ON THE 'UNITY OF POISON

IN SCARLET, TYPHOID, AND PUERPERAL FEVERS; ERY-
SIPELAS, DIPHTHERIA, SORE THROATS; CERTAIN
FORMS OF DIARRHŒA, AND ALLIED AFFECTIONS;
PLEURISY, PNEUMONIA, PLEURO-PNEUMONIA;
AND MANY OTHER AILMENTS USUALLY CON-
SIDERED TO BE SEPARATE AND ENTIRELY
DISTINCT DISEASES.

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"If a man be content to begin with doubts he shall end with cer-
tainities."—BACON.

"All scientific novelties are subject to inconsiderate criticism."—
PASTEUR.

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From the facts connected with these ² affections—clinical and pathological—embracing as they do, the etiology, the history of origin, progress,³ interchange, and inter-communicability, mode of propagation, hybridity, and transmutation, we cannot avoid arriving at the conclusion that these ailments may be generated *de novo*, and from one common source, as well as by direct communion with one already suffering; or by indirect, through some secretions, excretions, or some thing, or person, that has been in contact, or communication with the sick.

That there are two forms of scarlatina is distinctly manifest—a fact thoroughly established on evidence from which there is no escape. There is—1st. That contracted from a scarlatina patient, or from some one or thing that has been in contact with him, or in communication with him. To this I give the name orthodox scarlatina. There is—2nd. That generated *de novo* from blood poisoning, such as occurs in puerperal patients, and in surgical cases (the "surgical scarlatina" of Sir James Paget) from deleterious matters absorbed or otherwise passed into the blood, and thence into the tissues of the body more or less generally; or from noxious drains, sewers, imbibition, or ingestion of pernicious articles of drink or food—such as tainted water, milk, cream, decomposing animal or vegetable substances. To this form I have given the name toxæmic, to distinguish it—as regards origin—from the orthodox. As an example of what I mean I shall give the following particulars:—

At one of the meetings of the Harveian Society the President brought forward a case which he termed

¹By unity is meant not that the poison is always the same, but that the one poison—the one *origo mali*—whatever it may be, will originate several so-called different affections.

²I had at first placed at the head of my paper only the first four diseases (keeping to the conventional word).

³"Erysipelas is a specific disease, due to the presence of some blood poison that has probably been introduced into the body from without; it is intimately allied with other blood poisons, such as are found in scarlet or puerperal fever, or septicæmia in any of its forms: for their poisons seem to be convertible."—Bryant's *Surgery*, second edition, vol. i, p. 44.

"simulative scarlatina from eating tomatoes," in which occurred all the symptoms of scarlatina, except that there was no desquamation; and for albumen in the urine no search was made.

In passing, however, I may observe again—I have made the same remark elsewhere—that desquamation is no proof whatever of the existence of orthodox scarlatina, and its absence no disproof of orthodox scarlatina having attacked a patient; for, high temperature of the skin, arising from any causes—and they are various, setting scarlatina aside altogether—will occasion such rapid cell changes, that the surface cells will be thrown off in larger or smaller quantities, and, according to the height to which the temperature has run, will they be shed wholesale, being effete. Thus arises that process which we call desquamation. In some undoubted cases of scarlet fever there may be only slight exaltation of temperature, or so transiently may it take place that it escapes detection; or it may even be altogether absent, owing to the great and sudden depression of vitality produced by the virulent character of the virus, or by the largeness of the dose taken into the system, or the suddenness of its invasion. With this absence of high temperature desquamation will also be absent. Nor is albumen in the urine a pathognomonic sign of scarlet fever poison pervading the system. In how many conditions and affections of the body other than those demonstrating the existence of scarlatina will not albumen be found? "There is nothing unusual in the fact that inflammation of an important surface should be attended with albuminuria," says Mr. Jonathan Hutchinson. (See *Brit. Med. Journal*, May 3, 1879, page 665, under head of Discussion at Medico-Chir. Society on Report of the Committee on Croup and Diphtheria.)

