuberculous lymphatic glands, nor the blood, as a rule, although the blood retained in the flesh *may* perhaps sometimes contain tubercular elements. Cooking, if thorough, he finds will completely destroy the poison, — a remarkably fortunate circumstance, which probably prevents a great deal of sickness. Nor does he believe that milk can infect, unless the udder is tuberculous, to a slight extent at least.

Harms and Gunther, veterinarians of Hanover, made rabbits tuberculous by feeding them with the flesh and lungs of a pig and cow affected with the disease; but although the young of these rabbits were suckled for ten weeks, when killed at this time no tubercles were found.

Leisering, of the Dresden Veterinary School, fed a sheep on tuberculous glands of a cow. Six weeks

after appeared cough and emaciation, followed by shortness of breath, and ten weeks afterwards auscultation and percussion revealed infiltration of the right lung. On the eighty-fifth day the sheep was killed, and an autopsy proved the existence of numerous tubercles in the intestines, liver, and lungs, as well as in the mesenteric and bronchial glands. Similar results were obtained on other sheep and rabbits.

The results of further experiments we find summarized as follows in the article (already referred to) by George Fleming, M. R. C. V. S., in the *British and Foreign Medico-Chirurgical Review* for October, 1874: —

"Gerlach, of the Berlin Veterinary School, thus describes the results of his experiments: 1. The tuberculosis of cattle is very infectious. 2. The tubercles covering the serous membranes, as well as those in the other organs, are as infective, and produce the same tubercles as the tuberculous matter of the lungs. The identity of pulmonary phthisis of cattle and general tuberculosis cannot be doubted. 3. Infection can be produced after inoculation, as well as after ingestion of the tuberculous matter. 4. The *flesh* of animals affected with tuberculosis possesses, in certain conditions, the power of infecting, though to a less degree than the tuberculous matter. 5. The temperature of boiling water destroys the infective principle; though boiled tubercles often, nevertheless, preserve a certain degree of virulence. It is in this as in trichinosis. Although the temperature of boiling water, or even a lower temperature, destroys

the parasites, yet there may be found in the centre of the boiled flesh living trichinæ; muscle being, in general, a bad conductor of heat, and the high temperature only reaching its interior after some time has elapsed. In some experiments it will be found that tubercles an inch in diameter will, after half an hour's boiling, still possess infective properties, though these are, of course, diminished. Gerlach's experiments have been, and are now being carried on with all kinds of animals, and particularly with cooked and uncooked flesh and uncooked milk. He asserts that the infective properties of this fluid can no longer be doubted or denied.

"Zürn, of the Jena Veterinary School, has fed pigs, first with the milk, and then with the flesh of a phthisical cow, and produced various degrees of tuberculosis in them.

"Böllinger, of the Zurich Veterinary School, has made nineteen experiments, the results of which led him to the following conclusions: —

"1. Tuberculous matter obtained from man and inoculated in the dog, produces a typical miliary tuberculosis of the pleura, lungs, liver, and spleen. Inoculations on carnivorous animals in general are negative, or only produce an insignificant local reaction.

"2. The inoculation and ingestion of tuberculous matter from the ox produces, in herbivorous animals (goats), tuberculous infection in two forms, — miliary tuberculization of the peritoneum, and caseous deposits in the intestinal mucous membrane, as well as in the mesenteric glands.

"3. The contents of the bronchia of the tuberculous lungs of an ox produce the same effects as the caseous matter of the lungs, when inoculated or ingested.

"4. The ingestion of fresh tuberculous matter from the ox has no effect on carnivorous animals; with herbivorous creatures, on the contrary, it produces intense tuberculous infection, characterized by caseous lesions of the intestinal mucous membrane and mesenteric glands, hypertrophy of Peyer's patches, and the eruption of miliary tubercles in the peritoneum, liver, and lungs.

"5. The tubercle virus is active in small doses; twenty to twenty-five grammes of tuberculous matter from the lungs may kill such creatures as the goat, in two months.

"6. The ingestion of pus alone from the caseous lesions does not produce tuberculosis in the goat.

"7. Certain forms of tuberculosis induced by the ingestion of tuberculous matter present, in an anatomical and pathological point of view, a great analogy to human scrofula, and, like it, are manifested by caseous degeneration of the cervical and mesenteric glands.

"The experiments of Villemin, Chauveau, Klebs, Gerlach, Bagge, Semner, Gunther and Harms, Zürn, Biffi, and Vergad, and some personal experiments previously made, are quoted by Böllinger, who arranges them under four heads or sections, as follows:—

"1. Ingestion of tuberculous matter obtained from man. In two pigs and two rabbits, a negative result. In one pig, enlargement of Peyer's patches, and mesenteric glands,and caseous degeneration of portions of these.

"2. Ingestion of tuberculous matter from the ox; fresh glands, caseous matter, the contents of the bronchia. Animals experimented with: five sheep, two goats, four pigs, eight dogs, a large number of cats, twenty rabbits, one porpoise, eight pigeons. A negative result with the dogs and cats; nearly always a positive result with the pigs, sheep, and goats. Most frequently, with these animals, there was caseous degeneration of the intestinal mucous membrane, mesenteric glands, sometimes the cervical glands, and the lungs. In three sheep there was veritable 'tabes mesenterica.' With the rabbits, the ingestion of raw tuberculous matter produced results sometimes positive, sometimes negative. In one instance, the ingestion of boiled tuberculous lymphatic glands developed general tuberculosis; but, on the other hand, boiled tubercles had no injurious effect on five rabbits. And a pig fed with the same material only exhibited, after death, tumefaction of the mesenteric glands. Cooked or uncooked tubercles given to other rabbits infected them; and the ingestion of tubercles from an ox infected a porpoise, but had no influence upon two pigeons. The ingestion of caseous pus alone had no effect on a sheep.

"3. Ingestion of the *flesh* from phthisical oxen, or those artificially infected. Positive result in three pigs; general tuberculosis or alterations in the lymphatic glands. In one pig leucæmia, scrofula, and tuberculosis. Result always negative with rabbits when fed with raw or cooked flesh.

"4. Ingestion of the milk of a tuberculous cow.

Three pigs, three calves, one sheep, two goats, two cats, and fourteen rabbits. In the three pigs miliary tuberculosis, and lesions analogous to those of scrofula; in the two cats, a negative result. A positive result in two rabbits; a negative result in fourteen rabbits fed with the boiled milk.

"Böllinger thinks it possible, or rather probable, that intestinal tuberculosis, consecutive to pulmonary phthisis, may be produced by the sputa being swallowed and passing into the intestines. He also thinks it proved that scrofula and tuberculosis are only two forms of the same disease, at different periods of development.

"Klebs has been successful in producing tuberculosis by giving animals milk from those which were diseased, and his experiments, therefore, have an extremely important bearing. In addition to rabbits and guinea-pigs, — creatures which appear to be very susceptible to the artificial production of the malady, — he accidentally induced the disease in a dog by feeding it with the milk of a cow in the last stage of phthisis. The results of his experiments led him to the conclusion that the use of this milk always produces tuberculosis, which commences as an intestinal catarrh, and then assumes the form of tubercles in the mesenteric glands; it afterwards affects the liver and spleen, and subsequently the thoracic organs. He asserts that the tubercle virus is present in the milk of phthisical cows, whether they are slightly or gravely affected; and that it chiefly exists in the serous portion, as when milk has been so filtered as to

deprive it of its solid particles, the fluid portion appeared to be as active as when the malady had reached an advanced stage in the animal from which it had been procured.

"He admits that it may produce no injurious effects on vigorous subjects, and he has even observed fully developed tubercles to be absorbed and disappear after a time. He thinks it probable that the virus of tuberculosis may exist in varying proportion in the milk of phthisical cows, according to the extent of the disease in them; and he is further of the opinion that the malady may be developed in children born without any tendency to it, through the medium of the milk of the mother or nurse.

"Since Kleb's researches were published, Viseur, an able veterinary surgeon at Arras, France, has been successful in producing tuberculosis in cats, by feeding them with tuberculous matter. One of these animals was found to have all the lymphatic glands enormously hypertrophied, the mesenteric glands greatly increased in size, and the lungs studded with white, hard tubercles, — some of them as large as a grain of barley. These cats ate the tuberculous matter voluntarily.

"St. Cyr, of the Lyons Veterinary School, has likewise proved the transmissibility of the disease by the stomach.

. . . . . .

"From what has been already ascertained, there is every reason to view with grave suspicion the use of the flesh of phthisical cattle as food, especially if the

disease is much advanced and the tissues generally involved. But with more reason the milk from cows affected with tuberculosis should be prohibited, more particularly for the use of infants, who mainly rely upon milk for their sustenance, and whose powers of absorption are very active. Even if such milk did not possess such dangerous infective properties, its deficiency in nitrogenous elements, fat, and sugar, and the increased proportion of earthy salts, would alone render it objectionable as an article of diet. It has long been known that it was liable to produce diarrhœa and debility in infants ; but though many children fed on such milk have died from general or localized tuberculosis, the part probably played by this fluid in its production has not been suspected."

M. Toussaint has been able to produce, almost at will, tuberculosis in pigs, by feeding them on lungs of tuberculous cows.*

The *British Medical Journal* of July 31, 1880, narrates the results of some experiments which M. Puech reported to the Académie des Sciences, Paris. A cow which had been sold to the butcher to be killed, still continued to give three or four litres of milk every day. Discovering that the cow was affected with phthisis, he fed two sucking-pigs and two rabbits with her milk. These animals, when killed, showed tubercular deposits in strict proportion to the length of time the milk had been administered. The post-mortem examination of the cow confirmed Puech's diagnosis.

