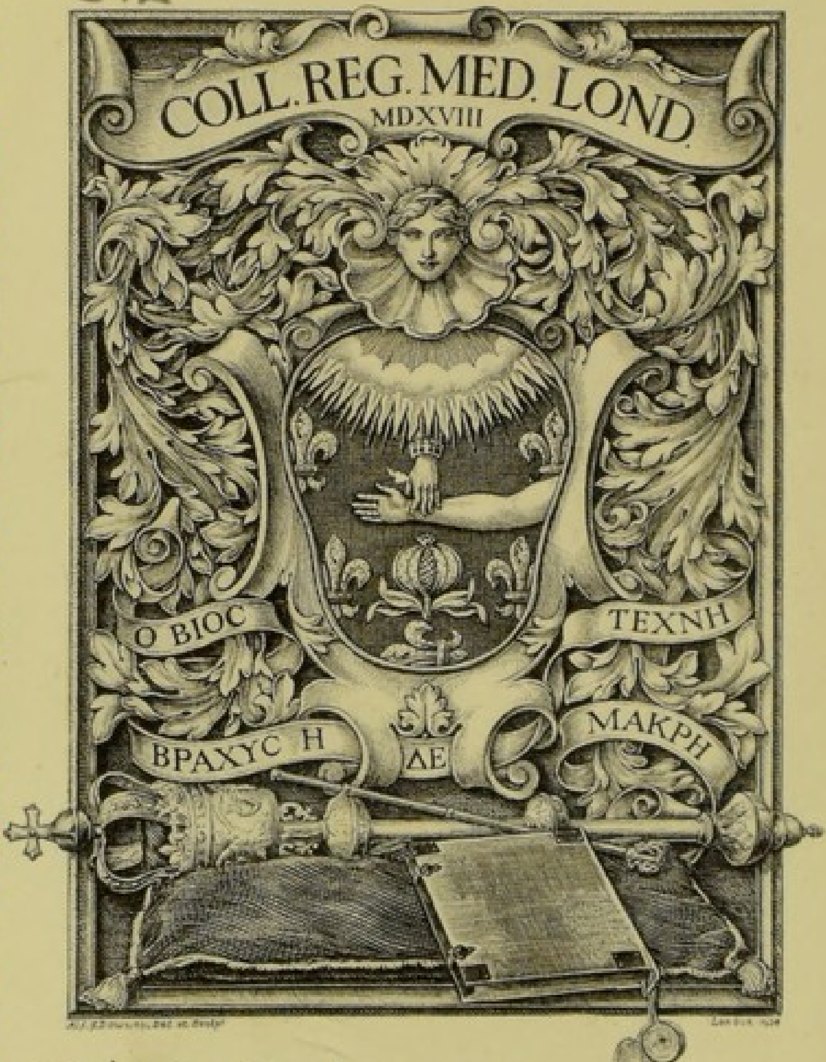
COLL. REG. MED. LOND.
MDXVIII
Ο ΒΙΟϹ
ΤΕΧΝΗ
ΒΡΑΧΥϹ Η
ΔΕ
ΜΑΚΡΗ

S.L.

Presented by Thomas Watson, M.D.
President of the College.
1864.

LETTERS ON SYPHILIS.

LETTERS

ON

SYPHILIS:

ADDRESSED TO THE

CHIEF EDITOR OF THE UNION MÉDICALE,

BY

PH. RICORD,

Chirurgien de l'Hôpital du Midi; Chirurgien-Consultant du Dispensaire de Salubrité Publique;
Membre de l'Académie Nationale de Médecine, de la Société de Chirurgie,
et de diverses Académies et Sociétés savantes; Chevalier
de la Légion d'Honneur, etc. etc. etc.

With an Introduction,

BY

AMÉDÉE LATOUR,

Rédacteur en chef de l'Union Médicale; Secrétaire du Comité Consultatif d'Hygiène Publique.

TRANSLATED BY

W. P. LATTIMORE, M. D.

PHILADELPHIA:
BLANCHARD AND LEA.
1857.

STEREOTYPED AND PRINTED BY
T. K. & P. G. COLLINS, PHILADELPHIA.

TRANSLATOR'S PREFACE.

To what cause can we attribute the confusion which has so long existed, and which unfortunately still exists, in regard to Syphilis? With respect to our own country, where but few great minds devote their attention to the subject, the question is easily answered. We have given full credence to the teachings of English authors, and hence the same uncertainty which prevails in London relative to the disease may be observed in the United States. Some one has said that, "the higher the degree of civilization, the greater the prevalence of syphilis;" and since our motto is *Onward!* we may look for a further extension of the malady. Be this as it may, it is undeniable that this scourge of mankind no longer confines its ravages to towns and cities, but is beginning to penetrate our villages and hamlets. Our large commercial cities furnish the seed, which, like the down of the thistle, is scattered everywhere, from the shores of the St. Lawrence to the Gulf of Mexico. What can be done to arrest its progress? It is at least especially necessary that we should understand the character of the enemy we are required to combat.

Hunter, "the Father of English Surgery," systematically confounded blennorrhagia and syphilis; and this confusion still prevails in England and France; though the labors of M. Ricord, during the last twenty years, have done much, in the latter country, to open the eyes of physicians to a knowledge of the fact that these are distinct diseases. But so much is the influence of Hunter's great intellect felt that nearly all English writers still tread in his footsteps. Hunter taught that the administration of mercury during the existence of a primary sore prevented secondary manifestations. In this opinion all English syphilographists coincide. Now if, as M. Ricord teaches, and as we believe, chancre is at first *local*, why administer so powerful an agent? It is replied that *some* chancres are followed by secondary symptoms, and that it is better to subject the greater number, in whom constitutional symptoms would not be developed, to the action of mercury, than to permit the disease to manifest itself in the few. Were this reasoning correct, it might be as forcibly recommended to perform the operation of tracheotomy in every case of croup, or of amputation in every case of compound fracture. But, admitting the propriety of a universal mercurial treatment in primary sores, does the administration of the medicine prevent the subsequent manifestation of constitutional accidents? Does it prevent the establishment of what Hunter calls the "syphilitic disposition?" Not necessarily, as is proved by those cases in which the disease shows itself from one year to twenty years subsequent to the appearance of the primary sore. It no more *prevents* the establishment of the diathesis than it *radically cures* this diathesis when once developed. M. Ricord states that four-fifths of the cases of secondary syphilis never return when methodically treated. Do we observe a larger proportion of chancres which are not followed by constitutional syphilis, when *mercurially* treated? Yes; but this result is simply due to the fact that these chancres would never have infected the system. Why, then, persist in giving mercury indiscriminately in all cases of chancre? This is one of the superstitions belonging to a former period; and the time has now come for more enlightened practice.

The action of mercury tends to *denaturalize* syphilis, and to prevent its otherwise regular evolution. It leads observers to suppose that the first constitutional symptoms may really be manifested at any time whatever subsequent to the existence of a chancre—that is to say, after the lapse of months or years indifferently; and to infer that the disease always pursues an erratic course.

To this circumstance must we attribute the crude and confused notions which still prevail among English syphilographists. In France, the school of Broussais accomplished this good, if nothing else; that is to say, it permitted syphilis to pursue its regular course, uninfluenced by the trammels of mercury.

M. Ricord has reduced to order that which before was wholly devoid of system. Light has broken in upon the darkness of ignorance and superstition, and henceforth we may hope to see the pathology of syphilis assume that rank which, by reason of the universal prevalence of the disease, and of its grave consequences on the affected individual and on succeeding generations, it so well deserves.

To those who have listened to the able and interesting lectures of our author at the *Hôpital du Midi*, this volume will need no commendation; while to those who have not had the pleasure to which we allude, the book will commend itself by the truths it contains, told as they are in the same inimitable style in which M. Ricord delivers his clinical lectures.

These Letters appeared in the *Union Médicale* of Paris during the years 1850 and 1851, and were received with great favor by the profession. Although a knowledge of certain events which transpired in Paris may have had some influence in keeping alive this interest, yet we trust the translation of these Letters will prove acceptable to the medical profession in the United States, since they furnish the most complete exposition of the doctrines of M. Ricord which has yet appeared.

W. P. L.

NEW YORK, February 4, 1852.

CONTENTS.

INTRODUCTION.

To Monsieur Ricord:—

My dear Friend: My first expression ought to be one of gratitude.

The journal which is intrusted to my care has been fortunate in receiving yonr valuable communications; and I am still confused with the honor you have done me in associating my obscure name with the popularity and glory of yours.

Your Letters, my dear friend, have obtained a degree of success which is seldom recorded in the annals of our medical literature. I am well aware, and I ought to inform you of the fact, that some persons, who have, alas! very legitimate motives for loving neither wit nor style, blame severely both the wit and the style of your Letters. It is most fortunate that you are not at the commencement of your professional career! You would be dead as a practitioner, my dear friend. There would be an end of a physician, a man of *esprit,* who dares to write his language correctly and gracefully; who is impertinent enough to give attraction and piquancy to his descriptions; who is so unfortunate as not to recoil at an anecdote; and who is so imprudent as not to fear that he shall make his readers smile;—that is to say, there would be an end of you, my friend, for you have proved yourself to be a writer both *spirituel* and acute, a critic who possesses a charming atticism, and one who treats grave subjects in an agreeable manner. For the physician who aspires to a practice, there is no worse reputation than that of being a man of wit. At one of the late sessions of the *Faculte de Paris*, a fortunate candidate, although eminently *spirituel,* was obliged to receive from a friend, one of the judges, this strange compliment as a home-thrust: "I am satisfied with you; you have shown no *esprit.*"

Truly was Guy-Patin happily inspired when he addressed his delicious letters only under the confidential cover of friendship. Had others, besides his friends Spon and Falconnet, suspected his original and piquant sallies, the vigorous and *spirituel* enemy of antimony and of Mazarin would have enjoyed neither his rich practice, nor the honors of the deanery, nor his chair in the College of France.

And yet, dear friend—trust to my slight experience as gardener, and I refer you elsewhere for proof—the handsomest and the rarest flowers require, in order to produce their brilliant colors, a soil still richer than the richest cereals.

Happily for you, you commenced your career by the production of solid memoirs; from a large octavo volume you advanced to a heavy folio, completely filled with fine pictures; and you annotated the fine translation of the work of the grave and learned Hunter—a translation with which our skilful and modest friend Richelot has endowed French medical literature —before writing your Letters. Without this perfectly respectable baggage, you would run a great risk of not being *a serious man* in the opinion of a great number of honorable *confrères*, who only estimate success by weight and by the volume. You discovered something of this truth when you knocked at the door of our Academy—at that door which should have been widely opened to you, but which was twice made so narrow that your merit could not pass it. Were you perfectly aware of the reproach which was cast on you? The reproach was your teaching, my friend; that teaching which is so instructive, and at the same time so amusing; your improvisations at the hospital, so picturesque and descriptive; your attractive and imaginative lectures, of which your Letters are so faithful a reflex. Instead of putting your audience to sleep, you constantly keep it awake by the twofold attraction of science and wit. Now, there are many persons who do not like to be disturbed in their sleep. It is this circumstance that caused a witty friend of mine, who had the good sense to display his wit only in secret, to say that among physicians only the *imbéciles* had wit.