Here—in the person of the patient brought before us—was undoubtedly a case of "toxæmic scarlet fever," symptoms generated, as far as could be traced, by the eating of tomatoes. No source of contagion or infection having been discovered or being discoverable, no other source of origin, these fruits were apparently the *origo mali* of certain chemico-physiological changes in the system, which were sufficient to produce a general poisonous effect that demonstrated itself in certain physical changes, phenomena or symptoms—the majority of which, or the entire in the aggregate, being such as to lead the observer to say the patient had got scarlatina.

Bearing upon this case, I would adduce an instance wherein the symptoms of scarlatina were all so marked that it is specially applicable, and fully corroborates the President's in showing that ingested materials will at times induce symptoms so closely resembling orthodox scarlatina as to be undistinguishable. And here I would ask, if distinguishable, how, then, does the attack differ from orthodox scarlatina, *i. e.*, that which has been contracted from a scarlatina patient, or some one or thing that has been in communication with him? *The difference lies only in origin!* In my case the symptoms rose from eating very high game, and though not proceeding from eating tomatoes, it is relevant—1st. Because in all points it

was one of "simulative scarlatina"—a name I was the first to use some years back, but for which I now prefer to use the term "toxæmic scarlatina;" and. Because it arose from a similar, though not the same cause, ingested material; and thus, thirdly, was an answer to the query propounded,—Were the symptoms in the instance before us due to eating one of the solanaceæ order?—inasmuch as it shows that causes other than *the active¹ principle of that order* produce such symptoms, even while admitting that the active principle of the tomatoes may *per se* at any time, under any circumstances at all events, set up toxæmic scarlatina, and may in this particular patient have directly produced the symptoms and been the direct exciting source of the illness.

A second case which I would adduce bears strongly on the same; so strongly, indeed, that though having published it in 1875, I will quote it, as it also is to the point:—

1st. Because in symptoms it was identical; 2d. Was what would be termed "simulative (toxæmic) scarlatina;" and 3d. Showed that these symptoms may arise even from *animal* poisons passing into the system *in a way other than by the mouth*;—viz.: by the womb, in contradistinction, though not in opposition, to the tomatoes case.

Thus again answering the question directly by showing that the "simulative (toxæmic) scarlatina" symptoms are not *necessarily* dependent upon the ingestion of solanine, since they will be produced by animal irritants and poisons absorbed not through and from the mouth and stomach alone, but in other ways also.

I will here relate the case—a typical one, and such as has come before every medical man.

The conditions alluded to, and which are identical with scarlatina—are the same I now consider—have their origin not from orthodox scarlet fever contagium, that is, from a scarlatina-infected person, or 'some one or thing belonging to such a patient—there never having been any exposure to scarlatina in that way—but from the putrescing and offensive matter in utero and vagina, the resulting pyrexial action being indicative of the concealed cause, the latent mischief, that, without careful observation, might pass unobserved.

With the train of symptoms to which I refer, 'who practising obstetrics is not acquainted? The chilliness and shivering, the sickness, the dreadful headache, and the congested eyeballs from which the paining light must be veiled, or even shut off altogether; the sore throat, crimson with inflammation; the peculiar tongue; the hot, pungent skin, that so rapidly sends up the mercury, and is *dyed with the unmistakable rash*, with which there is a tinge of jaundice-yellow, here and there more noticeable; the delirium; the oppressed heart and breathing; and later on the diarrhœa. What are these symptoms to be termed? Not *simulative* scarlatina, surely, since they are not simulative at all, but are exactly the same as obtain in scarlatina contracted from a previously infected person or source—orthodox? What, then, are they?

Symptoms of what, for contradistinction's sake, I term "toxæmic scarlet fever," but assuredly as true scarlatina as the other form—the orthodox—though not generated in the same way, but generated *de novo* in the woman herself, and the outcome of the condition of blood poisoning existing, *not* from the introit of a *specific virus*, but of elements given off in utero or vagina—not, it may be, noxious when given off, but becoming so subsequently from morbid changes—and, when noxious, taken into the system; a blood-poisoning, I repeat, the result solely of the putrefying animal matter in utero or vagina, and of unhealthy changes, taking place locally first, in the generative tract and system, setting up local mischief primarily, and then developing and evolving through the entire body of the puerperæ, till all the symptoms I have portrayed have made their appearance. In a scarlet-fever epidemic, such a group of symptoms happening to the lying-in would be set down to scarlet-fever poison, contracted from another affected with it, or from some one or thing belonging to, or that has been in contact with, such an infected person; for it is not yet generally accepted that scarlatina can commence *de novo* in the manner I describe. But these symptoms will be found where no previous case, or contagium, or source of origin other than what I name, has existed.