On the other hand, we find mention of experiments

* Journal de Med. et de Chir., May, 1880.

undertaken by the Italian Perroncito, the Germans Möller and Schreiber, and a few others, in which animals fed on the milk of tuberculous cows did *not* acquire the disease. In 1873, Colin* read a paper before the Academy of Medicine, Paris, on the "non-transmissibility of tubercle by the ingestion of tubercular matter," in which he stoutly maintained that it *could not* be so transmitted, because, forsooth, he had failed in experiments with thirty animals. It is hardly worth while to present here a translation of his paper, because it is merely a series of negations in detail.†

Of course, one positive fact is worth many negative facts like these.

We have already stated that the most illustrious opponent of the identity of the pearl disease in cattle with human tuberculosis is Prof. Virchow, of Berlin. One of his objections is, that instead of being of small size (miliary) and ulcerating, as typical tubercle does, the pearl nodules grow very large, soon becoming the size of a pea, then that of a potato, and finally getting to equal a man's head, growing harder and harder all the time from contraction and calcification, and never ulcerating at all. They appear mostly on the surface of the serous membranes, especially the pleuræ and peritoneum.

* Bull. de l'Acad. de Med., May, 1873, p. 557.

† Colin believes in the subcutaneous inoculability of tubercle, and believes that when tuberculization results after an animal has swallowed tuberculous matter, it is really owing to its inhaling some of it into the lungs while swallowing, an accident which he tries to prevent by avoiding force in feeding, and by not giving it in the form of broth. He believes, therefore, in the transmission of tubercle, but not by the alimentary canal.

Virchow's experiments, undertaken with a view to settling the question as to the amount of injury done by eating the flesh and drinking the milk of cattle affected with the pearl disease, extended over a period of four years. In making his final report, he says that it might seem to any one a very easy thing to decide such a question one way or the other, but practically he found several difficulties in the way which proved almost insurmountable. His first difficulty was to distinguish the real pearl nodules, especially in their earlier stages, from other nodules, which bear a close resemblance, but are really quite unlike them; as, for instance, the cheesy nodules often met with in the lungs (resulting from a chronic peri-bronchitis or pneumonia), which have become contracted, fibrous, and even calcareous.

For his experiments he selected rabbits, guinea-pigs, cats, calves, goats, sheep, and especially pigs, a great number of which he used. Warned by Gerlach's ill success with dogs, he let them pretty much alone. His results were to some extent similar to those of his predecessors, but he hesitated to explain them in the same way. In all these animals he found a difficulty in deciding what was and what was not pearl disease. Especially was this so in the case of pigs, in whose livers he often found encapsulated cysticerci and echinococci (the latter also in the lungs), looking in their little sacs very much like independent nodules, and he argued that such appearances might easily deceive observers, and be at once set down as real products of the pearl disease. He also felt disappointed

because he did not always find the same glands or organs affected in his animals, especially when they came from the same litter. In the pig, sometimes the mesenteric glands were not, and the submaxillary were affected, Peyer's patches never being attacked. Besides, none of the changes revealed at the post-mortems of animals which had been fed on the pearl substance for five or six weeks seemed to be of recent date, whereas he had expected a series of changes corresponding to different periods of the experiments.

On these accounts, and others, it may readily be inferred that Virchow's conclusions were rather indefinite. All that he could be said to be *sure* of was that there were more *sickly* animals among those experimented on than among those kept for comparison, or among animals of the same species in their natural condition; and that the suspicions which many are beginning to have, that the products of the pearl disease are injurious, are, in reality, well founded, and that the public authorities should prohibit the sale of such products. He finds so little evidence of the injurious results from eating simply the *flesh* of such animals, provided that there are no pearl nodules in it, that he should hardly feel warranted in advising the government to prohibit the sale of such flesh, after all the nodules had been carefully removed.

As a counter test, he fed other animals with common meat, healthy in other respects, but in different degrees of decomposition, in order to determine whether or not this would produce the same effects as the pearl nodules, but no changes were produced in the animals after eating this meat.

His experiments relating to the feeding of animals with the milk of cows affected with the disease were not extensive enough to be satisfactory to him, and he therefore does not give a decided opinion on the subject, although it seemed to him probable that the milk of those cows whose udders were affected might be injurious.

Although he recognizes the injuriousness of such products when eaten, still he does not feel that it has been proved that there is a *special* virus in the pearl disease, and thinks that experimentation in the future should take the direction of feeding with still other morbid products, to see if some of them will not produce results similar to the above.

As bearing particularly on the question of the identity of the pearl disease in cattle with human tuberculosis, we may add that Klebs asserts that by inoculating calves with human tubercle, he has produced the characteristic pearly eruptions of the bovine disease.

Furthermore, the series of cases lately reported in *The Lancet* (June 19, 1880) by Dr. Charles Creighton may throw considerable light on this point. He had attended several patients whom he had considered to be affected with common acute miliary tuberculosis. At the autopsy, however, he was surprised to find exactly the same peculiar pearly-nodule formations which are wont to be seen in cattle who have died of the pearl disease. From these cases Dr. Creighton infers that even if the two diseases are not identical, they are at least intercommunicable.

Whether identical or not, the *practical fact* that

bovine tuberculosis can be communicated to man seems now to be pretty well established.

The two following paragraphs we translate from the very interesting *Thèse* of Dr. Paul Spillmann, *De la Tuberculisation du Tube Digestif*, published at Paris in 1878:—

"M. Metzquer, who has repeated Chauveau's experiments, pretends that he obtained, besides intestinal lesions, a generalization in the lungs and other organs, and he persists in considering these alterations not tubercles, but emboli. It seems to us, however, that anatomy as well as pathology prove him to be absolutely wrong in this view. In fact, it is impossible to conceive of emboli starting in the intestines and reaching the lungs, for the hepatic capillary ramifications of the portal vein would arrest them in their course. When we have cases of deep ulceration of the intestines, as in dysentery, for example, we never find pulmonary emboli, but abscesses in the liver. . . .

"There is one point to which we must call particular attention in closing. In all animals who have been rendered tuberculous by swallowing tubercular substances, a very marked predominance of tubercles in the abdomen has been noticed. When the animal has been killed not very long after the beginning of the experiment, the lungs are almost perfectly sound. Only a very few gray granulations are to be found in their parenchyma, or on the pleuræ, and in the bronchial ganglions there is barely a beginning of the process of cheesy degeneration. If the post-mortem examinations were carried no further than the chest,

in certain cases it would be thought that the animals had not been affected at all. It is, therefore, in the abdomen that tuberculosis transmitted by ingestion concentrates itself. The first symptoms, gastric and intestinal catarrh and diarrhœa, as well as swelling of some of Peyer's patches, yellow granulations dotting the external surface of the intestine, cheesy degeneration of the mesenteric ganglions, and tuberculous nodules in the liver, spleen, and kidneys, are referable to the alimentary canal or its immediate neighborhood."

Although *tubercular expectoration* cannot strictly be called *food*, yet, as it has, in a number of cases, seemed to convey the disease, when swallowed and absorbed by the alimentary mucous membrane, this seems the most proper place to speak of such cases.

The first one which we can find on record was reported by Malin in the *Gazette Médicale*, 1839, p 634, and may be rendered as follows :—

"A woman fifty-eight years old, who had been consumptive for several years, kept a pet dog, which for a year eagerly swallowed her phthisical expectoration. At the end of six months the dog coughed up copious purulent expectoration, and finally became very thin, and died. The woman then obtained another dog a year old and a foot high. Although she gave him plenty of milk and meat, he soon evinced the same taste as his predecessor. Six months afterwards he also became sick, and died at the end of twenty weeks. Both of his lungs were found almost entirely destroyed by phthisical suppuration."

Villemin gave to four guinea-pigs about an ounce

and a quarter of sputa expectorated by persons in the second stage of consumption, after mixing it with bran. All four of them, at the end of three months, when they had died from the effects of disease, or had been killed, showed tubercular granulations in their lungs or ganglions.

Klebs * is very positive in his opinion that tubercular ulceration of the intestines can be produced by swallowing the expectoration of consumptives.

Parrot has likewise produced tuberculosis in guinea-pigs by making them swallow tuberculous sputa.

Better known than any of these experiments, perhaps, are the instances mentioned by Jacobs † of dogs rendered tuberculous in this way.

Dr. Tappeiner,‡ of Meran, fed eight dogs with tuberculous sputa, mixing about half an ounce with each one's food every day. In the intestines and lungs of two of these dogs miliary tubercles were found after six weeks' feeding, but in the other six nothing unusual was found. Tappeiner, comparing the results in these experiments with those where he compelled eleven dogs to *inhale* atomized tuberculous sputa (p. 166), concluded that absorption by the intestines was not nearly so active as by the lungs. The intestines of those who inhaled the sputa had not become tuberculous at the time the dogs were killed.

Omitting mention of similar cases which are on record, we will quote the last case which has come to

* Traité d'anatomie pathologique, 1876.

† Presse médicale belge, 1870.

‡ Virchow's Archiv., 1878, T. XLVII. p. 393.

our notice, reported in the *British Medical Journal* of May 22, 1880, by D. H. Cullimore, M. K. Q. C. P.:—

"About six years ago, when I was acting as resident surgeon at Mandalay, a Bengalese servant, suffering from advanced pulmonary consumption with copious expectoration, came under my notice. One of my dogs — a pariah — developing his natural talent as a scavenger, was, as I afterwards learned, in the habit of frequently visiting the house of the sick man, and lapping up the expectorated matter. How long this had been going on I cannot, with certainty, say, but a few days after the death of the man the dog appeared out of sorts, refused his food, rapidly emaciated, had a cough, which increased quickly in severity and was attended later on with a tenacious and glairy discharge from the mouth and nose.