It is true, my dear master, that this friend placed you in the front rank of the—*imbéciles*.

You understand very well that I have not written this letter with the sole object of thanking and complimenting you. Yet I scarcely know how to reconcile what I have said with that which is to follow; for I have a reproach to make, and an omission to point out.

The reproach I have to make is not the expression of my personal opinion alone; it is also the sentiment of a great number of our *confrères*, men of taste, of science, and of prudence—of men whose opinion and advice you are accustomed to hold in great esteem.

Very well! my friend, as the faithful reporter of what I have heard, I reproach you with having attached too much importance to some recent ideas upon *syphilization* and *syphilism*.*

There are views and theories with respect to science which we must allow to make a certain progress before we give them our attention. By criticizing them too soon, we give them the appearance somewhat of being martyred, of which their advocates are not slow to avail themselves. The sciences are paved with these misunderstood geniuses, who run after the inquisition of Galileo. You are well aware that all the follies and all the extravagances of the human race invariably take shelter behind this great name; but you are also aware that for one Galileo there are to be found a thousand Cyranos de Bergerac.

One of the greatest and most incontestable principles of the Baconian philosophy is the fact that, in scientific criticism, an idea, an assertion, a theory is nothing unless it be accompanied by demonstration, by proof, by fact. Now, the fact which you demanded has not been produced. This proposition is all it was your place to verify. To enter upon the ground of speculation and of dogma, was to expose yourself to defeat by adver-

* I ought, however, to state that this reproach should not fall upon M. Ricord exclusively. The publication committee of the *Union Médicale* received the letter of M. Auzias-Turenne, and inquired what should be done with it? Not to publish it would be to give the author the pretext of crying out against a systematic stifling of his doctrines. To publish it without remark or comment would be to assume a sort of responsibility which none of the members of the committee, and which the *chief editor* especially, were unwilling to accept. Consequently, M. Ricord was invited to reply; and I esteem it unfortunate that this invitation was too much in conformity with his desires.

saries who can wield better than yourself, a man of practical science, the perfidious and so frequently deceptive weapons of dialectics. By reasoning, one can prove anything he wishes. Our learned and *spirituel* friend Malgaigne—another *imbécile*—proved to us one day, with the aid of an irreproachable syllogism, that a part was equal to the whole. Men of learning were present who revolted *in petto* against this audacious paradox, but who remained dumb, so logically impregnable did it seem.

You cultivate general with the same success as special surgery; and were you not, my friend, slightly bitten by the tarantula which bit the surgeons of a certain period in regard to strabismus? Confess that you too have on your conscience some section of the muscles of the eye. But, as you are a loyal and sincere practitioner, I am sure that at the present time you are satisfied that ocular myotomy has occasioned more cases of strabismus than it has cured. Well! I, who from taste and duty interest myself a little more than yourself in mental maladies, have discovered one which I name intellectual strabismus.

Fix the glance of one who squints; you are never sure that he looks at you.

Listen to, or read an intellectual squinter; I defy you to divine whether he speaks or writes from conviction or from reason. Seek to rectify a chain of reasoning which appears awry; you only displace the deformity. He squinted to the right; he is going to squint to the left. And this is precisely what occurs after ocular myotomy in visual strabismus.

Do not deceive yourself, my friend; your reflections upon syphilization will have produced no influence upon the minds of those who propagate this idea. Perhaps, however, it may produce a little more irritation against yourself, whom they will accuse of wishing to stifle the truth.

As to the facts which you require, beware of them! Nothing is more deceptive, more fallacious, more perfidious than the medical *fact*. For a very long time, in my lucubrations as journalist, I have asked what is a fact, what its definition, what its characteristic? Our great philosophers have not yet had time to satisfy my curiosity; and I am compelled, as before, to admit or to reject a fact solely in accordance with the feeble light of my own intellect. You are aware, however, of the number of errors and follies which have been put in circulation in the medical sciences by the aid of pretended facts. Syphilography has its full share in the contingent of absurdities supported by facts—a circumstance which no one knows better than yourself.

Observe, that it is not simply a medical fact, already tolerably complex, which you will have to appreciate, but also an experimental fact, which singularly complicates the problem, and which ought to stimulate all the nervous pulp which presides over your organ of attention.

But what am I doing? I am sermonizing to a convert, am I not? You have given proof of so penetrating a criticism in regard to the inoculators of secondary accidents, that you will not allow this valiant sword to waver in your hands, when the hour shall come, if it ever does come, for combating the theory of syphilism. The public loves you; it esteems your works; and it trusts that, in this particular, you will not disappoint it. But my affectionate devotion authorizes me to tell you that the public is disquieted by some expressions contained in your last letters. It has observed a little complacency, a little weakness, perhaps, in regard to the *naïve* avowal that syphilism *is born of your school;* that *it is the offspring of your doctrines;* that you have been *the prophet of syphilitic vaccination*, &c. All this is true; but for this very reason you have felt a greater reserve in acknowledging your children. You should recognize only those which are legitimate; and if you really intend to be the Saint John forerunner of syphilism, you assume from that very fact the obligation of announcing only the true Messiah.

Now, I have no fear of stating that the theory of syphilization, as produced at Turin and at Paris, does not yet deserve to fix the attention of serious men like yourself. It may make, it has already made, victims, which is the highest of all reasons why you should not give it a semblance of importance by untimely criticism. For, you are aware that a theory which is contested by no one remains a theory. If you criticize it, it becomes a religion; and every religion has its martyrs. Do you not think that syphilis has made enough of these martyrs?

2

You are undoubtedly, my friend, better acquainted than myself with syphilographic history and literature; you are also more familiar than myself with the fact that this part of medical science has been, since the close of the fifteenth century, a soil in which eccentric ideas and extravagant opinions have plentifully sprung up. Have you not been surprised, in the course of your reading, to find that all these extravagant opinions, with whatever *éclat* they have been brought forward, have so slightly disturbed the tendency of true and positive ideas? Is not this fact attributable to the slight attention which really influential men have paid to them?

I shall give a single example—for I have a horror of all appearance of erudition, which should be presented only for the gratification of an internal court. Indeed, I will say of it what Voltaire said of self-love; that is to say, he compared self-love to the generative organ, which gives pleasure, which it is comfortable to possess, but which must be kept concealed.

In 1811, several years before the physiological school, again bringing forward the forgotten theory of Bru, dreamed of denying the existence of the syphilitic virus, there appeared a pamphlet entitled: "ON THE NON-EXISTENCE OF THE VENEREAL DISEASE; *a work in which this disease, invented by the physicians of the fifteenth century, is proved to be only the union of a great number of morbific affections of different natures, the cause of which is falsely attributed to a contagious virus which has never existed.*" Certainly, this is a moving title; and, by the way, you see that it is cousin-german to the title of the more celebrated work of M. Richond des Brus. This pamphlet appeared when the doctrine, I might say the religion, of the venereal virus, was in its highest ascendency. Something more than temerity was needed in thus daring to brave all the medical opinions of the times. The author felt this necessity; consequently, notice the proud disdain of his opening paragraph:—

"Let one of those incredulous persons who believe only what they see, or one of those men who believe everything, be placed on a high tower; let him examine the sun from morning till evening: he will see the sun rise on one side, and disappear on the opposite, and he will be perfectly persuaded that it makes the same journey each day.

"Thus believed the philosophers of Greece and of Rome; and the savans of Judea, of Arabia, and of China; and thus would we still believe, had not men of genius, rising superior to generally received opinions, proved that the sun always remains in the same place, and that it is the earth which moves around it.

"It is known"—here comes Galileo—"what persecutions Galileo experienced for having announced this truth.—I find myself in the same condition as Galileo," &c.

Thus, the author expected all sorts of persecutions. But compose yourself on his account: this prudent Galileo did not make himself known, and his pamphlet remained anonymous.

What noise did it make? What emotion did it excite? I cannot tell; for I have found neither trace, nor remembrance, nor mention of it in the literary history of the time. Cullerier the elder, the Ricord of his time, perhaps did not read it; certainly he did not speak of it. And yet, I assure you, this work is not without value. It is written in an energetic and attractive style; and I find in it many ideas which, though paradoxical when they appeared, have by you been established as truths; such, for example, as the distinction the author draws between benign and virulent blennorrhagia. I even believe—may God forgive me!—that Jourdan only developed and extended the various chapters of our anonymous author.

Be this as it may, this doctrine was received in silence, and fell into oblivion. All the revolutionary power of Broussais, all the ardent proselytism of his school, were needed to revive it some years afterwards; a fortunate resurrection, my friend, for you have had the glory of showing its nothingness, and of placing the doctrine of virulence upon the solid basis of observation and experiment.

But I must express, in the name of your most admiring readers, my regret that a void exists upon a subject which, it appears to me, might have been very properly filled by your Letters.

Where is syphilis principally contracted at the present time?

Had you proposed this question, it would have led you to treat of one of the gravest and most delicate points of public hygiene and of medical police. I am about to point out the problem, without being able to solve it, and shall be fortunate if I succeed in inducing you once more to resume your pen, with the object of making known what your very favorable position has taught you on this subject.

Two facts, which are equally important, but between which we can perceive no connection, strike the attention of all who are at this time studying syphilis in its relations to public hygiene.

On the one hand—and I speak especially of civilians, for it appears that in the army the case is different, since the adoption of certain measures in 1842—the number of syphilitic men does not sensibly diminish.

On the other hand, the number of diseased prostitutes has been considerably reduced; to such a degree, indeed, that, according to an official communication which I recently received from the learned M. Trébuchet, chief of the sanitary bureau at the prefecture of police, the dispensary contains at present scarcely one diseased girl in four hundred.

Whence arises this apparently contradictory result—this decrease of the disease at its very source, while the number of syphilitics is now almost equal to those which formerly existed?

This circumstance, we are everywhere assured, is attributable to the fact that the sources of syphilis have been shifted. The disease, so happily checked in public prostitution by the judicious and efficacious measures adopted by the administration, has tended to concentrate itself entirely in that continually augmenting class of the female population which practise clandestine prostitution, against which the police, believing itself to possess no control over it, leaves the public without protection.

Who is better adapted than yourself, my dear friend, with so many opportunities for observation in a vast nosocomical clinique, and in an immense civil practice, to inform us how much truth there may be in the assertion?

If all that I have stated be true, is it not for the interest of public morals and health to enlarge the definition of prostitution?

Is there not ground for calling the most serious attention of the vigilant magistrates of the city to the necessity of reaching this prostitution, which is a thousand times more dangerous than that which is legalized, inasmuch as it is more attractive? By this means syphilis is contracted, and extensively propagated with a frightful rapidity!