What, then, are those phenomena? I gave them the name of bastard or simulative scarlatina when I first wrote on this subject, not at that time considering them true scarlet fever; or knowing that the ailment could be so produced; or, indeed, be produced *de novo* in the ways that further researches have shown me it can. True—orthodox—scarlatina I consider to be that form contracted from a previously existing case, or from some one or thing connected with, or which has been in some way or another brought under and near the influence of the scarlatina virus existing in a previously infected patient. Bastard, or simulative scarlatina—toxæmic—I consider to be those scarlatina manifestations or appearances heretofore deemed mere resemblances—a view I also then took, but which I now know to be not resemblances, but the actual manifestations themselves of toxæmia set up otherwise than from orthodox scarlatina, differing only from the orthodox form in that they originate *de novo* from such sources of contamination as may occur in the lying-in; or from vitiated water, milk, cream, or the taking in of "high," *i.e.*, putrescing articles of diet,¹ or decomposing drinks, such even as milk which has begun to undergo deterioration, it may be simply because it has been long "drawn;"² or it may be because it

¹ Upon this Dr. Carpenter wrote very clearly in 1853, in the *Brit. and For. Medico-Chirurg. Review*. See his article, "Predisposing Causes of Epidemics."

² The following remarks will explain what I mean when I say that even milk undergoing deterioration will produce scarlet fever:—On Thursday, January 30, 1879, at the meeting of the Metropolitan Counties Branch of the British Medical Association, Mr. Gay, in a paper on "Milk as Food for Man," said:—"In the absence of any positive knowledge as to the source of the various exanthemata—measles, scarlatina and the like, characterized by fever, and without doubt related to some form or forms of blood poisoning, disorders that abounded mostly after dentition, and when milk was often the predominating, if not the sole article of food, was it unreasonable that we should turn with suspicion to those *unwholesome ingredients of decaying milk* for their explanation?" He continues—"Milk when once exposed to the air and rest, underwent a series of chemical changes through the influence of external and internal agencies which

¹ Belladonna, it is well known, produces scarlatinous efflorescence, etc., etc.

has been allowed "to stand" in a place, and under circumstances of temperature,¹ etc., which would permit or hasten unwholesome changes,² the milk being at the time not unpleasant to the ordinary taste, nor even at all perhaps perceptibly altered in flavor.³ I now recognize these symptoms, arising from causes other than the infection or contagion derived from a previously infected person or thing, to be as true scarlatina, and capable of becoming in its evolutions and its transmigrations from one to another, as infectious and contagious as the other form. In confirmation of this view I would name that Sir Wm. Gull has stated his belief that diphtheria may "*commence locally, forming a poison which will propagate itself;*" and he has "drawn a distinction between diphtheria and diphtheria poison, regarding the disease as beginning with a poison, but *sometimes as commencing locally, and forming a poison which may propagate itself.*"—(*British Med. Journal*, May 3, 1879. Page 666.) This bears me out in the contagious and infectious character of toxæmic scarlatina; and the evidence of Burdon Saunderson, which I have quoted in my former paper, to the effect that an inflammation may begin as "simple" but go on progressively to what would be termed specific, also is corroborative. Again, we know that a benign may be converted into a malignant ulceration, progressively, under continued irritation.