"The stethoscope detected moist râles, with rhonchus and sibilus over a greater part of the chest. These symptoms continued to grow worse for a week or ten days, when convulsive fits of about five minutes' duration, and occurring several times in the course of the day, made their appearance; these fits were of an epileptic form, or tetanic character. The poor animal moaned a good deal, and appeared in great pain as he lay on his back reeling and kicking about. On the second day from the commencement of the fits, and about the twelfth day from the beginning of the disease, there being but little hope of his recovery, a large dose of prussic acid was given, which speedily proved fatal.

"The post-mortem inspection was limited to the

contents of the skull and the chest, as these were the parts supposed to be more prominently diseased, and in both were found lesions to account for the symptoms during life, and their probable cause. Both pleuræ were adherent, the adhesions being recent, and the lungs were studded with softened patches in varying stages of caseous degeneration, many of them containing muco-purulent matter, with which the minute and larger bronchi were also clogged. . . . As European dogs are liable to degenerate in the tropics, and become an easy prey to disease, I may say that this dog was a native, had previously enjoyed good health, and, as the weather was not cold, I have no reason to believe that this acute affection was due to any other cause than contagion ; the tubercular virus being fresh, and therefore in the state of greatest activity or supposed vitality."

The dangers liable to arise from using as food meat and milk from phthisical cattle have been pretty well indicated in the preceding pages. Although we cannot quite yet affirm that the question has been definitely settled, yet the preponderance of evidence and the authority of the greatest names in medicine are decidedly in favor of the transmissibility of tuberculosis from animals to men in this way. Even those who oppose these *specific* views, all of them, with one consent, agree that such food is not desirable, and even injurious in a general way.

Now what is the remedy? *Careful, honest governmental inspection of all the meat that comes into our markets, especially at the slaughter-houses, and of the*

cows which furnish our milk, with particular reference to the existence of this disease. It may be a difficult and expensive undertaking, but, for our safety, it must be done.

To show how necessary it *may be* at times, especially in our large cities, we quote the following graphic account from an article by Dr. A. N. Bell, in the New York *Sanitarian* for August, 1877, which many of our readers will remember. Nobody pretends that these are average cases, but if such extremes are possible in this country, with our boasted enlightenment, is it not likely that the average is at least a little below par?

"May 23, 1877, a party from New York, consisting of Mr. Henry Bergh, president of the Society for the Prevention of Cruelty to Animals, Dr. Janes, assistant sanitary superintendent of New York, Mr. A. Berghaus, chief of Frank Leslie's staff of artists, and four or five police officers and other persons, visited certain cow-stables in the outskirts of Brooklyn. At the first one, at Blissville, one poor white cow was seen with both eyes nearly totally destroyed by ophthalmia caused by ammoniacal gases being continually generated under her body. The full proportion of stump-tailed animals was seen, and a great number without teeth.

"The section in which the poor beasts are compelled to stand from one year's end to another is, on the average, two feet and eight inches wide by six feet long. The height is six feet. Some slimy swill was standing in the troughs, and one of the myrmidons informed Dr. Janes that it had been there since

early in the morning. Into it the thermometer was plunged. ' The temperature was 102 degrees Fahrenheit. Another specimen of the swill gave a temperature of 109 degrees Fahrenheit. The swill, as it ran into the vats, was boiling; one of the officers plunged the thermometer into the filthy mess, and the glass was fractured by the heat

"After leaving Blissville, the party proceeded to the dens on the corner of Marcy Avenue and Floyd Street, Brooklyn.

"Twenty-eight cows were here confined in one shed, which is only a little over seventy feet long. They were fastened with ropes about their necks, to which there were chains six inches long. When the poor animals lie down, they go through a sort of hanging process. In another shed, not much larger, there were fifty-one head of cattle. Seventy-nine steers were found in a shed that measures seventy-two by fifty feet, and is filthy enough to turn one's stomach; sea-sickness is preferable to it. The cattle are kept here and fattened on swill by one Block, who keeps a slaughter-house in New York.

"Several wagons were seen about the place, all bearing the inscription, ' Pure milk.'

"A visit was then paid to the Brooklyn Board of Health, and the following conversation took place between Mr. Bergh and the secretary of that body.

"MR. BERGH. — 'Mr. Secretary, I and my associates have been visiting some swill-milk dens in your city, and have called to see if your board intends to take any action in the matter.'

"MR. SECRETARY. — 'What ones have you visited?'

"MR. BERGH. — 'We have been to the establishments at Blissville.'

"MR. SECRETARY. — 'Oh, they are out of our jurisdiction.'

"MR. BERGH. — 'They are. Well, we have also paid a visit to the filthy pest dens of Ehlers, and I think they are in your district.'

"MR. SECRETARY. — 'Yes, they are in our district; but everything is nice and clean about them.'

"MR. BERGH. — 'Everything is not nice. We have just left there, and a more filthy place I never saw in all my life. If the Inquisition should be revived, and if that body should endeavor to institute some means of torture for cows, they could not hit upon a more severe and outrageous plan than the one that is practised daily at the beastly places in question. The proprietors violate the law daily in feeding their cows with distillery waste. I have the law here, and will read it to you. The act passed by the Legislature in 186–.'

"MR. SECRETARY. — 'Oh, that law has been abolished as far as this county is concerned.'

"MR. BERGH. — 'Well, then, here is another.'

"MR. SECRETARY. — 'When was that passed?'

"MR. BERGH. — 'At the same time. I presume that this has been abolished too.'

"MR. SECRETARY. — 'Yes, sir; all the laws passed prior to June 21, 1875, at which time this board was created, were abolished by the creation of this board.'

"MR. BERGH. — 'Then you have no State laws on

Long Island, and these swill people can do as they like.'

"MR. SECRETARY. — 'No, we do not allow them to keep cows without a permit from this board.'

"MR. BERGH. — 'I don't believe I can do much good, then.'

"MR. SECRETARY. — 'No, sir; I don't think you can, for we are endeavoring to do all we can, and we can't do much.'

"MR. BERGH. — 'Very well, sir. My province, I am fully aware, is only the animals; for if the people prefer swill milk, I have no right to interfere with their taste.'

"MR. SECRETARY. — 'I prefer Orange County milk myself, and shall endeavor to keep the swill-milk traffic within bounds; but we have very hard work to do it. Only last week we convicted a man named Luke Flannagan, on the corner of Thirty-ninth Street and Third Avenue (Brooklyn). It is hard work to convict these fellows, for we are obliged, when one judge discharges them, to rearrest them and take them before another judge. Sometimes the judge suspends sentence after they are convicted, and we are virtually defeated.'

"MR. BERGH. — 'Well, sir, as all the laws are suspended in Brooklyn in regard to the swill-milk establishments, I will go home. Good day.'

"The party then withdrew and returned to New York.

"With a knowledge of these conditions, and with the hope of adding somewhat to the reasons for their

suppression, some six months ago I secured a privilege with an offal contractor, of making post-mortem examinations of cows; since which time, of eleven examined, ten evidently died of tuberculosis of the lungs. The other one, which also had the disease incipiently, died in first calf-birth. One of them was of a choice breed, imported at a cost of about $500; she had been carefully kept in a small, dark, and close stable, and allowed but little exercise. All the rest were from stables such as those above described. These eleven examinations are too limited, in themselves, for the elucidation of any doctrine; but taken in connection with the testimony from other sources, and the conditions common to city cows, they are suggestive of dangers worthy of the most serious consideration. That the milk of cows affected with tuberculosis is likely to induce that disease, usually commencing as intestinal catarrh, is not only rendered probable by the experiments cited, showing that it has this effect when fed to domestic animals, but this evidence receives additional strength from the prevalence of fatal intestinal catarrh, common to infants fed on cow's milk, in most American cities.

"From a physiological standpoint, all observers appreciate the important difference which exists in regard to the requirements of nutrition between the young of all animals, and those of mature growth, and in none is it more marked than in the human species. In infancy, and throughout the growing period of life, not only is it necessary to repair the waste of tissues constantly going on in the young, as well as in the old, but the

frame has to be built up to the degree of development it is destined to reach. To meet this demand on the part of the animal economy, is the foundation of the enormous appetites and rapid digestion common to the young of all animals. Every organ of the body, in addition to sustenance, appropriates the additional amount of nutriment necessary to give it required bulk. The current of nutritive life in the young is, therefore, an exceedingly rapid one, and the especially remarkable feature of it is, that the blood itself, from which the organism is built up, receives its impressions and modifications — its constitutional predisposition — chiefly from the nature of the food during the growing period of life. Such, indeed, is the main foundation of the so-called 'hereditary' diseases, for it is well known that the offspring of progenitors with well-marked constitutional tendencies may often have their hereditary tendencies wholly overcome, and a radical change effected, by a change in the quality of the nutriment, and the physical surroundings; and, on the contrary, the offspring of those possessed of the highest degree of physical organization may be dwarfed to the lowest degree of degeneracy, by withholding the necessary conditions of healthful development, or by subjecting them to poor food and a vitiated atmosphere.

"That city cow-stable milk is peculiarly liable to produce diarrhœa, debility, and marasmus in infants is well known; but that such fatal affections are *probably due to localized tuberculosis, communicated through the milk upon which they are fed*, appears hitherto not to have been suspected. . . .