This prostitution is called *clandestine*—a singular term, dear friend, to designate that which is exhibited in the galleries of the theatres, in the public balls, in all those places of pleasure, in fact, which are at present no more than immense brothels! What! does the police think it has the right of imprisoning in Saint-Lazarre, without process and without judgment, an unfortunate girl, inscribed upon its books, who may in some point have contravened the severe regulations to which she is subjected, and thus to disarm the poor girl; while a cohort of women are left with impunity to compromise the fortune and the health of young men! What! has the police the right to enter at all hours those houses where *imbéciles* and dupes give themselves up to the chances of dice, while it pauses undecided upon the threshold of a courtesan who poisons ten or twelve lovers a day! What, then, is prostitution, if it is not "the notorious commerce of one's charms?" Some one says there must be provocation upon the public street. That is a bad test of prostitution. The best frequented houses take good care to give no direct provocation; else would they be at once deprived of their prudent and wealthy custom; and yet the police holds them none the less under its beneficent supervision. And what is the tendency of those strangely lascivious dances at the balls of Asnières and Mabille; of those nights at the opera, where provocation lurks in everything—in the costume, in the gestures, and in the voice; and of those nocturnal orgies in the private saloons of some famous *cabarets*, the description of which casts into the shade the frightful picture of the manners of the Romans at the decline of the empire?

What pen is more competent than your own, my dear friend, to describe the ravages of this clandestine prostitution, the misfortunes it occasions, and the troubles it excites in families? Who is better adapted than yourself to trace the syphilitic poison from its present numerous sources, insinuating itself into the ranks of the best classes of society, infecting the purest and chastest spouse, and rendering her barren, or unfit to carry to term the fruit of conception? Who better than yourself can trace the affecting history of him who has *inherited* syphilis—the subject, I know, of your most earnest researches? Who, in fine, is better adapted than yourself to make known to the administration the only sure and efficacious prophylaxy against the disease—the one which must be intrusted to medical science?

I well know that the treatment of these questions is an excessively difficult and delicate task. I am also aware that, in spite of the estimable productions which have appeared, in the front rank of which must be placed the judicious and excellent work of Parent-Duchâtelet, much still remains to be done in regard to prostitution. I well know that the administration too often remains powerless to repress abuses of which it is not ignorant; I well know that prostitution is to-day imperfectly and very arbitrarily regulated; I well know that the administration itself requires a power less contested, and a jurisdiction more legally constituted than it possesses; I well know that great and numerous efforts, in this respect, have been made by the governments which have existed since the Convention; I well know that it is more than doubtful whether a legislative assembly will ever consent to give its attention publicly to this sad and painful subject; I know, finally, that the study of prostitution and its cause is intimately allied with the closest investigations into social economy, into the condition of women in modern society, and the meagre compensation of their labor—and that recent developments in this respect have occasioned trouble and indecision among the most generous minds.

Yes, this question is full of difficulties; but, in view of the impressive fact that the prostitution which I am unwilling to call legal, and still less official, is at present, in the city of Paris, a social evil incomparably less than that which results from what may be called free prostitution, which is wholly without clogs, I believe, my friend, to use a homely phrase, that we have something to do; and I should be happy to transmit your ideas on this subject to the readers of your Letters.

Like myself, you believe that the noblest mission of our art consists, not in the cure of disease by therapeutics, but in the prevention of them by hygienic measures. For this reason, I present these ideas with confidence, and as it were in a propitious soil—in your mind and in your heart. To syphilis, to its pathological and therapeutical study, you owe the finest portion of your legitimate renown. To you especially, by your intelligent counsels relative to the employment of the speculum in our search after the syphilitic virus, belongs the glory of having almost extinguished the poison in public prostitution. Well, my dear friend, it is necessary to complete this truly humanitary trilogy. Pursue this frightful malady even to the perfumed boudoirs of our modern Laïs. The poison, incessantly hunted, is gradually dying out in the Venus of the crossings. Having taken refuge in the libidinous and covetous alcove of unpunished courtesans, it imagines itself safe from the investigation of the *Bureau des Mœurs.* Prove that the syphilitic virus should not enjoy, any more than robbery and murder, the privilege of an asylum, and public morals will owe you a debt of gratitude.

Yours, with affectionate regard,

AMÉDÉE LATOUR.

LETTERS ON SYPHILIS.

LETTER I.

My dear Friend: The new doctrine on Syphilis experiences the fate of every scientific discovery. For nearly twenty years, I have sought, by my teaching and by my writings, to make it penetrate the minds of my contemporaries. I see, however, that it is not equally understood by all. Certain adversaries still make objections to it—objections which I have a hundred times refuted; and, what is more curious, others seize upon objections raised by myself, and imagine, a little naively, perhaps, they can vanquish me with arguments introduced into this discussion by myself.

At this I am neither astonished nor indignant. On the contrary, I find in it a new inducement to continue my work; and, far from complaining of my adversaries, I rather thank them for keeping my zeal on the alert.

Thus, I am about to ask your permission to make an exposition of the true doctrines of the *Hôpital du Midi*, in your widely-circulated journal. Allow me to say that it is less an individual response, than a general exposition, which I intend to make. As I proceed, I shall encounter objections, and will endeavor to reply to them; I will also bear in mind, as far as I ought, a recent publication from the pen of one of your skilful co-laborers, who, in order to find *followers*, need not have gone modestly *to the country* to seek them.

I will present you, my dear friend, a preliminary reflection suggested by the publication to which I have just alluded. Because an observer may not be permitted to see all the facts of a particular part of pathology, and to arrange a general system, we must not hence conclude that this observer has accomplished nothing, has seen nothing, has established nothing—that his labors and his researches ought to be regarded as naught; and thus make a clean sweep of his teaching. This method of philosophizing in medicine, perhaps a little too common at this day, is convenient and expeditious; but it is neither true nor just. In Syphilography, particularly, it would lead to deplorable errors. A serious study of the history of our art demands more moderation of language, more justice of appreciation. For my part, I am pleased to recognize and to proclaim that, instead of all Syphilographic literature being worthy of contempt, there is to be found in it, by those competent to observe them, interesting and curious observations, sound precepts, and even doctrinal absurdities which some find it good to exhume, while they discredit their source. Of a surety, the long discussions on mercury, guaiacum, sarsaparilla, &c., are not wholly devoid of utility; the history of blennorrhagia may be cleared up by the observations of those who have preceded us. Undoubtedly, the spirit of speculation and charlatanism have left too frequent traces of their existence; but you will often find in them also the indications of a sound judgment, of a veritable scientific tendency, and of laudable efforts to arrive at a systemization and a doctrine. Besides, had these labors no other interest than that of reflecting the ideas and opinions of past times, they would not merit the contempt which has been so unjustly cast upon them.

I would profess the same belief in regard to observers of modern times. Criticism, I know by experience, finds frequent occasions to exercise itself upon their works. But must we hence consider these works unimportant? Far from me be this injurious thought. On the contrary, I hold in great esteem the writings of Bell, of John Hunter, of Swediaur. The time has come to render full justice to the two Culleriers; to M. Lagneau, particularly, whose reputation was justly popular; and to all those intelligent and energetic workmen, by means of whose conscientious studies we have been enabled to advance with increased facility in the path they sought to open for us.

Should I be unjust towards my contemporaries? God forbid, dear friend. Whatever may be our differences, it is with hearty pleasure that I render the most sincere homage to the works of MM. Baumès, Gibert, Cazenave, Cullerier (the nephew), Bottex, Ratier, Puche, Diday, Payan, Venot, in France; to Wallace, Carmichael, Babington, and my pupils Acton and Méric, in England; to Thiry and Herion, in Belgium; and to the remarkable publications of laborious Germany and of ingenious Italy.

I do not then experience, either towards the past or towards the present, any sentiment of injustice or of disdain. You will excuse me for making this declaration very explicitly before entering upon the subject. I think it proper to say that I partake in no manner of the opinion of those exacting and difficult critics, according to whom both ancient and modern Syphilography is only a medley unworthy of attention. I believe, on the contrary, that this branch of pathology is as fertile as any other in useful works and valuable researches.

However, the works of the ancients and moderns have not preserved this part of our science from the general revolutions impressed upon medicine by the physiological doctrine. The school of Broussais, in blotting out the past, had put everything in doubt. Was there a syphilitic virus? Did syphilis exist? You know how physiologism resolved these questions. The most extreme confusion reigned in science, and was transferred to the publications of the time. Doubt was everywhere; certainty nowhere.

It was at this epoch that, surgeon by "concours" to the Central Bureau of the hospitals, it was my lot to enter the *Hôpital du Midi.* I there met an honest and loyal man, a serious and honest practitioner, M. Cullerier, who, having abandoned the family traditions, so to speak, had taken upon himself to doubt his own observations, and appeared no longer to believe what he had seen.

Everywhere doubt had displaced belief. There was doubt concerning the cause of syphilis, doubt concerning its effects, and, as a consequence, doubt in relation to its therapeutics.

And mark! that which was called the new doctrine was enveloped by a great scientific apparatus. M. Richond des Brus wrote an enormous book completely filled with facts; M. Desruelles supported the new idea by statistics which were regarded as exact; all strove to combat the speciality of the disease and the specific nature of the remedy.

History was largely laid under contribution by one of the most learned writers of our century, M. Jourdan, who, in one of the most remarkable works of our epoch, was pleased to take observers, one by one, and to put them in contradiction with themselves; an easy triumph, if the critic, in a rigorous and impartial analysis, does not know how to establish a marked difference between the ideas peculiar to the author, those which result from his researches and his observations, and those which he draws from the scientific media of his time. The first are useful materials, which it is necessary to preserve; the others constitute the prejudices of the epoch, and have only an historical value. Jourdan did not take this precaution; it sufficed him, in order to combat the specific nature of the disease, to indicate the confusion of opinion among our predecessors, and he did it with a luxury of erudition which would have been an ornament to a more healthy criticism.

Such, then, was the state of opinion and of science when I entered the *Hôpital du Midi.* It was necessary to rebuild, according to some, a ruined edifice; according to others, only to consolidate it.

That which was most of all necessary was to investigate the cause of Syphilis.

Had it a special virus? Or, were venereal accidents the result of a common cause?

For this research and this study, *two methods* of observation presented themselves to my mind.

The *first* was the pure and simple observation of phenomena—a method practised by our predecessors, but which had conducted them to such contrary opinions. This method was followed by Devergie, and is not unlike that adopted by Vigaroux, by Blegny, &c., in their report, for example, of the case of three officers, all of whom, having connection with the same young girl affected with a discharge, became infected—the first with a urethritis, the second with a chancre, and the third with warts. It is true that Devergie failed to afford information on one small point, that of the precise state of the young girl, whom he did not examine with the *speculum*.

Evidently, this mode of investigation was worn out, and could conduct only to vagueness and confusion.

The *second method* was more satisfactory to my mind; it was besides more in accordance with the demands of modern science; it seemed to me to open a sure way to the study of the cause of syphilis, and of necessity to lead to incontestable results; I speak of EXPERIMENT.

I laid down for myself the following *conditions:*—

To obtain the syphilitic virus from a known source;

To place it upon a region of the body open to observation;

To note its effects.

You see that these conditions could be fulfilled by experiment alone.

But experiment had already been interrogated, and by it people had arrived at contradictory conclusions. When John Hunter said Yes, Caron, Bru, Jourdan, Devergie, and M. Desruelles said No. What was the basis of affirmations so opposite, when the same method of investigation had been employed? I did not then know; I have since learned. What my reason then told me was, that a series of rigorous and well-conducted experiments must lead to precise results; and the dissensions of experimenters did not dishearten me.