And it will be found that scarlatina generated *de novo* from blood poisoning, or effected in any other way than that *usually accepted as being the only way* in which scarlatina is generated and propagated—viz.: from a previously scarlatina-infected person or thing

ensued in its degradation and decay, its last stage being that of a limpid fluid, mainly composed of water, salts and acids, amongst which figured the lactic, butyric, capronic, and capriolic. To this downward career it was carried by becoming the habitat of broods of vibrios of different kinds, and, according to Prof. Lister, of a certain bacterium—bacterium lactis—and by certain carpetings of its surface by oidium and other varieties of mould."

¹"M. Fauvel, of the Paris Chemical Laboratory, has discovered an additional danger to that abominable invention, the baby's feeding bottle. Of 31 feeding bottles taken from various creches in Paris, 28 contained both animal and vegetable life, the milk remaining in all smelt badly, was acid and half coagulated; the globules were deformed, and numerous very lively bacteria, along with some vibrios, were present. On cutting open the caoutchouc tube through its length, coagulated milk, with small organisms, was met with; but a still more important fact was the presence in the nipple of a mass of vegetation of cryptogamic nature. Some of these bottles had been washed and were ready for use."

²The lactic fermentation has relations to a small oval bacterium, which hence has been called by Lister, bacterium lactis.

³These deleterious, if not absolutely poisonous changes, in a fluid introduced into the system and taken up into the blood, show how milk and cream, and substances or fluids with which they are mixed and combined, can and will prove a source of originating blood-poison which, in different periods of life—cycles of age—and under different circumstances, will declare itself in symptoms at one time, or in one person, or one or other of so-called different AFFECTIONS; at another time in the same person in a different form or set of symptoms, or at the same time in different people in the different manifestations to which have been given specific names—scarlet fever, typhoid, puerperal fever, diphtheria, erysipelas, etc.—for the purpose of distinction and of conveying to others what we mean, and which I look upon as phases of the evolution of the one poison.

I would call to mind how often the puerperal woman is dosed with gruel made with milk—that has in many cases been "standing" for some hours, and it may be in the puerperal room itself, wherein the atmosphere is not always, by ignorant people, kept the sweetest,—on the supposition that milk creates milk; how the children, and husband, and household often partake of the same milk; and how vitiated (as shown in Mr. Gay's paper) even good milk becomes by "standing," how much more by standing in the atmosphere of the lying-in chamber; and how readily this one fluid becomes an originating virus, a one *origo mali* in a house-hold, or circle of people poisoned with it, amongst whom there would be from this one source a differentiation and divergence of results, of phenomena of the poisonous action, of symptoms directly consequent, so that one or more would be said to be suffering from scarlatina, others from typhoid and diphtheria, or one of the other generally supposed specific and distinct ailments of which I write, the lying-in mother being attacked with what would be termed puerperal fever: all the differentiations resulting not from a difference in the poison, but of the persons brought under its influence.

—is infectious and contagious alike with the other form. I do not now consider *that* to be bastard or simulative scarlatina which I formerly described as such, but look upon it as true, *generated differently, propagated similarly*. I was first led to these conclusions by a case of auto-genetic poisoning, which was under my care in 1875, in which scarlet fever symptoms obtained, but which, recognized to be wholly the consequence of a morbid condition of the uterine system and its excretions, in fact, begotten and conceived *in utero* from putrescing matters therein, I treated accordingly, by grasping the soft spongy uncontracted uterus with my hands, compressing it, expelling thereby the retained and offensive clots, washing out the vagina and uterus by means of Higginson's syringe—Condy and warm water being used every two hours till the lochia became quite inodorous. Had I, however, seen the patient for the first time when those scarlatina symptoms were fully developed, and had I overlooked her puerperal state and the condition of the uterine system, I might have erred in supposing I had to deal with scarlatina from a previously infected person or thing, have overlooked the *de novo* origin of the case—the latent uterine mischief—have neglected the ablutions and cleansing processes, have treated the ailment in the orthodox fashion as being contracted from previous scarlatina infection, and no doubt have lost my patient. But being alive to the puerperal facts and bearings to which I have drawn attention, I at once proceeded as I have described, and the correctness of the diagnosis was evinced by the immediate improvement in the lady, commencing as it did from the very time I got rid of the offensive clots and washed away the noisome lochia. Within twelve months I had three cases of puerperal blood-poisoning, in which not alone was scarlatina rash well marked, but also the other symptoms of scarlet fever. I had learned from my first case valuable knowledge by which I have profited, and these later patients I accordingly treated as suffering from toxæmic scarlatina. Some time ago I had, in consultation with Dr. Joliffe, of Shepherd's Bush, a case of typhoid fever, in which relapse occurred while convalescence was advancing and desquamation—as truly marked as in what would be termed a typical case of scarlatina—was actually taking place. In the same house where lay this sick mother some of the youngest members of the family had previously been affected with scarlatina, others having suffered from what is termed scarlatina throat, while others again were at the time complaining of the same, the cause of the outbreaks being, as far as could be discovered, the defective drains, which allowed regurgitation of noisome smells and vapors into the house.