"It is in some respects anomalous, perhaps, but nevertheless true, that in the marked progress of public hygiene, in recent years, veterinary sanitary science has been the vanguard, and chiefly because people are wont to respond with more alacrity and with greater liberality for the suppression of an epizootic among their horses or a pleuro-pneumonia among their horned cattle, than for the arrest of small-pox or the prevention of cholera infantum. Individuals, communities, and states will make liberal appropriations to improve the breed of stock or contribute to the contest for a prize at a dog-show, while they will refuse assistance or oppose a tax for the admission of air and sunlight into a stunting school, or for the drainage of a marsh which, by its emanations, is a perennial source of human degeneracy, disease, and death. It is therefore fortunate, that in the progress of veterinary sanitary science, it has been discovered that many of the most fatal and loathsome diseases which afflict the human race are equally common to — if, indeed, they do not actually take their rise from and originate in — domestic animals inhumanly treated. Scrofula, small-pox, syphilis, malignant pustule, hydrophobia, trichinosis, and tuberculosis are examples and, consequently, veterinary sanitary science may well be regarded as the right arm of public hygiene."

CHAPTER VII.

THE INOCULABILITY OF TUBERCLE.

When the poisonous virus from the nostril of a horse or ass sick with glanders is directly introduced into the circulation of another horse or ass not so affected, or even into that of man, as not infrequently happens, owing to an abrasion of the skin or mucous membrane, or even without such manifest abrasion, the animal or person thus inoculated will, if susceptible, be attacked not merely by *a* disease, but by the *same* disease, as has been fully proved time and time again, both accidentally and intentionally, as the result of experiment. This tendency to the reproduction of the same disease makes it what is called *specific*. To determine whether consumption is or is not entitled to be ranked among the specific diseases, many experiments have been made, on animals, of course, by different physicians, in the way of inoculating them with tubercular or other material, and then watching the result. If not the first, by far the most prominent of these experimenters was the able Frenchman, Dr. Villemin, professor at the Val-de-Grâce Hospital, Paris. In a series of articles on the cause and nature of tuberculosis and its transmission by inoculation, in

the *Gazette Hebdomadaire de Medicine et de Chirurgie* for Dec. 15, 1865, and Oct. 19 and 26, Nov. 9 and 30, and Dec. 7, 1866, he gives an account of his experiments and the conclusion which he draws therefrom. The latter he sums up as follows: —

"Tuberculosis is the effect of a specific or virulent causal agent, — in one word, of a virus. . . . Introduced into an organism capable of being affected by it, this agent is reproduced, and at the same time reproduces the disease of which it is the essential principle and the determining cause."

His first experiment was performed on March 6, 1865. Two healthy, sucking rabbits, about three weeks old, were selected, and were protected from the weather, and otherwise carefully provided for. Behind the ear of one of them a small subcutaneous wound was made, and into it were inserted two fragments of tubercle and a little puriform liquid taken from the lung and intestine of a patient who had died of phthisis thirty-three hours previously. More tubercle was inoculated on March 24 and April 4. On June 20, the inoculated animal, which still seemed healthy, was killed. On examination, very small, gray, miliary tubercles were found in the stomach, intestines, and kidneys, and larger masses, about the size of peas, in great abundance in the lungs. The microscope confirmed what the gross appearances indicated. The other rabbit, whose quarters, food, exercise, etc., had been precisely the same throughout, but who had not been inoculated, was then killed, and found perfectly free from tubercle.

For his second experiment, on July 15 he inoculated three fine, healthy-looking, well-fed rabbits, living in the open air, and having access to a large box in case of storms. A week later the operation was repeated, and a fourth rabbit living with them was also inoculated. The four rabbits were killed about two months after, and in the lungs of all of them abundant transparent gray tubercles were found.

M. Villemin thought that the results in these two experiments were important enough to report to the Academy of Medicine, and he accordingly did so, giving, at the same time, the theories suggested by them.

For a third experiment, on Oct. 2 three pairs of rabbits were procured, and after one of each pair had been inoculated, all were put into the same habitation, and lived together under precisely the same conditions and with the same surroundings. On Oct. 24 the inoculation was repeated, and also a fourth full-grown and very vigorous rabbit was for the first time inoculated. The results were as follows: —

Pair No 1. — Nov. 23, the inoculated rabbit being found dead, a post-mortem examination showed two red patches of congested lung substance occupying the front and posterior borders of the lungs, scattered through which were very small grayish tubercular granulations. In the kidneys were many cysts filled with a transparent fluid. The other rabbit of the first pair which had not been inoculated was now killed, and, on examination, only perfectly healthy appearances were observed.

Pair No. 2. — Nov. 29, the inoculated rabbit was

killed. A very large number of granulations of miliary tubercles was found throughout both lungs, and especially under their pleural coverings. The other rabbit of this pair was killed at the same time, and was found entirely free from tubercle.

Pair No. 3. — Nov. 29, in both lungs of the inoculated rabbits were found red streaks clearly defined from the healthy lung tissue, and in which were seen a few small gray, nascent granulations under the pleural covering. Tuberculosis was evidently just beginning. Nothing abnormal was found in the other rabbit.

Nov. 27, the fourth full-grown rabbit, who had been inoculated only once, was killed. The entire surface of the lungs was riddled with granulations, the smallest of which were surrounded by congestive areolæ. Two or three lumps were seen as large as a small pea. The lung substance was also filled with tubercle, and also the surface of the spleen. The microscopic appearances of these granulations were those of real tubercle.

Villemin killed the rabbits to see if there were any tubercles, without waiting long enough for the production of the full symptoms of the disease — the emaciation, etc. — because he was anxious, as soon as possible, to establish the principle that tubercle is inoculable.

He inoculated most of the rabbits a second time, because he thought that possibly the first time might fail, as it often does in common vaccination, and he desired to eliminate this difficulty.

To still further increase the certainty of inoculation, he always used a mixture, broken up with the point of a knife, of tubercle in its different stages, — the gray, hard granulation, and the softening mass. To avoid the products of inflammation which tubercle is apt to cause around it in the lungs, he selected his material, when possible, from other parts of the body.

After the operation, for two or three days nothing was generally seen ; but on or about the third day came redness, heat, and swelling, which soon disappeared, leaving behind, however, a slight nodosity, which, after a while, appeared to suppurate and discharge. Soon the nodosity would reappear and discharge again, and so on. The first rabbit had a small ulcer near the wound, probably due to the introduction with the tubercle of some putrid material.

To prove that it was really the tubercle inoculated which produced the tubercle found after death, he substituted in other experiments various materials, such as pus, the discharges of cholera, etc., but after their use no tubercles were found. This seemed to prove that tuberculosis was indeed a specific disease ; that tubercle could be artificially produced only by tubercular matter.

His experiments in inoculating dogs and cats were not so successful ; but this comparative exemption seemed to him no more than what might be expected, owing to the very infrequent occurrence of the disease among these animals in their natural state.

Next, being desirous of ascertaining whether his inoculations would be equally successful, if he substi-

tuted the tuberculous material from animals, especially from the bovine species, for that from man, he introduced into a rabbit such substance from a phthisical cow. The rabbit soon became emaciated, and when killed, six weeks afterwards, its lungs, liver, kidneys, etc., contained tuberculous masses similar to those found in other victims of his other experiments. This convinced Villemin of the truth of the conclusions reached on other grounds by many other observers, that phthisis in the ox or cow is identical with that in man.

As a further and still more convincing proof that the disease in the animals inoculated was essentially the same as that which furnished the virus, he inoculated two rabbits with matter from one which he had artificially rendered tuberculous. One of these was killed in about six weeks, and the other died at the end of the second month, both showing tubercle in their lungs and other organs. Material from one of these seemed to retain all its activity, and to be as efficient as any in producing, in six weeks, intense tuberculization in another rabbit. This showed conclusively that artificial tuberculization produced by inoculation possessed the power, like that which arises spontaneously, of being transmitted.

On Oct. 30, 1866, Villemin presented a second memoir on the subject to the Academy, in order to answer certain objections which had been raised to his experiments. Among animals, the rabbit (as well as the ape, the cow, the guinea-pig, and perhaps a few others) is particularly susceptible to tuberculosis,

and it was urged that his cases were hardly a fair test on this account, and that the test, to be really satisfactory, ought to be made on animals which are seldom or never phthisical. To this Villemin replied that in spite of this acknowledged peculiar susceptibility in rabbits, it does not spontaneously affect any considerable proportion of them, and that the chances were altogether against the opinion that the rabbits used by him in his experiments were to any extent previously affected by the disease. His inoculations were in all twenty-two, only two of which were unsuccessful. In one of these two cases he attributed his failure to poor material, and in the other to an imperfect method of operating.

Another objection had been made that, on account of his using tubercular material taken from men who had been dead from twenty-four to thirty-six hours, his results might have come from the cadaveric substance simply, and were not, therefore, a proof of the specific nature of tubercle. To meet this objection, he had inoculated three young, healthy rabbits with warm tubercle taken from the lungs and kidneys of another rabbit which was killed on the spot. His former result, were confirmed in all these cases, in which the disease proceeded very rapidly, one of them being the most intense and rapid case he had ever seen. He knew not whether to attribute this intensity to the freshness of the matter employed, or to the fact that the organisms of the giver and receiver were more alike than in the former cases.

Villemin's experiments were repeated and confirmed

by the well-known physician M. Simon, who reported his results to the Pathological Society, March 19, 1867. Similar results were also obtained by Messrs. Herard, Cornil, and Genaudet, in Paris, and Lebert, in Germany, besides others of more or less celebrity in different parts of the world.