These researches were difficult and delicate. Convictions, and I dare say courage, were needed to undertake them; it was necessary to be certain of clearly appreciating the circumstances under which I was about to act; it was necessary to rely upon previous experiments; it was particularly necessary to rely upon purity of intention, and upon the testimony of the conscience.

I did not, in fact, content myself with the great name of Hunter; with the experimenters cited by Bell; with the work of Hernandez, so honorably recognized by the Academy of Besançon; with the authority of Percy, and other names equally renowned; but I wished to study the question in itself; to place myself in the conditions appropriate to a veritable inventor; in fact, to assume myself alone all the responsibility of the results.

How was it necessary to proceed in this experimentation?

I could inoculate a healthy individual; I could experiment on the patient himself.

The first method of experimenting, that is to say, the inoculation of a healthy individual, it appeared to me, ought always to be rejected by the physician. I do not believe that we have the right to make such experiments. The physician not only ought not to use his natural authority to induce any one to undergo experiments of this nature, but I farther think that he ought to resist the desires of individuals, who, seduced by a generous devotion, would voluntarily expose themselves to the chances of an inoculation. I cast no blame on those who have acted differently. I only repeat that, for my part, I have been unwilling to proceed thus far. There remained the possibility of experimenting upon the patient himself.

Could this present inconvenience and danger to the patient?

In the event of its harmlessness, could it lead to conclusive results?

Here is what history, observation, and experience teach on this subject.

It was generally admitted that a primary contagion did not prevent a second one; and the old saying of *pox following pox* still had full force. At the present day, we know what to understand by this expression.

As to inconvenience and danger, we see every day that primitive accidents are rarely isolated; that they are multiplied with great facility; and that, strictly, the gravity of the disease does not correspond with the number of accidents.

Then, in order to elucidate so grave a question of etiology and of practice, art could, without inconvenience, do what nature habitually does.

A much graver question here presented itself: Are the profound and consecutive accidents dangerous in proportion to the number of the primitive lesions?

Rigorous clinical observation has ever proved, and still proves, that the gravity of constitutional syphilis bears no proportion to the number of primitive accidents, *existing at the same time, and developed at the same period.*

An additional accident, then, adds nothing to the chance of infection, *if we know how to conduct the experiment.*

The question of *surface*—that is, whether an extensive ulceration exposes more to a general infection than an ulceration of moderate size—also remained. Here, again, observation had shown, that a more or less extended surface of a primitive ulceration has no influence upon the production of consecutive accidents. A very small chancre exposes to a general infection, just as much as a very extensive chancre; and, reciprocally, a large ulceration is no more nor less dangerous than a small one.

Finally, there remained the question of the seat of the ulceration, of the place to be selected for experimental punctures. It had been said, and by Boerhaave among others, that venereal accidents, contracted elsewhere than on the genital organs, presented additional gravity; but clinical observation had already proved, and it has since proved to me, that this opinion was erroneous.

I know very well, that on this point a great noise has been made in regard to diseases contracted by physicians and *sages-femmes*, the result of examinations, punctures, &c. There are very good reasons, which I do not wish to give here, why these accidents should have excited attention. I can say, however, without occasioning offence, that members of the profession, who are cognizant of such accidents, have no motive for concealment, while ordinary syphilitic patients have excellent reasons for saying nothing.

I remained, then, convinced that the seat of ulceration could not only have no unfavorable influence upon the production of consecutive accidents, but that it might even diminish or destroy certain troublesome casualties; for example, the production of buboes. Thus, observation had already proved that primitive chancres of the thigh were scarcely ever followed by adenitis; and, in fact, among my numerous experiments, I have never seen an adenitis supervene upon the punctures of inoculation on the thigh.

Then, my dear friend, from history, from clinical observation of all time, from the experimenters who had preceded me, from the testimony of my conscience, severally interrogated, I arrived at this encouraging conclusion, that, in experimenting on the patient himself,

I would in reality give him no more disease;

I would not augment the gravity of the accidents with which he was already affected;

I would not expose him to any additional chances of a consecutive infection.

These capital conditions being found, it was necessary to seek for those which should offer to science and to art every desirable guarantee.

Yours, RICORD.

LETTER II.

MY DEAR FRIEND: I do not write a didactic essay; I would like so to do, but you know I have not at this time the power. I address to you familiar *Letters*, for which I claim all the privileges of the epistolary form; that is to say, liberty of style and spontaneity of thought. Thus, that which I did not say in the preceding letter, I will say without formality in this one; without a too rigid adherence to a plan, to a method, and to the other artifices of composition which are elsewhere so useful.

In order that my first letter should be complete, in a hasty exposition of the attempts made in the way of experiment, I ought not to have omitted to mention the efforts made to transmit syphilis from *man* to *animals*. Either to get rid of the inconveniences which might result from the inoculation of man himself, or to solve the curious problem of the transmission of Syphilis to animals, Hunter and Turnbull had already attempted this inoculation of animals; but without success. I had repeated all of these experiments, and had arrived at the same negative results. However, more recently, a young and laborious *confrère*, M. Auzias-Turenne, has taken up these experiments, and varied them; he has employed other methods than those commonly recognized, and he thinks that he has arrived at the experimental demonstration of the transmissibility of syphilis from man to certain animals. I then recommenced these experiments, and convinced myself anew, that syphilis was not strictly communicable to animals, and that the facts invoked by M. Auzias were illusive. M. Cullerier, at the *Hôpital de Lourcine*, has studied this subject with much care, and has arrived at the same results as myself. My colleague, M. Vidal (de Cassis), has, in turn, experimented, and his conclusions, I believe, do not differ from mine.

Direct observation, then, and experiment on the patient himself, being my only resources, to these I resolved to address myself.

First of all, it was necessary to seek a certain source whence I could derive the cause towards which I wished to direct all of my investigations. There must be no more trusting the statements of the patients. It was likewise necessary to avoid the objections justly brought against the experiments of Hunter and of Harrison, and against the experiments reported by Bell and Hernandez; and to do this I first sought to determine clearly the state of the tissues from which I was about to borrow the reputed specific cause.

In fact, it could no longer suffice me (as Petronius formerly stated), that a woman was reputed to be *tainted;* it was no longer sufficient to take, at random, a morbid secretion coming from the genital organs of a woman, and to make of it, according to the picturesque expression of Alexander Benedictus, *a venereal tincture*, which should spread a uniform color over all the accidents resulting from it. No; the scientific tendencies of my time, and the demands of my own reason, determined me to employ a more decisive method, and more rigorous processes.

I do not wish to lay too much blame upon the facility with which many have drawn their conclusions of the cause from the effect. But who can be otherwise than surprised that, in a question like that of venereal disease, where ignorance *or fraud*, in the words of Hunter, is so frequent a cause of error—in a disease which, after all, is almost invariably a flagrant proof of immorality—even the most judicious observers should frequently place confidence in the dicta of patients, and so unceasingly invoke the moral consideration of testimony?

Testimony! In such a matter, is there anything more deceptive; and particularly in respect to women? Let me cite to you two trifling examples, by which you will see one of the most rigorous observers fall into the snare of a woman's testimony.

Babington wishes to destroy the law laid down by Hunter, that, when there is neither pus nor a puriform secretion, the disease cannot be communicated; so that infection is not possible before the appearance of a gonorrhœa, or after the cicatrization of a chancre. "This conclusion," says Babington, "is not without its dangers, as may be seen from the following facts, which are far from being rare:—

"A married woman was seized with the ordinary symptoms of gonorrhœa, which surprised her very much, as her husband was entirely exempt from disease. However, the husband being questioned, confessed that he had had connection with a suspected woman, about eight days before his wife discovered herself to be diseased; but he positively affirmed that he had had no discharge, nor any morbid sensation, and certainly he then showed no symptoms of the disease. At the end of four days, that is to say, about two weeks after the impure connection, and one week after he must have communicated the disease to his wife, a gonorrhœal discharge manifested itself in him.

"A traveller exposed himself to the chances of a syphilitic infection, and three days afterwards reached home. About four days subsequent to his arrival, his wife was attacked with gonorrhœa; it was only ten days later that he was seized, for the first time, with a discharge and the other symptoms of gonorrhœa." (John Hunter, *Complete Works;* notes by Babington.)

If, in view of such facts, Babington had sought, not to obtain more complete avowals (there are avowals which women never make, even, as I have had only too many occasions to see, under threatening of the gravest dangers), but to assure himself, by a serious examination, of the true state of things, he would assuredly have seen, in these cases, that the infecting cause was not in the genital organs of these candid husbands.

It was, then, no longer possible to think of founding any pathological truth whatever, in regard to syphilis, upon the truth of the testimony of patients; I had no more confidence in doctrines and facts, based upon recitals of this nature.

It was necessary to dispel the mysteries of the closet, in order to expose to the broad light of day the cause which I sought.

Where ought I first to seek this cause? At the very source; that is to say, in the genital organs of the woman—as well in their deepest folds as in their external parts.

Fortune favored me. The *Hôpital du Midi*, at that time, received in its wards the unfortunates sent from the Dispensary.

Here, my dear friend, permit me to remind you that, previous to my entering the *Hôpital du Midi*, the manner of examining a woman consisted in seating her on the edge of a chair, and in separating the external organs of generation; and, if no lesion of tissue was found there, every morbid secretion coming from above the vulva was referred to a blennorrhagic discharge. With respect to chancre, my predecessors appeared to have placed at the vulvar ring the pillars of Hercules.

I could not content myself, nor ought I so to have done, with this incomplete and superficial examination. We were not far removed from the epoch at which M. Récamier had so fortunately exhumed the *speculum* from the surgical repertory. The fine applications of the speculum, made by this celebrated practitioner to the diagnosis of diseases of the uterus, are well known. But this precious instrument had not yet served for the diagnosis of syphilitic diseases; its employment, even in these cases, appeared, and was reputed to be, a contraindication. I paid no attention to this widely-circulated opinion; on the contrary, I extensively employed the instrument upon all the women in the wards.

I do not know whether posterity will partake of the opinion of one of my learned critics, who reduces to a very small sum all that it has been my fortune to accomplish in Syphilopathy. However, my dear friend, when I call to mind the profound obscurities which enveloped the diagnosis of syphilitic diseases before the application of the speculum; when I compare the embarrassments of practitioners of an earlier period in fixing their opinions, with the truly marvellous facilities with which practitioners of our day can make a sure diagnosis; when I remember all the service which the speculum has already rendered to this department of practice, I believe that, if my participation in the progress of our knowledge was limited to this, the above opinion would, even then, be too rigid.

The speculum enables me to examine, with the greatest care, all the surfaces venereally affected, and to verify with precision the state of the tissues which furnish the secretions.

These conditions being established, I sought to study all the modifications reputed as venereal, and to compare them with other morbid secretions.

I commenced with blennorrhagia.

You understand, my dear friend, that I ought to suppose my readers perfectly acquainted with the state of the question concerning blennorrhagia at the time when I undertook my observations. Again I assert I do not here write volumes of a complete history, but a simple and rapid exposition of facts observed by myself.

I sought to solve by experiment this problem, already solved in various ways by the observations with which you are acquainted.