Since 1875, when I first entered upon the study of this question—the unity of poison and differentiation of resultant phenomena which we call symptoms—not because of any difference in the poison which

The experiments of Pasteur on chicken cholera are well-known. In hope of diminishing the infective power of this organism, he grew it in oxygen for a long time, and the found not only that it produced a modified disease, but that this attack in most cases protected the animal from the effect of the organisms in their most virulent state!—*Brit. Med. Journal*, December 31, 1881. Page 1062.

may be the *origo mali* of so-called various diseases, but differentiations dependent upon the media through which the poison passes, or upon which it falls, the evolutions and developments of that one poison being marked by new phases, new manifestations to which rightly we should not apply the expression separate diseases, but rather call them simply what they are, the various expressions of the evolutions of that unity of origin whatever it may be, which has set them all agoing—since, I repeat, I first entered upon the study of this question, my researches have made clear to me what before I had not recognized; and out of much mistiness, obscurity, and chaos of facts, ideas and thoughts, clearness and definiteness have evolved in my mind, so that what was to me—as to every observer in a new path—at first vague, indefinite, unshaped, has become certain and embodied in realities. What at one time I feared to be incapable of proof has come to be more and more provable, till at length it has become proved; what at one time from former teaching and consequent prejudice seemed to me to be false, has gradually opened out to me as true, till it has come to be established in my mind as a great truth.

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APPENDIX.

It is with great pleasure that I embrace the opportunity of presenting through the medium of the *JOURNAL* the theory of "the unity of poison," advanced by Dr. Griffith, who has courteously permitted me to republish his views, as they have appeared from time to time in the columns of the *Glasgow Medical Journal* and *Midland Medical Miscellany*. The author has kindly consented to allow me to comment upon the views expressed in the foregoing pages, and I shall therefore make such comments as are suggested to me by their perusal.

While accepting, in the main, the arguments advanced by Dr. Griffith, and believing them to contain the germ of a great truth, there are some points to which I think we may take exception. In the first place (such eminent authorities as Paget notwithstanding), the propriety of dividing scarlatina into two distinct forms, such division being dependent upon a hypothetical difference in their etiology, is questionable. The occurrence of efflorescence, pyrexia, desquamation, and albuminuria, one or all, consequent upon the ingestion of tomatoes (which ordinarily are a healthful article of diet), does not constitute scarlatina, any more than pain in the side, fever and cough, one or all, constitute a pneumonia, but such scarlatinoid symptoms simply indicate that the poison, whatever it may have been, has the same physiological action as the *materies morbi* of scarlatina. The similarity of symptoms does not indicate a similarity of poison, but that there is some physiological co-relation due to the fact that the effects of both poisons are manifested through their influence upon the same portion of the economy, viz.: the sympathetic ganglia and trophic nerves. The "toxæmic scarlatina" is probably not scarlatina at all, being simply a collection of morbid phenomena due to profound disturbance of the sympathetic ganglia, by a powerful organic poison, and perhaps influenced greatly by