One of these experiments was very remarkable from the fact that the subject experimented on was a human being. Demet, Paraskeva, and Zallonis, in Syra, Greece, having succeeded in producing the disease in rabbits by inoculating them with sputa and blood from a man in consumption, argued that although the analogy between the effects produced in some animals and in man might be striking, yet the demonstration would necessarily be still more complete if they could operate on man himself. Under ordinary circumstances, of course, this would be out of the question, but in the following case they felt themselves justified in their course of procedure. A man who was suffering from gangrene of the big toe of the left foot, and for whom death was inevitable, in consequence of his refusal to submit to amputation, not only was shown, by a careful examination, to have perfectly healthy lungs, but there was abundant evidence that he had not the least tendency to tuberculosis. A quantity of sputa from a man in consumption was now injected into the upper part of the left thigh, whether with or without his consent we are not informed. In three weeks an examination of his lungs showed the beginnings of disease, and in thirty-eight days, when the man died from gangrene, seventeen tubercles were

found in the upper lobe of the right lung, and two in the left lung. They were in the first stage of development, gray and hard, most of them being as large as a grain of mustard-seed. Two were also found in the liver. This fact furnishes pretty conclusive evidence that the tubercles found were a direct result of the inoculation, as it is exceedingly improbable that a man of his age — fifty-five years — without the inoculation could have had so many in the first stage of development only.

Among those who accepted Villemin's views on the specific nature of tuberculosis, based on his experiments in the inoculation of tubercle, was Dr. Wm. Marcet, of London, who immediately conceived the idea of turning these facts to account in a very practical way. He argued that if there were any doubt as to the character of the disease in a person supposed to be in consumption, the doubt might be resolved by the simple process of inoculating a healthy rabbit or guinea-pig with some of the expectoration of the patient. If the inoculated animal became tuberculous, on account of the specificity of the disease it would follow that the patient was likewise tuberculous.

With this object in view, Dr. Marcet zealously went to work, and with the greatest care made nineteen experiments in inoculating healthy guinea-pigs with the expectoration of different patients. He used guinea-pigs instead of rabbits, because it was easier to obtain and more convenient to keep them. Of all the circumstances connected with these experiments he made careful records, and from these prepared a long and

elaborate paper on the subject, which he read, in 1867, before the Royal Medical and Chirurgical Society of London, which prides itself on being one of the most learned and scientific bodies in the world, and which, besides including most of the medical celebrities in London, enrolls in its list of "foreign honorary fellows" (limited to 20) such names as those of Andral, Cruveilhier, Donders, Langenbeck, Liebig, Louis, Rokitansky, Virchow, etc., of world-wide fame. We have not room here to make an abstract of his experiments, much less to give them in detail; but will content ourselves with reproducing the conclusions which he deduces from them, and which he appends to his paper: —

"*First.* The inoculation of guinea-pigs with the expectoration of patients suffering from tubercular phthisis will, at all events, in a certain stage of the disease, and possibly throughout, give rise to the formation of tubercle in the operated animals.

"*Second.* If two or more guinea-pigs inoculated with human expectoration brought up by coughing should die from tubercular disease, or should, on being killed at least thirty days after inoculation, exhibit tubercles in their bodies, this may be considered as a direct and positive evidence that the person whose expectorations were inoculated was suffering, at the time, from tubercular phthisis.

"*Third.* If two or more guinea-pigs be inoculated with the expectoration coughed up by a person considered to be in the third stage of phthisis, and if these animals do not die of tubercular disease, or exhibit

any tubercle when killed at least fifty-days after inoculation, it may be considered that in the present case the softening of tubercle and the secretions from the pulmonary cavities are arrested, the patient being in a fair way of recovery.

"*Fourth.* Besides the pulmonary expectoration, blood and pus taken from the human body in phthisis appear to be also possessed of the power of causing the formation of tubercles in guinea-pigs, when inoculated to these animals.

"*Fifth.* The spleen appears to be the first, and the lungs one of the last organs in guinea-pigs to be attacked by tubercular disease."

Among those who repeated Villemin's experiments was one of London's most prominent physicians, Dr. Andrew Clark.* In a number of cases he introduced a piece of fresh gray tubercle into a subcutaneous wound in the neck of a rabbit, and found at the spot in three days a hard swelling (which often went on to suppuration), and in two or three weeks gray granulations in the lungs, and afterwards in other parts of the body. He obtained similar results in other rabbits which he inoculated with material from the neck of one of these. These tubercles had all the naked eye characteristics of true gray granulations. They projected from the cut surface of the lung, were semi-transparent and tough, and existed in all parts of the lung indiscriminately. Still, he hardly liked to go so far as to say, with Villemin, that these were *identical* with tubercle in man, and that gray granulation

* Medical Times and Gazette, London, 1867.

was the specific local expression of a specific virus, capable, like that of syphilis, of being indefinitely propagated by inoculation. The gray granulation in the rabbit was found as often at the base as at the apex of the lung ; seldom in the areolar tissue about the bronchi or blood-vessels or in the pleura, and did not give rise to the secondary changes which cause death in man, since rabbits might often be kept in a fair state of health for a long time after inoculation. Besides, it was not always safe to trust to appearances as revealed to the naked eye, since some other little lumps, emboli, for example, might readily be mistaken for gray granulations ; and, as far as his observation went, the microscopic appearances of the so-called tubercle produced by inoculation in rabbits were not precisely like those found in man.* And even admitting, for the sake of argument, that they were structurally the same, it did not necessarily follow that their production was owing to the inoculation of a *specific* virus, for in the experiment the piece used would naturally decompose, and this decomposition would give rise to a local irritation, whence might follow the changes observed. In support of this latter opinion he cited two cases in which he had produced gray granulations by inoculation with *non-tubercular* matter. He had also tried the experiment of injecting cuttle-fish powder or sand into the veins of rabbits, subsequently finding in both lungs yellow or gray

*Virchow and others among the best pathologists, after the most careful examination, have since decided that this *is* really identical with the true tubercle in man.

spherical masses from the size of a mustard seed to that of a pea, and sometimes even secondary deposits in the liver and kidney. In the lungs of dogs, whose veins had been thus injected nine months before, yellowish-gray nodules were found *distantly* resembling gray granulations in man.

For these reasons, he considered that the subject was surrounded with so much doubt that it would be wise to reserve his opinion.

Experiments conducted by Dr. Clark's countrymen, Drs. Wilson Fox * and Burdon Sanderson † and others, pointed in the same direction. It was found that the inoculation of various substances might, under certain conditions, produce in rabbits this so-called tubercle ; and even the simple irritation of a seton, in one case, acted in the same way. In Fox's hands putrid muscle had this effect, in four out of five inoculations.

These experiments seemed to show that to produce tubercle in animals it was not necessary to use a *specific contagium*, but that the same object might be accomplished by a kind of blood poisoning occasioned by the introduction of certain putrid or other materials into the circulation.

Others among the numerous experimenters who followed Villemin's lead, but who, from one cause or another, reached different results, brought forward other objections, or tried to explain away his deduc-

* On the Artificial Production of Tubercle in the Lower Animals. London, 1868.

† Tenth Report of the Medical Officer of the Privy Council. London, 1868.

tions. To meet these, he delivered an address before the Imperial Academy of Medicine, in August, 1868, in which he earnestly maintained that the tuberculous matter inoculated really acted as a virulent element, and produced a true contamination. He demonstrated that it was absolutely impossible for the local nodosity which developed some days after inoculation to be merely the *remains* of the matter inoculated, as some had claimed, and for the tubercles found throughout the system to be there as a result simply of mechanical transportation through the lymphatics. He likewise showed the impossibility of explaining the transmission of tubercle by a process of grafting. What is of more particular interest to us, however, he explicitly pointed out the real differences between genuine tubercles and those *quasi* tubercles often mistaken for them, developed in man, and more particularly in animals, by *parasites*, although he admitted a close resemblance. He likewise discussed and differentiated the results of injection of sand, mercury, pus, and other irritant substances into the bronchial tubes and veins, showing how unlike they were to real tubercles, and comparing the mistake of confounding them to that of mistaking the pustules produced by tartar emetic with those of small-pox, or the redness of a rubbed skin with that of scarlet fever.

Prof. Parrot* thinks that he has discovered important differences in the local lesions produced by the inoculation of tubercular and non-tubercular material. In guinea-pigs which have received non-tubercular

* Mémoires de la Soc. de Biol., T. XXV. p. 16, 1873.

matter, the wound first swells and then suppurates, but it has invariably been found cicatrized at the time of death. On the other hand, where tubercle has been used, although the wound may at first cicatrize, sooner or later a large ulceration follows, perhaps fifteen millimetres in diameter, indurated, and with raised perpendicular edges. The induration extends all over the bottom of the ulcer, which is generally formed by a yellowish, cheese-like substance which extends by means of little hard, round threads, as it were, to the neighboring ganglions, which are much more implicated than in the other cases.

It is curious to read in this connection an experience met with over sixty years ago by the celebrated French physician, Laennec, to whose wonderfully successful efforts in bringing order out of chaos in the diagnosis of diseases of the lungs and heart by means of auscultation the world owes a far greater debt of gratitude than to any other man. In his *Traité de l'auscultation médiate et des maladies des poumons et du cœur*, 4th ed., Paris, 1837, p. 178, he says :—

"Can a direct inoculation cause the development, at least locally, of the tubercular material? I have only a single fact which bears on the point, and, although one fact proves very little, nevertheless I consider it of enough importance to be reported here.

"About twenty years ago, when examining some vertebræ in which were found tubercles, my saw slipped and slightly scraped the index finger of the left hand. At first I did not pay any attention to this scratch. On the next day a little erythema manifested

itself, and there gradually formed, almost without pain, a little tumor, which at the end of eight days had acquired the size of a large cherry-stone and seemed to be situated in the skin itself. At this time the epidermis which covered the tumor cracked, and I could perceive a little yellowish body, firm, and very like crude yellow tubercle. I cauterized it with deliquescent hydrochlorate of antimony (butter of antimony), experiencing almost no pain, and after a few moments, when the salt had penetrated the tumor, I detached it entire by means of gentle pressure. The action of the caustic had softened it so much as to render it very like soft tubercle, and of a friable consistency. The space which it had occupied formed a little cyst, whose walls were gray as pearl, slightly semi-transparent and without any redness. I cauterized them anew, the cicatrix formed promptly, and I have never since seen any result from this accident."