Does blennorrhagia recognize a specific cause? Hunter had learned that the pus of chancre, when inoculated, produces chancre. I said, if blennorrhagia recognizes a specific cause, the muco-pus which it secretes will, undoubtedly, when inoculated, produce phenomena similar to those produced by the inoculation of chancrous pus.

But, in order to get accurate results; in order to isolate the question from every complication, and to remove every cause of error, I ought, first of all, to inoculate the muco-pus coming from perfectly simple blennorrhagia to take this muco-pus from surfaces perfectly free from every ulceration. You thus see how precious to me was the employment of the speculum; without it, these experiments were impossible.

Now, these experiments, made in great number, and a long time continued, first conducted me to this fundamental result, which I present in the form of a *proposition*:—

EVERY TIME THAT THE MUCO-PUS HAS BEEN TAKEN FROM A

NON-ULCERATED MUCOUS MEMBRANE, THE RESULTS OF INOCULATION HAVE BEEN NEGATIVE.

Every experimenter who has followed me in this path has arrived at the same conclusion, and that, too, whatever may have been the duration of the blennorrhagia when the experiment was made.

Consequently it was with great surprise that I read in your journal the following passage from M. Vidal, who, in his *Letters on Syphilitic Inoculations*, reproaches inoculation with having usually remained powerless, so far as blennorrhagia is concerned. "In fact," says my learned colleague, "a distinguished pupil, M. Bigot, has attempted, under the eyes of M. Puche, Physician to the *Hopital du Midi*, sixty-eight inoculations with urethral muco-pus, and these sixty-eight inoculations have been without any kind of result!" I am astonished at the surprise of M. Vidal; these sixty-eight negative inoculations are entirely conformable to the facts which I had previously advanced; they confirm and corroborate my opinion on the rarity of *syphilitic* blennorrhagia; and when my contradictor asks you "Do you believe that, among these sixty-eight blennorrhagia, there were none with virus, none that bore the germ of syphilis?" reply to him boldly, No; and precisely because the inoculation was negative.

A dialectician so skilful, a logician so rigid as M. Vidal, cannot avoid acknowledging that the results of experiment, on whatever subject exercised, are either positive or negative, and that, scientifically, the negative results have no less value than the positive. The inoculation of the vaccine virus gives rise to no phenomenon in those subjects who have already had the smallpox; is this negative result without importance and without consequence?

But, in kind, we shall soon perceive what degree of value and of force belongs to these negative results, when compared with the positive results of inoculation. I will mention, in passing, an objection, which will hereafter receive a complete refutation. Writers on syphilis have thought, with Hunter, that blennorrhagia was a form of syphilis peculiar to the mucous membrane. I will confine myself, at present, to the remark, that the experiments previously indicated completely destroy this opinion. We shall hereafter see that the pus of chancre, when applied to a mucous membrane, produces a chancre with facility.

From the experiments which have been indicated, I draw this *conclusion:* THE BLENNORRHAGIA WHOSE INOCULATED MUCO-PUS GIVES RISE TO NO RESULT DOES NOT RECOGNIZE THE SYPHILITIC VIRUS FOR A CAUSE.

This conclusion, you are aware, has raised many and grave objections. But I fear you cannot, to-day, give me enough space to commence my exposition and refutation. This will form, if you please, the subject of the third letter.

Yours, RICORD.

LETTER III.

MY DEAR FRIEND: The conclusion with which I closed my last letter—that *blennorrhagia, the inoculated muco-pus of which gives rise to no result, does not recognize for its cause the syphilitic virus*—deduced from irrefragable facts, places the history of blennorrhagia at the point whence it was transmitted to us by the *Leviticus.* As old as man—yes, older than he, for animals created before him are subject to blennorrhagia, while they do not have syphilis—this disease, in its state of simplicity, has nothing in common with the syphilitic infection.

In spite of those who, since the time of Paracelsus, Bethencourt, and Fallopius, have wished to make a new disease of the blennorrhagia non-symptomatic of chancre, a disease identical with syphilis, the researches which I have made, corroborating the accurate descriptions of Alexander Benedictus and of Cataneus, have given to the doctrines of Balfour, of Tode, and of Duncan, the value and solidity which Bell himself would have given them, had he been able, like us, to explain the pretended exceptional facts.

But does blennorrhagia—as I understand it, absolutely foreign to syphilis in its causes, in its form, in its consequences—depend upon a particular virus?

It would not be at all improper to admit a special cause, having the specific power constantly to produce blennorrhagia and its consequences. In fact, nothing is better adapted to induce a blennorrhagia than the muco-pus furnished by certain inflamed premucous membranes.

But when we go back in the most rigorous manner, and with the severest criticism, to the determining causes of the best characterized blennorrhagia, we are forced to acknowledge that a blennorrhagic virus is most usually wanting. Nothing is more common than to find women who have communicated blennorrhagias the most intense, the most persistent, leading to *blennorrhagic* consequences the most varied, and of the gravest character, who were only affected with uterine catarrhs, which sometimes were scarcely purulent. In other cases, the menstrual flux seems to have been the only cause of the communicated disease. Finally, in a great number of cases we find nothing at all, or only simple changes in diet; fatigues; excesses in sexual connection; the use of certain drinks—beer; of certain food—asparagus. From this arises that frequency of belief on the part of patients, a belief very often legitimate, that they owe their clap to a perfectly healthy woman.

On this point I assuredly know all the causes of error, and I have the pretension to say that no one, more than myself, holds himself on his guard against frauds of every kind, scattered in the path of the observer; but it is with knowledge of the cause that I advance this proposition: *Women frequently give blennorrhagia, without having it.* Blennorrhagia, such as some persist in understanding it (that is to say, as a consequence of contagion), is as rare in woman as it is frequent in man. I do not think I go too far in saying that women give twenty claps for one which they receive. And this is easily understood; for women, so subject to discharges from the genital organs, which one cannot attribute to syphilitic causes, are the most frequent source of those discharges in men which can be attributed to contagion.

It is impossible for me to regard as serious the doctrine of my learned colleague, M. Cazenave, who acknowledges that many women, under the influence of chronic utero-vaginal catarrhs, can have sexual relations without communicating infection, provided they are not heated to boiling point; provided they are not raised, so to speak, to a virulent red-heat.

Is it not more simple to understand, and more rational to say, that with a less degree of excitation the secretions are less irritant, and that custom can produce an immunity from these secretions in some persons—as it were, by a kind of acclimation?

It is thus, as I have frequently seen, that a married woman may cohabit with her husband without communicating anything to him; but let a lover supervene, and the latter contracts a blennorrhagia. The husband was acclimated; the lover was not.

When one studies blennorrhagia without prejudice, without preconceived ideas, he is forced to confess, that it is frequently produced under the influence of most of the causes which determine inflammations of other mucous membranes.

The experience of Swediaur is at hand to prove this. This observer injected a volatile alkali into the urethra, and it produced a blennorrhagia. Does this experiment prove that we can always and at will produce blennorrhagia by irritating injections? No; any more than one can always produce a coryza by the same means, an ophthalmia, &c. For blennorrhagia, as for any other inflammation, there is required a pre-existing predisposition—that immense unknown which governs all pathology. This is proved by the circumstance that blennorrhagia is not always contracted under the same conditions in which it is most evidently communicable. Without this happy immunity which the absence of the predisposition gives, blennorrhagia, already very common, would be still more so.

An experience of twenty years has taught me, and allows me to affirm, that, excepting blennorrhoidal discharges, symptomatic of chancre, it is often wholly impossible to recognize the cause of a discharge.

I am aware that many of my colleagues obstinately refuse to admit this opinion. They view every blennorrhagia in relation to syphilis; and their therapeutical prescriptions are only the logical consequence of this preoccupation of mind.

Here, my dear friend, I ought to make you a confession, and

I will make it publicly. This persistence of some of my honored and learned colleagues in always considering and treating blennorrhagia as an accident of a syphilitic nature has many times moved me. Thus it has frequently happened to me, not to satisfy a frivolous curiosity, much less to yield to a culpable incitement to aspersion, but to enlighten and assure my understanding; frequently, I say, it has happened to me to have recourse to a stratagem, of which I wish to make the avowal with all the reserve and propriety which I owe to the honorable *confrères*.

It has occurred under the following circumstances: A man presented himself at my consultation, with one of the best characterized blennorrhagias. He declared to me that he had only had connection with one woman, and that this woman was his wife, or his mistress. This man was alarmed and disquieted. He brought with him the woman from whom he had contracted his disease, and she, protesting her innocence, in concert with the patient, supplicated me to submit her to the most rigorous examination. This examination, made with all the rigor and attention of which I am capable, showed me the sexual organs of this woman to be perfectly healthy. There was nothing, absolutely nothing, in the deepest folds of these organs, which could explain the blennorrhagia of the man. I requested the woman to step into another room, and, alone with the patient, I exhausted all possible means, the details of which I will spare you, to arrive at this certainty. The patient had had connection with this woman alone; it was only in this intercourse that he could have contracted his disease.

I reassured the husband, or lover; I exculpated the wife, or mistress; but then I asked them to become accomplices of the little stratagem which I am about to indicate.

I sent them both (separately, of course) to this or that learned colleague, whom I knew to be in absolute opposition to me on the question of blennorrhagia. I said to the patient: Ask this question distinctly, "Is my blennorrhagia syphilitic?" I said to the woman: Ask boldly, "Can I give blennorrhagia to a man?"

The couple returns; the man with a diagnosis thus arranged:

Syphilitic blennorrhagia; follow the treatment *ad hoc.* The woman had this indication: *The perfectly healthy state of the genital organs permits me to declare that madam cannot have communicated a disease with which she is not herself affected.*

It is not a unique and isolated fact which I cite to you, my dear friend. This experiment I have renewed frequently, and often enough to corroborate my convictions, and to assure my conscience.*

What do these facts signify? That the cause of blennorrhagia cannot always be known; that this malady may be produced by the causes common to all inflammations, provided there be a predisposition to it; but that the most special agent of blennorrhagia is the muco-pus furnished by the inflamed genito-urinary mucous membranes.

This view of the case seems to me more rational, much more philosophical, than that which would associate the blennorrhagia, called venereal, with a kind of demivirus conceived by our very learned *confrère* and skilful syphilographist, M. Baumés. According to him, blennorrhagia is, as it were, a degenerescence of chancre; it may give rise to a constitutional syphilitic infection, more feeble, however, than that produced by chancre, but still without power, by contagion or inoculation, to produce the latter. "We can then predict," adds M. Baumés, "the greatest resemblance between the constitutional symptoms which are the result of each of these diseases; and, in fact, experience proves that the difference between these symptoms lies, not in their

---

* There are facts still more extraordinary than these, relative to blennorrhagia contracted from healthy women. Here is one, the analogue of which, perhaps, is not presented by M. Ricord, and of the authenticity of which it is impossible to raise the least doubt:—

A young man, aged thirty, a physician, lived in chastity for more than six weeks, and his last sexual relations had not been suspicious. Chance allowed him to pass nearly a whole day alone with a young lady whom he loved. From ten in the morning until seven in the evening, he vainly endeavored to vanquish the resistance of this woman, and during all this time he was in an uninterrupted state of excitement. Three days subsequently, he was seized with a most painful and violent blennorrhagia, which lasted forty days.