idiosyncrasy. Some persons cannot eat shell-fish without a severe resultant urticaria, not because shell-fish *per se* are poisonous, but because the sympathetic system in such individuals is intolerant of that particular article of diet. As Dr. Griffith states, belladonna produces an efflorescence resembling scarlatina. Quinine, copaiba and other drugs will produce an eruption, and in the case of quinine especially, usually in cases of idiosyncrasy. The specific poison of syphilis produces an eruption resembling measles, which is entirely independent of cell proliferation. This "roseola" is transitory, and produces no permanent change in the tissues affected. We could hardly accept these phenomena as evidences of their identity with measles or scarlatina. But there must be some explanation of their similarity, and that is the fact that *certain poisons act upon the system through the medium of some physiological disturbance of the sympathetic*. The medium being the same in all cases, a similarity of the resultant morbid phenomena might reasonably be expected. Perhaps, as the author states, the difference between the different series of phenomena, as developed particularly in the two forms of scarlatina, lies only in "origin," but that is often a very wide difference. The only essential difference between narcotic and uræmic coma is in their origin, yet they are not the same. It is the peculiar origin of each that constitutes the disease.

The author is quite correct when he asserts that the absence of desquamation and albuminuria is no proof that scarlatina is not present in a given case, but at the same time that fact could hardly be advanced as evidence of the unity of origin of "orthodox" scarlatina, and the systemic disturbance caused by the ingestion of tomatoes, or too high game. Even if both phenomena were quite frequently present in the so-called "toxæmic" scarlatina, they could be quite readily explained, without recourse to the theory of "unity." Desquamation might readily follow any peripheral irritation which interfered with the nutrition of the skin, and as is well known, the same factor of peripheral irritation would explain the albuminuria. As has been shown by Mihrau¹ a full hot bath will produce it. Irrespective of the cause, a vicarious strain upon the secretory structure of the kidney may be quite as effective in the production of albuminuria, as may the selective action of some poison.

While I am heartily in accord with the author in his opinion as to the co-relation of the poisons of disease in general, and especially with the explanation of such an interdependence based upon evolutionary laws, I incline to the belief that the tomato argument is not of great weight. It is hardly safe to attempt to support so radical a position, with so few cases. Again, it must be acknowledged, that it is only in a very small proportion of cases that we can trace our cases of scarlatina to their sources of contagion. In the instance of both the tomato and game cases cited by Dr. Griffith, a mere coincidence is highly probable.

¹Thèse de Paris, 1882.

In a case of puerperal scarlatina, (even if the occurrence of a "toxæmic" form of scarlatina be admitted) it would be absolutely impossible to differentiate it from true scarlatina with a complicating septæmia, which is more than likely to occur, and which in my own estimation, is the explanation of the fatal character of scarlatina in the puerpera, whose secretions become septic as soon as the woman is attacked by the specific disease. The case of "toxæmic" puerperal scarlatina cited by the author, is an ideal picture of scarlatina in the puerpera, complicated by septæmia. I can see no logical reason for the inference that such a case of scarlatina is caused by septic infection.

If such were the case, the disease would occur more frequently in our hospitals, where septæmia is so frequently seen. Even from the evolutionary standpoint, (which in the basis of Dr. Griffith's views) it would be very difficult to explain so sudden a transformation of simple septic matter into the *materies morbi* of scarlatina, as such an occurrence in the puerpera would imply, particularly in cases of auto-sepsis. The law of evolution is certain, but it is as gradual as it is certain and irresistible. The train of symptoms referred to by the author may not be rare in London, but they are certainly rare in this country, as I think will be conceded by our physicians of extensive midwifery experience, and their rarity is still greater, if cases of indubitable contagion be excluded. As for the *de novo* origin of scarlatina, it can hardly be possible under the circumstances mentioned by Dr. Griffith.