Among all the experiments performed on animals, those which, from the similarity of their conditions, bear the closest relation to the general subject of the contagion of consumption, were performed by Dr. Tappeiner, of Meran, and published in Virchow's *Archiv.*, 1878, T. XLVII. p. 393, and also in abstract in the *Lancet* of Nov. 23, 1878. He argued that if consumption is contagious, it must in all likelihood be conveyed by inhaling with the breath fine particles of tuberculous sputa atomized into the air by the patient's cough; and that this point could to a certain extent be established, if animals could be made tuberculous in consequence of the inhalation of such sputa,

artificially atomized. Therefore he caused animals to breathe for several hours every day the air of a chamber in which tuberculous expectoration mixed with water was converted into fine particles, and uniformly diffused by means of a common steam atomizer. The expectoration of persons in the last stage of consumption, in whose lungs cavities had formed, was used. Dogs alone were employed in the experiments, because in them tuberculosis is very seldom developed spontaneously; and the objection could not be offered, which had already been made against Villemin's experiments with rabbits, that many of them, being predisposed to the disease, would have had it at any rate.

Eleven dogs were put into the chamber, and after a test of from twenty-five to forty-five days were killed. In all of them (with one doubtful exception) were found well-marked miliary tubercles in both lungs, and in some cases also in the kidneys, liver, and spleen. The microscope in each case confirmed the gross appearances.

The quantity of expectoration used was very small. In three cases only fifteen grains, weighed in its ordinary moist state, was daily atomized in the chamber. Probably most of it reached the air-cells of the lungs and was thus absorbed, for powdered cinnabar made a decided red stain in the air-cells of a dog who had inhaled it from a steam atomizer for one hour, and who was killed twelve hours afterwards.

Contrary to what might have been expected, all the animals (but two) in which tubercles were afterward

found seemed lively, in good flesh, and perfectly well until they were killed, — a condition, it must be confessed, not much resembling tuberculosis in man. From this fact it was considered a rational deduction, however, that tubercles may, for quite a while, remain latent in man, until a "cold," or some other exciting cause, starts them up into activity; a supposition rendered exceedingly probable by the fact that when consumption is unmistakably handed down from parent to child, the disease does not usually develop in the latter until after puberty, — sometimes, indeed, considerably later.

Tappeiner's inhalation experiments seemed conclusive, — even more so than Villemin's, — until their force was somewhat weakened by others conducted by Dr. Max Schottelius, of Würzbourg (*Centralblatt*, 1878, p. 18), who substituted the sputa of bronchitis for that of tuberculosis (the experiments being alike in every other respect), and claimed to obtain precisely the same results. He afterwards used pulverized cheese, brain, and cinnabar, with varying success, the cinnabar being almost negative, cheese being much less effective than the sputa, and brain still less.

If, to guide us, we had only the experiments of Villemin, Tappeiner, and others who obtained similar results, everybody would be thoroughly convinced that phthisis is a specific disease; but we confess that these apparently successful experiments of Clark, Fox, Sanderson, Schottelius, and others, with *non-tubercular* matter, *if we can trust to the accuracy of their judgment, and believe that what they considered tubercle*

was really genuine tubercle, are great stumbling-blocks in the way of our belief in the specificity of tuberculosis. These stumbling-blocks can be removed only by denying that what *seemed* to these men tubercle was tubercle. This has been done by many; among them, only to mention one, by such an eminent man as Prof. Da Costa, of Philadelphia, in whose interesting article on "Tubercle," in the *Philadelphia Medical Times* of June 19, 1880, we find the sentence: "The production of tubercles from non-tubercular material, either by inoculation or by inhalation, is not proved. Inflammatory nodules arise, but they have not the structure of tubercular formations."

If, however, we cannot see our way clear to call in question the accuracy of observation of these men, and if we accept the experiments of both parties as reliable, and acknowledge that all the artificial products which seemed to them tubercular were real tubercles, all that we could at present safely assert would be that the exciting cause of the tubercular development in the lungs and other organs of the animals experimented on, resided neither in the tubercular nor in the non-tubercular character of the matter which was inoculated or inhaled, but in the cheesy products of the inflammatory process which this matter excited. Therefore, strictly speaking, the inoculability of tubercle would not be proved; but it would be proved that a caseous focus, under certain conditions, may be produced locally by direct non-specific irritation, and that from this focus, by means of absorption, tubercles may be scattered, more or less, throughout the body. If,

then, on this supposition the experiments of Villemin and his successors did not demonstrate what he expected at the outset, they at any rate supplied strong confirmatory evidence of the modern theory of self-infection with tubercle from caseous foci, and of the consequent belief that the miliary tubercle is in general only a secondary product, which belief, although not yet universally adopted, has, nevertheless, recently caused, in many quarters, a great revolution in the pathology of consumption. Buhl says (*Inflammation of the Lungs, Tuberculosis, and Consumption*, p. 118): "As the final, but at the same time most weighty argument to prove that the origin of tuberculosis lies in absorption and infection, I must again mention *the results of inoculation, proving, as they do, that tuberculosis can be conveyed to animals.* This result, obtained by experimentation, is one of the brightest triumphs which has been achieved by experimental pathology."

Of course, if we should rule out the possibly mistaken judgments as to the recognition of tubercle lately spoken of, and consequently feel convinced by these experiments that tuberculosis is a specific disease, that it can be artificially produced in animals by the inoculation of tubercle alone and no other material, it would go very far towards furnishing a perfect demonstration of the contagiousness of consumption. But still this latter question does not necessarily depend on the specificity of the disease. To establish it, it is not necessary to describe precisely the character of the contagious principle; it is not necessary to

exhibit under the microscope any specific microzyme, but merely to show that septic material of *any* character is eliminated from the body or lungs of a consumptive person, which, transferred by direct contact or floating in the air in a state of minute subdivision, and absorbed into the system of a healthy person, may produce the same disease. The real question which concerns us is, *Can* one person contract the disease from another who has it? not, Is this the *only* possible way to contract the disease?

Although some may think that it militates against one of the postulates of the "germ theory" if pushed to an extreme, yet it seems to us perfectly proper to argue about consumption as Prof. Leishman, of Glasgow, argues in his classic work on Midwifery (p. 677) about puerperal fever: —

"Is puerperal fever contagious? Some confusion may here arise as to the meaning of the word contagion. 'By a contagious disease' says Schroeder, 'is meant one in which a specific poison is produced within a diseased organism, and which, transferred to other individuals, always produces the same specific disease, such as measles, scarlatina, small-pox, etc.' It will at once be noticed that if we concede that puerperal fever is not due to a specific poison, and at the same time admit the accuracy of Schroeder's definition of the word, we necessarily come to the conclusion that puerperal fever is not a contagious disease. Such hair-splitting in regard to the meaning of words tends more frequently to confusion than to precision; and preferring, as we do, the word 'contagious' to

'communicable,' we employ the former in its broader and more colloquial signification. In reply, then, we hesitate not for a moment to say that puerperal fever is contagious."

He then goes on to bring forward or point out irresistible proofs of the contagious properties of the disease.

Since writing the above, we have been fortunate enough to obtain a pamphlet* containing the latest views on this subject of one of the greatest pathologists, if not the greatest, in the world, — Prof. Cohnheim, — which, if fully confirmed by others, will clear up a great deal of the mystery which we must acknowledge has hitherto befogged our ideas on the subject of inoculation.

Cohnheim's views are all the more important because, when Villemin first promulgated his theory, he was one of its most vigorous and influential opposers, and he has been convinced against his will, by a long series of carefully conducted experiments. He finds that it does not make much difference how the tubercular matter is introduced into the system, whether under the skin, in the pleural or peritoneal cavities, or in other ways, either alone or mixed, provided that it is not decomposed, so that no septic influence will be present. From his experiments, conducted in conjunction with Salomonsen, he decides that the process may best be seen when the matter is introduced into the anterior chamber of the eye of the rabbit. Hav-

* Die Tuberkulose vom Standpunkte der Infectionslehre, vom Julius Cohnheim, Leipzig, 1880.

ing made a linear incision into the cornea, he introduces through it a minute fragment of tuberculous matter, which fragment, if fresh, so that no great amount of irritation is caused, gradually becomes smaller and smaller, and may even entirely disappear. After an incubation period of about three weeks in rabbits, or two weeks in guinea-pigs, an eruption appears upon the iris of minute gray nodules, " which increase to a certain size, and then undergo caseous degeneration, to be followed in turn, in the course of months, by a more or less general tuberculosis of the lungs, peritoneum, and various other organs." This general tuberculosis he found to occur *invariably* when genuine tubercular matter had been inoculated, and he utterly failed to excite tubercles in the iris, by introducing all kinds of *non-tubercular* animal tissues. He thinks that what certain other experimenters had called " tubercle," resulting from the inoculation of non-tubercular material, was not genuine tubercle at all, although it might often resemble it. So thoroughly convinced did he become of the specificity of tubercle, that, like Marcet, of whom we have already spoken, he decided that inoculation may be employed as a *test* to ascertain the character of any pathological product, and that we cannot, with absolute certainty, distinguish tuberculous material in any other way. " Neither the tubercular form, color, consistency, the histological characters, the occurrence of giant cells, caseation, nor all these circumstances together, are absolutely characteristic of tubercle. *The only absolutely perfect and certain criterion is the capacity of infection by*

inoculation." Microscopic tests for tubercle have been proved not to have the value previously thought. This test of inoculation, when applied to human miliary tubercle, the products of caseous pneumonia and cheesy scrofulous lymphatic glands, as well as to the fungating tissue of strumous joints, shows that they are all manifestations of one and the same virus, and old Laennec's doctrine seems to be confirmed. The leading principle here, as in all infecting maladies which produce local changes, is that a tubercular or scrofulous product is generated whenever tubercular virus is present and remains for a certain length of time. The chief factor, therefore, in determining the localization of the disease, is the mode of its introduction into the system, spreading in which, it follows the natural roads. If introduced into the peritoneal cavity, it first produces tuberculosis of the peritoneum, spleen, and liver ; if introduced into the eyeball, the iris is first affected ; if under the skin, the nearest lymphatic glands ; if inhaled, the lungs and bronchial glands ; if swallowed, the intestines and mesenteric glands.