Assuredly, this is the type of a non-syphilitic blennorrhagia.—*Note by* FRENCH EDITOR.

nature, but only in their degree of intensity, in their gravity, and in their seat, which, in the case of blennorrhagia, usually extends to fewer tissues, to less numerous organs, than in that of chancre." (Baumés, *A Theoretical and Practical Treatise on Venereal Diseases*, vol. i. p. 259.)

This is veritably a doctrine of the golden mean. This pure theory is justified neither by facts, nor observation, nor experience; it lacks only one condition—proof.

Thus far, then, and this is really my opinion, simple blennorrhagia is completely distinct from syphilis, so far as relates to the causes which produce it.

But, by way of objection, it is said the pus of chancre can produce blennorrhagia. This opinion is very ancient; it has been maintained since the first appearance of the pox in England, and very beautifully can it be maintained now. But what does the statement imply? Do we depend on the observations of the ancients? These are incomplete and insufficient; it is impossible with them to proceed scientifically from cause to effect. Would one make experiments similar to those of Harrison, who believed in the production of blennorrhagia from the introduction into the urethra of the pus furnished by a chancre, without knowing what it had physically produced? No! but more simply and more logically, we will conclude on the possibility of the production of a non-virulent blennorrhagia by the pus of a chancre, in considering this pus, as has been done before me, as acting in the manner of a simple irritant. A woman having chancre at the inoculable period may thus determine in a man a blennorrhagia which will not inoculate. We may thus explain the observations of Swediaur and others, supposing they made no error in diagnosis, seeing that they used neither the speculum nor inoculation; observations proving that men, affected with chancres, have communicated blennorrhagia to women.

Here is what clinical observation teaches, and what experimentation can demonstrate. It is not rare to see patients who, at first affected with a chancre of the glans or prepuce, are successively seized with a balanitis or a balano-posthitis determined by the irritating action of the pus of the chancre. But then, while the chancre furnishes inoculable pus, the balano-posthitis does not. (Hereafter we shall see that, in order that the pus of chancre may act specifically, conditions are necessary which are not always present.)

Adhering, then, to my first conclusion, and reducing to their just value these primary objections, I affirm that, when Harrison produced blennorrhagia with the pus of chancres, this pus either acted in the manner of simple irritants, or it produced a urethral chancre; this fact he did not verify. In the same way we shall see that, when Hunter produced a chancre with the pretended pus of a blennorrhagia, it was with the product of a veritable urethral chancre that he had to deal.

But if inoculation has proved that the cause, or causes of blennorrhagia, *whatever be its seat* in the two sexes, differs from the specific cause, from the virus which *infallibly* produces chancre, the consequences of blennorrhagia ought always, then, to differ from those of chancre; and yet how many cases of constitutional syphilis are attributed to blennorrhagia!

These are questions, my dear friend, which will form the subject of my next letter. We shall then see if it be possible to establish a differential diagnosis between affections which some wish systematically to confound.

You will first permit me to say a word on the incubation of blennorrhagia.

Yours, RICORD.

LETTER IV.

MY DEAR FRIEND: As I promised, I am about to say a few words to you concerning the incubation of blennorrhagia.

Incubation has been made a condition of virulence. Every virulent malady must present a period of incubation. Thus, those who admit that blennorrhagia is the product of a virus equally admit that this virus only produces its primary effects after a period of incubation of greater or less duration.

I say of greater or less duration, and this not without reason.

For the incubation of blennorrhagia, as well as for that of syphilis properly so called, authors have admitted a period which one can no longer conveniently determine. Its term has been fixed between several hours (Hunter and others), and fifty days and upwards (Bell). Here is certainly a very elastic contagion.

You know that this is far from being the case in virulent diseases, where incubation is incontestable. The limits of the period of incubation may be more clearly fixed in smallpox, in kinepox, in scarlatina, in measles, in hydrophobia. The instructive work of M. Aubert-Roche has even apprised us of the definite limit of the incubation of the plague, which never exceeds eight days. With respect to blennorrhagia, the case is different, as we are soon to see. There are here no certain limits.

What is this incubation of blennorrhagia, which I have been forced, even recently, to deny? We must understand it; it is a pure question of words. I do not deny that, between the action of the cause and the appearance of the first symptoms of blennorrhagia, there elapses a longer or a shorter period; but is this an incubation properly so called? an incubation similar to that of variola or vaccinia? I contest the fact; and I explain the longer or shorter period which elapses between the action of the cause and the appearance of the phenomena, by the condition, by the particular susceptibility of the tissues which have been exposed to the influence of the cause. There is no more incubation than there is between the action of cold on the feet and the appearance of a coryza. A person does not have a discharge of muco-pus from the nose immediately after the application of cold to the feet; a certain time passes between the two acts. Do you call this time the incubation of the coryza? Why, then, use a similar expression in relation to blennorrhagia?

In those cases in which the blennorrhagia only appears a long time subsequent to the exposure to the presumed cause, is it not more rational to admit an unknown cause: a cause other than this pretended incubation, which nothing explains and which nothing authenticates? Is it not thus in nearly all inflammations? Is it possible always to arrive at the direct cause of a pneumonia, or of an arthritis, of a phlegmon?

Undoubtedly, in man, the most powerful cause of blennorrhagia is sexual intercourse; but we should fall into strange errors if we attempted to refer all blennorrhagias to a virulent cause. I could cite you some very singular examples which prove the contrary; but I refer the reader to the interesting note with which you accompanied my preceding letter.

From this exclusive manner of considering the etiology of blennorrhagia, there often results, in practice, a singular method of interpreting facts. A man, affected with blennorrhagia, has had connection with several women; he hastens to make a kind of moral choice between them, and by elimination it frequently happens that the most innocent one is hit upon. This application of the law of suspicions has given rise to singular errors, of which I have often been the witness.

Hence, we conclude that the effects of blennorrhagia may be separated from the cause which produces them, but that there is no proof that the time which elapses between the action of the cause and the appearance of the morbid phenomena, is the result of a true virulent incubation.

I will not, my dear friend, be too unfaithful to my programme; but still, how is it possible not to enter upon some questions when they force themselves immediately on your notice? Such is the case with the specific seat of blennorrhagia. This seat, you know, has been much tormented. In man, it has been made to travel from behind, forwards; from before, backwards; to advance, to retire, at the will of the fruitful imagination of syphilographists. From the spermatic ducts, passing successively by the glands of Cowper, the fossa navicularis and the follicles of Morgagni, the seat of blennorrhagia has journeyed extensively. It is true that Bell, by establishing different degrees of blennorrhagia, caused its seat to retrograde. But it is not with these well-known questions that I wish to entertain you. I would, however, mention a strange preoccupation of Hunter. This great observer, you are aware, admitted a virulent blennorrhagia identical with chancre; he placed its seat in the fossa navicularis; but he asks whether this inflammation, which may be propagated step by step towards the posterior portions of the urethra, continues virulent beyond the fossa navicularis! It must be confessed that the genius of Hunter permitted itself to be singularly governed by the spirit of system. Besides, in studying Hunter, we see his observing genius continually struggling with his theory of blennorrhagia. He is a victim of a false idea. Facts come incessantly to demonstrate this to him; but the theory is there to bind his intelligence, and, in place of uncloaking his theory by the facts, he seeks to make the facts agree with his theory. An illustrious example of the dangers of preconceived ideas in the culture of the experimental sciences.

In woman, Graff placed the seat of virulent blennorrhagia in the follicles which lie in the neighborhood of the urethra. Moulinié, of Bordeaux, one of our brotherhood, some years deceased, thought he saw in the vulvar glands, so well described by Bartholin, something like an organ of virulence, in a blennorrhagic point of view.

Amidst all these opinions, rigorous observation has shown that such portions of the mucous membranes as are most exposed are the most easily affected. Nevertheless, we must acknowledge that the urethral mucous membrane, in both sexes, is more frequently diseased, after sexual intercourse, than the other mucous membranes of the genital organs. This fact is an argument in favor of the partisans of virulent contagion. I will corroborate it by this proposition, which seems to me to be incontestable—that a woman affected with a urethral blennorrhagia may, generally, be considered to have contracted it from a man also affected with blennorrhagia. And this proposition, you see, may be important in legal medicine. Thus, for my part, I would be inclined to admit that a woman, in whom I found a urethral blennorrhagia, contracted the disease from a man. But does this fact furnish any support to the idea of the existence of a virulent contagion? No; for I explain it by this other fact, perfectly true and incontestable—that the pus furnished by the urethra is the most irritating of all pus with respect to certain mucous membranes.

While some syphilographists contest the existence of urethral blennorrhagia in women, others admit the existence of the disease in her only so far as the urethra is its seat. These two extreme opinions are erroneous. Observation has led me to admit every variety of blennorrhagia on all the mucous membranes.

At this point, will you allow me to get rid of some other questions incidental to blennorrhagia? Henceforth I shall proceed more freely and more rapidly with respect to the great questions which remain to be treated.

If I examine the lesions of tissue produced by blennorrhagia, whatever be the mucous membrane affected, I find nothing which simple inflammation may not produce. Sometimes the part presents a light erythematous condition, without secretion. This is the *dry gonorrhœa* of some authors, a ridiculous and absurd designation; in view of which one cannot help admiring the persevering efforts of M. Piorry to effect a reform in nomenclature. Sometimes it is a mucous, catarrhal element, and all its products, with which we have to do. Finally, there occur real phlegmonous complications, from which result in man the chordee blennorrhagia, and the tolerably frequent production of abscesses along the tract of the urethra.

But neither in the state of the tissues, nor in the nature of the products, do we find anything which can be compared with the accidents of syphilis, properly so called.

Are the consequences of blennorrhagia comparable to those of syphilis? This has been asserted, but not proved. Some analogy undoubtedly exists between the two, but what notable differences!

Thus, one of the first accidents which blennorrhagia may induce, and which resembles one of those produced by syphilis, is bubo. But, first, *adenites* are infinitely more rare as a result of blennorrhagia than as a result of chancre. Bubo is never met with in blennorrhagia, unless where the disease affects the urethra in either sex; the other varieties never occasioning adenitis. I am well aware that a physician of Belgium speaks of *peri-auricular buboes*, which are manifested in ocular blennorrhagias; but I confess that of these I am yet to meet an example. Finally, blennorrhagic bubo possesses this peculiar feature; it is frankly inflammatory; it has but little tendency to suppuration; and when this does happen, *the pus is never inoculable.*

Would you proceed to ascertain what blennorrhagia may produce in common in the two sexes? There is the ophthalmic blennorrhagia, which is never manifested except during urethral blennorrhagia. In fact, is it possible, without confounding everything, to establish the least similarity between this ophthalmia and syphilitic *iritis?*

Here is blennorrhagic rheumatism; is it rational to establish the least analogy between this affection and those produced by syphilis upon the osseous system? Is there anything in the world, for example, more dissimilar than blennorrhagic arthritis and syphilitic exostosis?