But can scarlatina ever be developed *de novo*? I am quite ready to believe that it can; *but such development must occur by a normal and gradual process of evolution, not suddenly by autogenesis through the medium of a simple septæmia.* Scarlatina has its own peculiar and specific germ, *materies morbi*, or whatever we may choose to term it, and this cannot develop spontaneously in the true sense of the term, *but in the presence of an appropriate environment, the minute organisms (or minute particles of organic matter, if such be the term preferred) everywhere present in the atmosphere, may by a slow and certain process of evolution, undergo a transformation, and acquire new properties by virtue of which they become infectious, and capable of producing disease of a specific type.*

In the same way, organisms of a higher specific type may retrograde, and become harmless. These views may serve to explain the gradual rise and decline of intensity of epidemics. The increase and decrease of virulence of specific diseases may thus be due to imperceptible changes of environment. The propriety of this adaptation of the great truths of evolution to the explanation of the origin and natural history of disease, will of course be disputed by all who oppose evolution as a whole, but if the theory of evolution be correct, and the germ theory of disease be proven, the co-relation of the two is indisputable. *Accept evolution, and we must accept the evolutionary theory of disease.*

It is extremely doubtful whether the processes of simple fermentation and putrefaction are capable of

developing the *materies morbi* of specific disease (that of scarlatina for example) an occurrence which to Dr. Griffith, appears highly probable. Septic materials, when introduced into the stomach, are speedily shorn of much of their noxiousness, and the results of such ingesta are as a rule, chiefly local. The gastric juice is the great physiological antiseptic, and constitutes an almost impenetrable barrier to many noxious materials of an organic nature. That simple lactic fermentation is capable of developing a systemic poison of a specific character, I am hardly prepared to believe, and surely, we have very few facts to support such a theory. Fortunately for us, the gastrointestinal tract is very intolerant of many of the injurious products of fermentation and decomposition, and this intolerance results in their elimination, before they have had an opportunity to act injuriously upon the general nutrition.

In confirmation of his views the author quotes Sir Wm. Gull as stating "that diphtheria may commence locally, forming a poison which will propagate itself," he having consequently come to recognize two distinctive forms of diphtheria.

Now I am under the impression that it is a well recognized fact that diphtheria may commence locally by inoculation, and it is supposed by the majority of observers, that such commencement can occur in no other way. There is, however, no clinical distinction as far as known, between the cases resulting from inoculation, and those from constitutional infection. Gull's theory does not imply inoculation, but a spontaneous development of the diphtheritic poison. This I believe may occur, but in a manner so gradual, that it is extremely difficult to say where a simple process terminates and a specific begins. In accepting this view, however, I can see no grounds for differentiating the disease into two distinct forms. The degree of severity may differ in different cases, but the type of disease remains unaltered. Again, admitting Gull's theory as to the origin of diphtheria to be correct, it is difficult to understand how it can be adduced as evidence in favor of simple lactic fermentation developing scarlatina.

That the noxious vapors and gases of the sewer are capable of producing a variety of diseases, is well known, but it is quite generally accepted that such sewer emanations contain the distinctive germs of these different affections. There would seem to be, however, many peculiar cases which are hardly explicable upon the ground of a specific germ infection, and it is just here that an acceptance of Dr. Griffith's views is almost unavoidable, at least, in so far as his doctrine of the interdependence of the various poisons of disease and their unity of origin in certain instances, is concerned. As before stated, however, I believe that the transformation of the one original poison into a number which affect the human system by the production of various so-called specific disorders, must occur through a gradual process of evolution, and not by a sudden metamorphosis such as must occur if the author's deductions regarding the origin of his cases of scarlatina be correct.

There are many striking facts bearing out the theory of "unity of poison." The close similarity between

certain malarial fevers and typhoid, between typhoid and typhus, and the marked resemblance of a certain febrile condition attended by diarrhoea (which we are so fond of attributing to sewer gas), to typical typhoid, may be explicable from the standpoint of unity. There must be something more than professional stupidity to account for the difficulty experienced in differentiating certain diseases, and which almost brings the diagnosis down to the level of guess work. What peculiar combination of circumstances relating to the atmosphere, constitutional condition or hygienic surroundings of the people, determines the prevailing epidemic influence in any given locality, is unknown, and probably will always remain so.

The possibility of the spontaneous origin of specific disease has ever been a source of controversy, but that such an origin is demonstrable will, I think, be evident to anyone who carefully studies some of the more recent epidemics. The late epidemics of typhus fever in New York are excellent examples, and the preponderance of evidence would seem to show that the epidemic of cholera now declining in France, was due to local causes, and consequent spontaneous development of the disease. It is only by a recognition of the *de novo* origin of diseases of a specific type that modern sanitation can accomplish its object.