Cohnheim thinks that in adults the poison is generally inhaled, because the lungs are oftener affected than any other organs. Why in some cases general tuberculosis and in others caseating pneumonia is thus produced, we do not yet know. When the tubercular masses in the lungs ulcerate, some of the infecting matter, leaving the lungs, reaches the trachea and larynx, and tubercular deposit may in consequence there result. Likewise with the pharynx, soft palate, root of tongue, and tonsils. Some is swallowed and

infects the alimentary canal, in the different parts of which, as we should naturally expect, infection is found in varying amounts, in proportion to the length of time that the intestinal contents have been retained in them ; the favorite seats being the neighborhood of the ileo-cæcal valve, the lower part of the ileum, cæcum and ascending colon, the upper and lower extremities of the tube being less exposed. Soon, by their proximity, the mesenteric glands, and possibly liver, become infected, and the peritoneum, from deep ulceration of the intestines.

Sometimes, when inhaled, instead of or before infecting the lungs, the virus may be directly absorbed in the first instance by the larynx and trachea, producing primary laryngeal phthisis.

If the poison is not inhaled, but is swallowed with the food, the disease is apt to start first from the digestive canal. This is the probable explanation of the reason why young children are more apt to have consumption of the bowels than of the lungs, they being to so great an extent fed on cows' milk, which, if it come from phthisical cows, as we have shown in Chapter VI., is so fraught with danger to our little ones. Certain it is that in young children advanced disease of the mesenteric glands and peritoneum, without any (or but slight) disease of the lungs, is very common.

Cohnheim concludes that the nature of tuberculosis can best be described by comparing it with syphilis, between which and tuberculosis he discovers at all points the closest analogies. "Everything depends upon the virus," he says ; both diseases require above

all things infection — transmission from person to person. Each must have its specific virus, which must reach a mucous membrane or a broken surface, to be absorbed and induce the disease. Each manifests itself at first by a local lesion, from which it spreads and becomes general. Each is transmitted by heredity; and, thus transmitted, may lie latent for a longer or shorter time. It is from the influence of the poison during this "latent" period in tuberculosis that the phthisical "constitution," "predisposition," or "habit of body" arises, which has nothing to do with a mere *facility* for receiving the virus, as heretofore supposed. Such individuals are not *predisposed* to tuberculosis, they *already have it*, either inherited or early acquired. Every person is tubercular in whose body the tubercular virus has taken root. There is no more predisposition for tuberculosis than for syphilis, although some persons are more easily affected than others; nor is there any constitutional predisposition which causes inflammatory products to undergo the cheesy degeneration in some persons instead of being absorbed, but only those products undergo this degeneration which are already the products of tubercular virus.

Both tuberculosis and syphilis may disappear from the body entirely, and a perfect cure may result; but with either it is not always possible to feel sure that, even if it is apparently cured, it may not return; and, on the other hand, sometimes the patient may never get it removed entirely from his system.

Again, while both diseases *may be* inherited, it is also true that both diseases, in the vast majority of cases, are *not* inherited, but acquired.

Thus Cohnheim commits himself most decidedly to the doctrine of contagion. Standing, as he does, in the very front rank of the medical profession throughout the world, his opinions are already exciting a very deep interest. If they are destined to be confirmed and adopted by others as eminent, we can have no stronger argument to rely upon to prove the contagiousness of consumption. But it is not necessary that they should be fully confirmed in every particular to establish the extreme probability of this doctrine. There may be a flaw here and there, and yet the main structure be sound. The London *Lancet*, in commenting on his pamphlet, says: "But it must at the same time be remembered, that because a theory is not entirely true it is not therefore entirely false. Tuberculosis may be communicable, and yet not always or often due to the reception of a poison from without. Many clinical facts suggest that phthisis may sometimes be contracted by one individual from another. The theory of Cohnheim, if partially true, may throw new light on such facts, while his experimental results are of such importance as to demand the attention of pathologists, and the test of renewed experiment."

The inoculation experiments of Cohnheim and others have excited such an intense interest in the Old World, that many are now repeating them, mostly, we may add, so far as we have seen, confirming Villemin's and Cohnheim's results. For instance, we find the following in the *Philadelphia Medical Times* for May 8, 1880, translated from *Archiv. f. exp. Path.*: —

"F. A. Rheinstadtler cultivated tubercular matter

in Bergmann's solution, and injected it into the tracheas and lungs of dogs and rabbits. In other cases tubercular sputa and small bits of scrofulous glands were introduced into the trachea. Meantime, another animal had meal broth thrown into the trachea as a control experiment. While in the latter case only inflammatory changes could be found, subsequently the animals injected with tubercular matter showed in every instance tuberculosis of the lungs, the other organs, especially the liver, being also occcasionally the seat of tubercular infiltration."

In the *Journal de Méd. et de Chir.* for May, 1880, Paris, we read of certain experiments by M. Toussaint, of the Veterinary College at Toulouse, on pigs, which very rarely have tuberculosis. He sometimes injected the fluid scraped from the cut surface of tubercular nodules, and sometimes the blood of a tuberculous pig. In all cases tubercle was reproduced, acute tuberculosis set in, and the animals experimented on died in a very short time. In one case he inoculated two cubic centimetres of meat juice pressed with a hydraulic press out of a fragment of the muscle of a tuberculous cow, into a pig five months old, and killed the pig sixty-seven days afterwards. A jar, containing parts of the lung, liver, spleen, and bronchial and submaxillary glands, all showing tubercular lesions in an advanced stage, was submitted to the academy for examination by M. Bouley.

The names of other experimenters who have obtained results similar to those of Cohnheim, are Harnsell, Perls, Manfredi, Koester, Leber, Samelsohn, Sattler, and Angelucci.

APPENDIX.

DR. KOCH'S GREAT DISCOVERY OF THE BACILLUS OF TUBERCULOSIS.

A Letter, Descriptive and Highly Eulogistic, from Professor Tyndall to the London Times of April 22, 1882.

TO THE EDITOR OF THE TIMES:

Sir, — On the 24th of March, 1882, an address of very serious public import was delivered by Dr. Koch before the Physiological Society of Berlin. It touches a question in which we are all at present interested, — that of experimental physiology; and I may therefore be permitted to give some account of it in the *Times*. The address, a copy of which has been courteously sent to me by its author, is entitled "The Etiology of Tubercular Disease." Koch first made himself known by the penetration, skill, and thoroughness of his researches on the contagium of splenic fever. By a process of inoculation and infection he traced this terrible parasite through all its stages of development and through its various modes of action. This masterly investigation caused the young physician to be transferred from a modest country practice, in the neighborhood of Breslau, to the post of Government Adviser in the Imperial Health Department of Berlin.

From this department has lately issued a most important series of investigations on the etiology of infective disorders. Koch's last inquiry deals with a disease which, in point of mortality

stands at the head of them all. If, he says, the seriousness of a malady be measured by the number of its victims, then the most dreaded pests which have hitherto ravaged the world — plague and cholera included — must stand far behind the one now under consideration. Koch makes the startling statement that one seventh of the deaths of the human race are due to tubercular disease, while fully one third of those who die in active middle age are carried off by the same cause. Prior to Koch it had been placed beyond doubt that the disease was *communicable;* and the aim of the Berlin physician has been to determine the precise character of the contagium, which previous experiments on inoculation and inhalation had proved to be capable of indefinite transfer and reproduction. He subjected the diseased organs of a great number of men and animals to microscopic examination, and found, in all cases, the tubercles infested with a minute, rod-shaped parasite, which, by means of a special dye (methylene violet and resuvin) he differentiated from the surrounding tissue. It was, he says, in the highest degree impressive to observe in the centre of the tubercle cell the minute organism which had created it. Transferring directly, by inoculation, the tuberculous matter from diseased animals to healthy ones, he in every instance reproduced the disease. To meet the objection that it was not the parasite itself, but some virus in which it was imbedded in the diseased organ, that was the real contagium, he cultivated his bacilli artificially, for long periods of time and through many successive generations. With a speck of matter, for example, from a tuberculous human lung, he infected a substance (blood-serum-gelatine) prepared, after much trial, by himself, with the view of affording nutriment to the parasite. Here he permitted it to grow and multiply. From this new generation he took a minute sample and infected therewith fresh nutritive matter, thus producing another brood. Generation after generation of bacilli were developed in this way, without the intervention of disease. At the end of the process, which sometimes embraced successive cultivations extending over half a year, the purified bacilli were introduced into the circulation of healthy animals of various kinds. In every case inoculation

was followed by the reproduction and spread of the parasite, and the generation of the original disease.