Of cutaneous affections, what shall I say but this—that I am exceedingly astonished that physicians, well acquainted with affections of the skin, have sought to discover a resemblance between the cutaneous affections produced by certain remedies employed in the treatment of blennorrhagia, and the very special skin affections produced by syphilis? The preoccupation of a false doctrine has here produced strange confusion. It has been said that blennorrhagia, like chancre, produces cutaneous affections; and some have cited, as examples, the roseolæ which succeed the use of cubebs and copaiba. I assure you that such roseolæ only appear when these resins are administered. To this, some reply: But they only appear when there is blennorrhagia. I answer, that we only give cubebs and copaiba when there exists a blennorrhagia. I may add—and this is important—that I have administered copaiba in catarrh of the bladder, and have seen the exanthemata supervene.

But these resinous exanthemata have such marked characteristics that it is impossible, how favorable soever the inclination, to confound them with true syphilitic exanthemata. They are usually very quickly developed, and are animated. They exhibit either the rubeolic form, or that of the lichen. They are not very confluent. They readily group themselves in the neighborhood of the articulations, and in the sense of extension—the wrist, the elbow, the knee, the ankle, and around the ears. They are usually accompanied with much itching, which is not the case with syphilides; and, what is more important than all, we can say of them, *sublatâ causâ tollitur effectus;* in fact, they rarely survive more than one week the cause which produced them.

The mention of these syphilitic exanthemata recalls to my mind a curious fact, which I ask your permission to relate in the form of an episode. This fact conveys instruction:—

Two or three years since, one of our most distinguished young *confrères* came to me in fright. "Up to this time," said he, "I have had confidence in your doctrine; but I find it at fault, and in my own case. This is painful."

Saying this, he took off his clothes, and, raising his shirt, said, "What is this?"—showing me his breast and back.

I examined him, and replied, "It is a fine syphilitic roseola." "Syphilitic, you say? Are you sure of it?" "Perfectly sure." "Very well! you condemn yourself. I have never in my life had any venereal symptoms, excepting a blennorrhagia, and that was twelve years since." "Are you, in your turn, sure of this?" "As of my existence."

I examined him from head to foot, and the examination completed, I said to him, gravely and solemnly: "*Confrère*, you have *recently* had a chancre upon the right hand, and this chancre was seated neither on the thumb nor on the index finger, but on one of the last three fingers." "You joke!" So little was I in joke, that I added, "You still have a bubo." And in fact I placed his finger upon an epitrochlean ganglion still engorged.

Then, interrogating his memory, he told me that, in fact, some months before, he had attended a woman affected with chancres, which he himself had dressed; that an ulceration supervened upon the middle finger, to which he paid no attention; and that this ulceration cicatrized. "There is the source of your roseola," I said; and acted accordingly.

Finally, what physician at this day can confound blennorrhagic epididymitis with syphilitic sarcocele? This was no longer possible even in the time of Bell; still less is it possible subsequent to the labors of Sir Astley Cooper, and those which I myself have made on this subject.

You will allow me to pass over in silence the pretended tubercular diathesis, invented in Germany as a consequence of blennorrhagic virulence. The question of tubercles in general is already sufficiently obscure, without rendering it more so.

You see, dear friend, that I finally approach the programme which I have marked out. In my next letter, I will resolutely enter upon it.

Yours, RICORD.

LETTER V.

MY DEAR FRIEND: I promised to enter, to-day, upon the great questions raised by the study of blennorrhagia; I am about to try to do honor to this serious engagement; serious in reality; for, as I wish to demonstrate, the point I am about to discuss may be considered as the keystone of the syphilographic edifice.

All that I have heretofore said upon blennorrhagia refers to *simple* blennorrhagia, considered not as the product of a particular virus, but as a virus completely foreign to that which produces syphilis properly so called.

Nevertheless, according to a great number of authors, this blennorrhagia may produce consecutive accidents perfectly identical with those produced by chancre.

It is incontestable that a great number of patients affected with constitutional syphilis accuse as antecedents only a blennorrhagia.

These patients are sometimes right. I do not deny the fact. But, after establishing it, I do not bind myself to leaving it in the rough state; to exclaim with emphasis, "It is a fact," and to thrust it with intolerance against every explanation.

The whole question may be reduced to these terms: When blennorrhagia is the starting-point of a constitutional syphilis, does it not involve something else than what we have previously studied in blennorrhagia properly so called?

Experiment has proved—and pathological anatomy has confirmed the fact—that the urethra and the deep and concealed points of the other genital mucous membranes may be the seat of chancre, the necessary source of syphilitic accidents.

It was from ignorance of the urethral chancre that the doctrines of Balfour, of Tode, of Bell, and the immense scaffold built upon the experiments of Hernandez, have necessarily crumbled.

The doctrine of the existence of urethral or concealed chancre being granted, virulent blennorrhagia can no longer be put in doubt. It is identical with chancre; it is chancre itself.

This idea is not new in science; and I am astonished that the seekers after priority have cast no reflections upon me in this respect. Ulcerations of the urethra have, for a long time, been recognized. Mayerne, in the seventeenth century, already attributed urethral blennorrhagia to the pus produced by intra-urethral ulcers, and gave it the name of πυβοια. Many others besides, whom I do not wish to name, have noticed the presence of urethral ulcerations. But do you not consider it curious that Swediaur, who maintains the identity of blennorrhagia and of chancre, should formally state that virulent blennorrhagia cannot be denied when ulcerations exist in the urethra?

If, in the autopsies of the three men who were hung while they had blennorrhagia, Hunter did not discover the presence of ulcerations in the urethra; if, in the autopsy related by M. Philippe Boyer, as well as in other autopsies, nothing has been found, it is because these cases were only simple blennorrhagias. I have shown, at the Academy of Medicine, two specimens of pathological anatomy, the designs of which may be found in the *Clinique Iconographique* of the Venereal Hospital, and on which MM. Cullerier and Lagneau were directed to report. In these specimens existed, at different depths, urethral chancres; chancres which had been recognized by inoculation previous to death.

Thus, inoculation first, and subsequently pathological anatomy have proved incontestably the existence of urethral chancres; and truly, the fact is denied by no one, even among those who would ascribe syphilitic consequences to simple blennorrhagia. Concealed urethral chancre is not then an hypothesis; it is a fact as carefully established as any other medical fact.

And yet—singular phenomenon!—those who have most closely studied chancre of the urethra; who, like M. Baumes, have been able to recognize it at the depth of an inch in the canal, take greater pleasure in flinging themselves into the field of hypothesis, than in admitting what their observation and good sense naturally indicate. Witness, for example, M. Baumes, among others. He establishes, with rare sagacity, the difference which exists between chancre and blennorrhagia; he traces with clearness their differential characters; and arrives at the end of his parallel by inferring the identity of the two accidents.

The same struggle, dear friend, is ever going on between the logic of facts and preconceived ideas, the results of which I showed you in the case of the great Hunter. Quite recently, I have seen still farther indications of this fact in an otherwise interesting pamphlet of M. Lafont-Gouzy.

But here are presented serious objections.

The existence of urethral chancre being admitted, cannot, it is said, explain all the cases of constitutional syphilis which seem to have blennorrhagia as their starting-point.

The number of urethral chancres, it is added, is too few relatively to that of constitutional poxes with blennorrhagic antecedents.

Finally, there were blennorrhagias in which it was impossible to discover urethral chancre, and which have been followed by constitutional accidents.

I am about to astonish my antagonists by making this enormous concession: All this is true. But you will see that this concession is only apparent; for I make haste to add: The explanations which have been given of the facts are not true.

It is very certain that, relatively to the immense number of blennorrhagias which exist, the blennorrhagia symptomatic of concealed chancre constitutes the exception. So that it has been remarked, with an appearance of reason, How does it happen, then, that the number of poxes supervening upon pretended concealed chancres should be almost proportionate to those supervening upon external chancre?

Here, my dear friend, I demand all your attention, not because I wish to be subtle or captious, but because the form of reasoning which I am forced to employ in order to reply to this objection, itself very subtle and very captious, needs to be pursued in all its phases. Yes, concealed urethral chancre is rare; but the number of poxes following concealed urethral chancre is not few.

You are about to cry out against the sophism. Hear me.

Concealed urethral chancre is rare; that is incontestable; my experiments, those of my honorable colleague and friend, M. Puche, those of many other observers, have proved the fact beyond doubt. Do you wish me to establish a proportion? I am willing to do so. Let us admit that it is, one in a thousand, which is much beyond the reality, I am deeply convinced.

Let us assume, then, one concealed urethral chancre to one thousand cases of clap.

Remember, on the other hand, how common and how disseminated is blennorrhagia. Remember that Lisfranc, with a little exaggeration, perhaps, said that in one thousand male adults he counted eight hundred who either had had, who already had, or who would have the clap.

However this may be, in one thousand blennorrhagias there are nine hundred and ninety-nine of which you hear nothing farther, and which will have no unpleasant result, against one alone which will have produced a constitutional infection.

This proportion is small, without doubt; but operate on hundreds of thousands, on entire populations—on the population of Paris, for example, which numbers three or four hundred thousand male adults; compute the number of blennorrhagias contracted in this immense city; deduct from them for the chancrous *larvé* only the small proportion of one in one thousand; and the result will exhibit a well-sustained number of blennorrhagias, which have been able to produce consecutively the pox.

Well! what occurs in practice? That you only see in the hospitals, or at the consultation of physicians, those patients whose syphilitic infection is due to a blennorrhagia with a chancrous *larvé*. A physician of a special hospital may meet, in the course of his practice, ten, twenty, thirty examples of this kind. What is this relative to the number of simple blennorrhagias which occasion no bad result? But those patients who give no antecedent to their constitutional infection other than a blennorrhagia arrest the attention of observers; their memory remains profoundly impressed; the number of cases, though relatively small, swells in the imagination; and we do not fail to see it presented as a formidable objection to the doctrine of the non-identity of blennorrhagia and syphilis.

You see to what this objection is reduced; I trust I have destroyed it. I have been accused of instituting an hypothesis relative to chancrous *larvé;* of establishing a system. The fact I have proved by pathological anatomy, have deduced from my experiments on inoculation. Is not blennorrhagia, in the immense majority of cases, exempt from all syphilitic consequences? To what, then, attribute the infection, when it supervenes upon blennorrhagia? I myself attribute it to the chancrous *larvé* which inoculation led me to discover, of which pathological anatomy has demonstrated the existence. And to what do my adversaries attribute it? To a pretended identity, to which daily observation and an immense number of facts continually give the lie. And yet they accuse me of being the slave of system, of elevating a doctrine on the basis of observation, of experiment, and of cadaveric inspection. Who, then, are my opponents, whose only support of their doctrine rests upon a rough fact, the interpretation of which is based on none of the elements which, at the present day, the exigencies of science demand?

Deign, then, to believe, dear friend, that it is my opponents who rush into hypothesis, while I, on the contrary, seek to conduct them into the path of truth. You now see how easy it is to reconcile the two terms of my proposition: that concealed urethral chancre is rare; but that the number of poxes resulting from concealed urethral chancre is not few.

It does not appear rare, because we see again only those patients who have had this chancrous *larvé;* but if we could establish a rigorous proportion between the blennorrhagias which are followed, and those which are not followed by syphilitic accidents, we would see that the latter are very rare, and that this apparent frequency is only illusory.