A very interesting instance was recently reported in the *N. Y. Med. Record*, by Dr. L. J. W. Lee,¹ which would appear to be strong evidence in favor of the theory of unity. In this instance, among ten cases of disease in one house, variola, varicella, measles, typhoid, and remittent fever were said to be represented. As might be imagined, the theory of unity of poison is not an easy one to support by actual demonstration, *e. g.*, it would be difficult to prove in any given instance, that the emanations from the sewer, did not contain germs derived directly from cases of specific disease.

The theory of unity as I understand it is in nowise opposed to the germ theory, and the same may be said of the theory of the spontaneous origin of specific disease. Each may take as its point of departure, the harmless organisms universally a component of the atmosphere. These organisms, from causes not at present definitely known, but unquestionably by a gradual process of evolution and adaptation to environment, become metamorphosed into poisonous germs, which, when introduced in the human system, will produce disease. An example of this is quoted by Dr. Griffith,² from Paget.³ In this instance, occurring on board a vessel at sea, a sailor wounded his hand with a beef bone, (the condition of the bone is not stated, and there is reason to surmise that it may have been poisonous from decomposition), the injury being followed by fatal cellulitis with axillary suppuration. Subsequently, cellular or erysipelatous inflammation in various forms, ran through the whole ship's company, and terminated in an epidemic of mumps. The whole

course of disease was over a month, the ship being at sea the whole time, and during the greater part of it, in the severe cold of a southern winter. Here, the first case in the presence of the crowd poison always present on board ship, afforded a laboratory for the formation of an intensely infectious poison, which in consequence of the intimate relations existing between the members of the ship's company, had no obstacle to its dissemination. Whether the parotitis was the genuine "mumps" or not is questionable. It may have been, in a mild form, the same inflammation of the parotid, which is liable to occur in pyæmia, septic peritonitis (so-called), and other toxæmiæ. A case is cited by Dr. Griffith from his own experience, which, although interesting, is open to the same objections offered to some of his other evidences in favor of "unity of poison." In this case a patient convalescing from typhoid fever suddenly developed symptoms of scarlatina. Cases of true scarlatina, or of scarlatinal sore throat, were present at the time, or had been present in the house. "The cause of the outbreaks was, as far as could be determined, the defective drains." Now, these causes probably did result from the same cause as far as the sewer exhalations were concerned, but, unfortunately, sewer gas is a compound and decidedly complex substance, and in this case, as in others, may have contained the germs of several specific and distinct diseases. I wish that it were possible to accept such evidences of "unity of poison" as indubitable, as it would greatly simplify our studies of etiology. As I have already remarked, I believe it extremely difficult to support the theory by actual demonstration, but, nevertheless, the known facts and powerful evidences of the theory of evolution in general, associated with our knowledge of the germ origin of disease, render the theory of "unity of poison" one of the most logical explanations of the causation and differentiation of disease, yet advanced. I believe, however, that the common origin of the poisons of disease is not a poison, but a harmless microscopic organism.

125 State street, August 5, 1884.

MEDICAL PROGRESS.

OBSTETRICS AND GYNÆCOLOGY.

MASSAGE IN GYNÆCOLOGY.—Dr. Prochownik has recently read a paper on this subject at the *Versammlung Deutscher Naturforscher und Aerzte* at Magdeburg, a translation of which is given in the *Medical Chronicle*. He refers to the doubt and reserve with which this reintroduction of massage into therapeutics was received, and acknowledges that its results were certainly for a time exaggerated, and that in consequence it fell into the hands of the humbler sort of practitioners and of quack specialists. But already the results of massage in surgery, and even in gynæcology, in the hands of some of the operators by profession, have been so striking that the subject urgently challenges an experimental investigation. As one who has freely used it for two and a quarter

¹Influence of diathesis upon contagion. *N. Y. Med. Rec.* Vol. xxv, No. v, p. 84.

²*Midland Medical Miscellany*.

³Paget, *Surgical Pathology*, 3d Ed. P. 373.