Permit me to give a further, though still brief and sketchy account of Koch's experiments. Of six guinea-pigs, all in good health, four were inoculated with bacilli derived originally from a human lung which in fifty-four days had produced five successive generations. Two of the six animals were not infected. In every one of the infected cases the guinea-pigs sickened and lost flesh. After thirty-two days one of them died, and after thirty-five days the remaining five were killed and examined. In the guinea-pig that died, and in the three remaining infected ones, strongly pronounced tubercular disease had set in. Spleen, liver, and lungs were found filled with tubercles; while in the two uninfected animals no trace of the disease was observed. In a second experiment, six out of eight guinea-pigs were inoculated with cultivated bacilli, derived originally from the tuberculous lung of a monkey, bred and rebred for ninety-five days, until eight generations had been produced. Every one of these animals was attacked, while the two uninfected guinea pigs remained perfectly healthy. Similar experiments were made with cats, rabbits, rats, mice, and other animals; and without exception it was found that the injection of the parasite into the animal system was followed by decided, and in most cases virulent tubercular disease.

In the cases thus far mentioned inoculation had been effected in the abdomen. The place of inoculation was afterwards changed to the aqueous humor of the eye. Three rabbits received each a speck of bacillus culture, derived originally from a diseased human lung. Eighty-nine days had been devoted to the culture of the organism. The infected rabbits rapidly lost flesh, and after twenty-five days were killed and examined. The lungs of every one of them were found charged with tubercles. Of three other rabbits, one received an injection of pure blood serum in the aqueous humor of the eye, while the other two were infected in a similar way with the same serum, containing bacilli derived originally from a diseased lung, and subjected to ninety-one days' cultivation. After twenty-eight days the rab-

bits were killed. The one which had received an injection of pure serum was found perfectly healthy, while the lungs of the two others were found overspread with tubercles.

Other experiments are recorded in this admirable essay, from which the weightiest practical conclusions may be drawn. Koch determines the limits of temperature between which the tubercle bacillus can develop and multiply. The minimum temperature he finds to be 86° Fahrenheit, and the maximum 104°. He concludes that, unlike the bacillus anthracis of splenic fever, which can flourish freely outside the animal body, in the temperate zone animal warmth is necessary for the propagation of the newly discovered organism. In a vast number of cases Koch has examined the matter expectorated from the lungs of persons affected with phthisis, and found in it swarms of bacilli; while in matter expectorated from the lungs of persons not thus afflicted he has never found the organism. The expectorated matter in the former cases was highly infective, nor did drying destroy its virulence. Guinea-pigs infected with expectorated matter which had been kept dry for two, four, and eight weeks respectively were smitten with tubercular disease quite as virulent as that produced by fresh expectoration. Koch points to the grave danger of inhaling air in which particles of the dried sputa of consumptive patients mingle with dust of other kinds.

It would be mere impertinence on my part to draw the obvious moral from these experiments. In no other conceivable way than that pursued by Koch could the true character of the most destructive malady by which humanity is now assailed be determined. And however noisy the fanaticism of the moment may be, the common-sense of Englishmen will not, in the long run, permit it to enact cruelty in the name of tenderness, or to debar us from the light and leading of such investigations as that which is here so imperfectly described.

Your obedient servant,

JOHN TYNDALL.

HIND HEAD, April 20, 1882.

Dr. Koch's researches are entitled to all the more consideration from the fact that he has the reputation of being an eminently cautious man, who would be very unlikely to give to the world such statements as he has lately made without being quite sure of their verification. He has more than once, with proper conservatism, held back a too rashly enthusiastic experimenter from a premature publication of his results. Undoubtedly his processes will soon be repeated by an army of investigators. Indeed, an important verification of a part of his work has already been reported, and almost in the same breath. Another man has arrived at practically the same results, independently, by a different process. Koch's paper was printed March 30. On April 3, Prof. Baumgarten, of Königsburg, published a paper in the *Centralblatt f. d. Med. Wissen.*, in which he claimed that after two years of fruitless study he had at last succeeded in detecting innumerable masses of genuine rod bacteria in the tubercular nodules produced in animals by the inoculation of tuberculous matter, as well as in human tuberculosis. He had two years previously suggested (in print) that a parasite was the cause of tuberculosis, but had not been able actually to verify his suggestion by microscopic inspection until two weeks before the publication of Koch's paper. His description of the organisms seen is not absolutely identical with Koch's, but the two accounts are so nearly alike that the same objects must be referred to; and the independence of the methods employed and the similarity of the results attained but serve to

inspire us with all the greater confidence in the remarkable observations of both men.

The London *Lancet* of April 22 and 29 has two long editorials devoted to a careful description of Koch's researches. The second ends as follows: —

"Koch hesitates to follow into the region of hypothesis the considerations which are suggested by the hereditary tendency which manifestly plays so important a part in the causation of tubercular disease, but he points out in this relation that the growth of the tubercle bacillus is slow. This makes it highly probable that the tubercle bacilli cannot by their successive development infect the whole body from a small wound, as do the organisms of anthrax. To render an animal tubercular with certainty the infectious material must be inserted in the subcutaneous tissue, or in the peritoneal cavity, or in the anterior chamber of the eye, etc.; *i. e.*, in a locality in which the bacilli may gain a firm footing for their development. Infection from the surface of a wound or from the cornea is very exceptional. The bacilli are eliminated before they can get into the system. This consideration is adduced as the reason why persons are not rendered tubercular by making post-mortem examinations on the bodies of those who have died from phthisis, even when the tubercular material comes in contact with small incised wounds on the hands. Similar considerations apply to the problem of infection by the lungs. It is probable that the bacilli do not get into the system even when inhaled, unless they can develop in stagnant secretion, or unless the loss of epithelium facilitates their ingress; otherwise it is difficult to understand the low degree of infectiousness of phthisis.

"These admirable researches of Koch and the important facts he has discovered can scarcely fail, however, to bring to the reader's mind the paramount distinction which must always be drawn between the value of the facts ascertained by experiments and that of the more or less hypothetical application of those facts to the far more complex problems of disease. . . . The

aptitude or inaptitude of the soil may be as specific and as important as the organism which grows in it. These experiments are certain to be prolific of a vast increase in our knowledge, and let us hope, in our power also, and the numerous questions they suggest will doubtless soon engage the attention of experimental inquirers."

The *British Medical Journal*, the organ of the national medical society of Great Britain, says, editorially, April 29, 1882 : —

"For several years observers have been making experiments on the relation of bacteria and infectious diseases, but in only a very few instances have definite and positive results been obtained. This failure is now shown to have been due to the use of imperfect methods of observation and research, and it was not till the appearance of the first writings of Dr. Koch that a definite basis for such observation was obtained. Dr. Koch has of late done some striking work, and within the last few weeks has published some remarkable experiments on tuberculosis. He is a worker on whose observations and accuracy the most implicit reliance can be placed, and those who have had the pleasure of seeing him at work will hesitate before they find fault with his statements."

Note to the first paragraph on page 165.

THE TUBERCULOSIS DUE TO THE INOCULATION OF INDIFFERENT SUBSTANCES. — In a communication to the *Société de Biologie*, in 1881, M. Martin claimed that the histological structure of tubercle nodules was devoid of special significance, since inoculation with indifferent foreign bodies produces the same *anatomical* lesions as those obtained with tuberculous matter. The pseudo-tubercular eruptions thus produced in no way differ in their anatomical arrangement from the bodies held to be characteristic of genuine tuberculosis.

M. Martin then sought to determine whether or not the tuber-

cles produced by the inoculation of tubercular matter differed in other respects from those following ordinary inoculations. He found that the tubercles of tuberculosis possessed the property of *infection*, whereas the pseudo-tubercles lacked this quality. (*Gaz. Med. de Paris*, Jan. 22, 1881.)

It was ascertained that in a series of consecutive inoculations on different animals the infective properties of the original tuberculous matter became more and more active. In this respect it resembles the virus of septic processes, which Davaine has ascertained to acquire additional virulence with every repetition of inoculation on a new animal. The tubercles of indifferent substances acted in an entirely different manner. No matter how much of such material was employed, the lesions produced were merely local in extent. This was invariably observed. In no case did any tendency to generalization manifest itself, and not even the neighboring vessels and lymph glands became inflamed or showed tubercular changes.

The non-infectious nature of pseudo-tubercles was best shown by attempting serial inoculations. The second animal subjected to inoculation from the pseudo-tubercular matter of the first showed scarcely a local lesion, and the third showed no effects at all. It appears, therefore, that in addition to the true or infecting tubercle there exists a second kind of granulum anatomically identical with the former, but totally devoid of all specific properties.

Certainly this is another important contribution to our knowledge of general pathology, and it seems to corroborate Cohnheim's previous assertions concerning the infectious qualities of tuberculosis. Martin is apparently ignorant of the work of Cohnheim, but this makes his results and conclusions all the more valuable, because they were evidently obtained independently of the German pathologist.

Note to the middle paragraph on page 168.

TAPPEINER (Virchow's Archiv. 82; 353) has since repeated his experiments in the Pathological Institute in Berlin, with

positive results. Where he had used phthisical sputa, Prof. Virchow and others confirmed his diagnosis of tubercles produced; but where he had used sputa from chronic bronchitis, the results were negative. He severely criticises the work of Schottelius, claiming that he erred in using for inhalation such *large doses;* for in this manner the substances all acted merely as foreign or putrid bodies. He himself, on the other hand, had used such small quantities that no local effects were produced at the time, and the eruption of miliary granulations took place only after a period of incubation, thus showing the specific character of the disease.

ERRATA.

Page 15, line 14, for "imply" read "apply"; page 37, line 24, for "specifie" read "specific"; page 66, line 30, for "smptoms" read "symptoms."

IS CONSUMPTION CONTAGIOUS?

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