Moreover, in every case in which a constitutional syphilis has been attributed to a blennorrhagia, have all possible precautions been taken to prevent error? I think not, when the observer contents himself with the diagnosis of the patient, and with the history related by himself. One would almost say that the physician had declined his office. You will see striking examples of this confidence of the physician, in the memoirs and writings of MM. Martius and Cazenave, and in the thesis, otherwise so excellent, of M. Legendre.

Yet to how many causes of error are the recitals of patients liable? Blennorrhagia is ordinarily a painful accident, extremely uncomfortable, and one which leaves burning reminiscences in the minds of those who have experienced it. When you interrogate patients relative to their preliminary history, it is always of their blennorrhagia that they first speak. They do not suspect the importance of a chancre which, when it infects, is usually indolent; suppurates little; has little tendency to extend; and frequently cicatrizes of itself. Of this accident they rarely make mention; or if, by a pressing interrogatory, you cause them to remember it, they will tell you it was a flying chancre, a simple excoriation. I may well be allowed to recall the circumstance that it is only since the period of my labors that blennorrhagia, as regards accidents of constitutional syphilis, has been subjected to a more precise and rigorous method on the part of physicians. By pursuing the course I have traced, it will be seen that the great number of urethral blennorrhagias which furnish uninoculable pus are not followed by constitutional accidents.

Among other statistics, I would cite the most recent; those compiled last year by M. Lafont-Gouzy, who, in three hundred and eighty cases of inoculated blennorrhagias, found only two in which the inoculation gave specific results. Four months afterwards one of these two presented symptoms of constitutional syphilis.

In this work of M. Lafont-Gouzy, mention is made of two cases in which inoculation gave no result, but which were nevertheless followed by syphilitic accidents. We shall hereafter explain these exceptional cases.

M. Baumes cites five cases of individuals in whom the inoculation of the blennorrhagic muco-pus having failed, constitutional syphilis subsequently appeared; and from these facts our honorable colleague draws the conclusion that blennorrhagia, non-symptomatic of chancre, may, like chancre, produce syphilitic infection. *

But, first, are all poxes which are attributed to blennorrhagia really the consequence of it, even admitting the necessity in these cases of the urethral chancre? If care is not taken relative to the manner in which statistics are compiled, it will be found, as M. Cazenave and others have observed, that blennorrhagia is the most frequent antecedent of constitutional syphilis, because it is really rare to find individuals who have not had one or several blennorrhagias. But when, recognizing the value of chancre as the necessary antecedent, we institute an examination relative to its frequency, even in the works of authors whose appreciation of it leaves so much to desire—M. Cazenave, for example—we find that in seventy-two observations, blennorrhagia only existed alone or with buboes eighteen times, while chancre was found thirty-eight times; whence M. Cazenave very logically concludes, as you see, that blennorrhagia is the most frequent antecedent of syphilis. The same results, and just as logical a conclusion, were deduced from the observations of M. Legendre.

As an acquisition to science, and as a confirmation of my opinion, remains the fact, thus far, that even from the statistics of my antagonists, chancre, clearly recognized and acknowledged by the patient, is the most frequent forerunner of syphilis.

My wards at the *Hôpital du Midi* contain, at the present time, sixty-one clearly identified cases of constitutional syphilis, submitted to specific treatment; in all, without exception, was chancre an antecedent.

Now with respect to the cases in which we cannot go back, either by interrogation or through the recollection of the patient, to the pre-existence of an external chancre, what reason is there for absolutely denying, at least in a certain number of cases, the fact of such pre-existence?

* One of the five patients of M. Baumes had previously had chancre. To this chancre, then, must we attribute the pox of the patient.

You see, then, what we must think of the following opinion of M. Cazenave: "Thus, while blennorrhagia does not always give rise to secondary symptoms, it would seem to determine them more frequently than chancre."

You know, dear friend, for it is in your journal that the circumstance is recorded, that this opinion of M. Cazenave has been warmly approved. M. Vidal (de Cassis) has thus expressed his opinion of M. Cazenave, who is not, he says, an academical authority, but has the advantage of being an authority of a special kind:—

"The position of M. Cazenave, the vast theatre of his observations, and his taste for statistics and for all the methods of investigation which, according to my adversaries, lead to certainty, are known. Well! M. Cazenave *has succeeded in proving* that the symptom, whose virulent character is rarely proved by experiment, ought to be the very symptom which observation shows to be the most virulent and infective!"

It is true that, to prevent M. Cazenave from too hastily felicitating himself on this warm approbation, M. Vidal adds on the following page:—

"Nevertheless, I would not dare to go so far as M. Cazenave, who, in my opinion, places too many syphilides to the account of blennorrhagia. Blennorrhagia, in my opinion still, is a disease much more contagious than infectious."

That is precisely my opinion, Monsieur Vidal, you well know; only allow me to be astonished that it should be upheld by you, who believe that M. Cazenave *has succeeded in proving* the contrary. I do not wish to follow up this flagrant contradiction, which, after all, is, perhaps, only a conciliatory criticism.

As to the blennorrhagias, the inoculated muco-pus of which has given no result, and which have been followed by general infection, the observations which have been reported leave much to desire, and are—I ask pardon of my learned *confrère* of Lyons—covered by a bill of exceptions. The astonishing credulity, the truly blind confidence of some physicians, although rendering their works very respectable, are far from carrying conviction to all minds. In these particular cases, I would place but little reliance on the symptomatology of constitutional accidents, which is incomplete relative to some important points to which I must return. I wish that in these cases the disease be veritable constitutional syphilis. I admit that the appearance of these syphilitic accidents corresponds as to time with the occurrence of the blennorrhagia; but can we be sure, from this fact alone, that the patients had had nothing but a blennorrhagia—that syphilis could not have entered at some other door? My *confrère* of Lyons has somewhere said that I denied the possibility of a constitutional infection as a result of simple blennorrhagia, because I had never met with an example of it. On the contrary, it is because I have often had the opportunity of seeing patients in whom physicians, who think differently from me, have recognized only a simple blennorrhagia where I have found another door for the entrance of syphilis;—it is because of this, I say, that my convictions have become more and more strengthened.

When those who seek to show that a simple blennorrhagia should give rise to syphilis have told you that the patients presented ulcerations neither on the fingers nor on the genital organs, they think they have proved all it is necessary to prove. They forget the innumerable ports of entry which the surface of the body presents; secret, deceptive doors, which close almost as soon as they have opened, of which the patients have no knowledge, or which they have an interest in concealing. How many students have come to me from other hospitals of Paris, in whom nothing but a blennorrhagia had been found, and in whom I afterwards found chancres in peculiar situations! In this connection, I present a history, the analogues of which are frequently met in my practice:—

A lady came to consult me for a disease of the rectum; the symptoms of which she complained were those of fissure. Upon examination, I found nothing at the anus; but the finger, introduced into the intestine, enabled me to recognize, opposite the internal sphincter, a fissure situated anteriorly, and reposing upon a hardened base. I proposed to operate; she refused. I then submitted her to injections of rhatany. Her treatment had lasted scarcely fifteen days, when I perceived an exanthematic eruption, having all the characters of a confluent syphilitic roseola. Pushing the examination further, I recog nized the engorgement of the posterior cervical ganglia. The patient experienced nocturnal cephalalgia, and some crusts had already began to be developed in the scalp. No doubt could remain as to the nature of the accidents. I then examined the genital organs, but could find nothing there but a very simple uterine catarrh. I interrogated her as to the circumstances in which she had been placed relative to the contagion of syphilis, and she confessed to me that her husband was diseased; that he had ulcerations upon the penis; and that, from fear of communicating them to her, he had connection with her *à preposterâ venere*. Then the nature of the deep fissure was unveiled.

In this case, is it not true that, without the painful accidents determined by the fissure, ulceration would have passed unnoticed? It would then have happened that the only antecedent would have been a simple uterine catarrh.

But there exist still other causes of error which I must mention. This will be the object of my next letter

Yours, RICORD.

LETTER VI.

MY DEAR FRIEND: Let us continue the exhibition of facts and arguments which have been opposed to my doctrine.

There is an observer about whose works my opponents make much ado; and these works are, in fact, worthy of the greatest esteem. I have noticed them honorably in my preceding letter, and you see me disposed to accord to them all the value they deserve. This observer, whose conclusions have constantly been opposed to mine, is C. Martins. Well! how far do the results at which M. Martins has arrived elucidate the great question concerning the consequences of blennorrhagia as a cause of syphilis. Mark! it is precisely on account of his closeness of observation, of his scientific method, of his statistics in short, that the conclusions of this author have excited so much attention. What, then, do his statistics show? I find them very favorable to my doctrines. Do I flatter myself in this? You shall be the judge.

M. Martins gives an account of sixty cases of syphilis. Now how many times did he note chancre as an antecedent? Forty-six times, my dear friend. M. Martins assures us that in fourteen cases only he met with no other antecedent than simple blennorrhagia, two of which had bubo, and two orchitis. But he adds that he did not diagnose the blennorrhagias, but was obliged to accept the testimony of the patients. You know what I think on this point. Undoubtedly here is testimony which we ought to believe; but I will ever contend that, when the question concerns a diagnosis so difficult as that of urethral chancre, the testimony of persons wholly strangers to the art, who are often unlearned and narrow-minded, who understand neither the importance nor the bearing of a question, is of very little value. Without doubt, the testimony of such persons is received in graver matters, in questions of life and death; but it does not follow that this testimony is always true, nor the judgments based upon it always equitable.

Allow me to offer a general remark, which is appropriate in this place. In many of the observations of M. Martins, as in many of those of M. Cazenave, and in nearly all of those of a great number of authors, you find in their summary these words: *Several primitive accidents.* These primitive accidents, which have consecutively brought on constitutional syphilis, are chancre and blennorrhagia. If my opponents, from whatever reasonable motives, attributed the consecutive infection rather to the blennorrhagia than to the chancre, we might examine the doctrine. But no; you know the fact—you have read it—and you ought not to have been the least surprised at it; it is *en masse* that they group these primitive accidents, without regard to the time which elapses between the appearance of the one and the other. To all they give the same value, and to all they attach the same results. In truth, does this indicate accurate science and rigorous observation? What would you think of a physician who should say to you: There is a man with hydrophobia; he has been bitten ten times—three years, two years, one year, six months ago, and again quite recently. But his hydrophobia evidently depends upon the successive inoculations which he has undergone. Or, here is a variolous patient who has with impunity passed through five or six epidemics of smallpox; at the last the disease was developed; but this result was due only to successive contagions and infections.

I confess that I do not thus understand science. I am astonished that so critical a mind as that of M. Martins, who acknowledges with myself that blennorrhagia is most usually due to causes wholly foreign to syphilis; who is logically forced to admit that blennorrhagic antecedents, as causes of syphilis, are extremely rare, and that chancre is, consequently, the most frequent antecedent of pox—I am astonished, I say, that M. Martins, in arriving at the conclusion that a simple blennorrhagia can occasion syphilis, should be satisfied with the three observations of which he has made choice out of sixty, and especially with the one which I here present:—

"An apothecary, aged twenty-three, contracts a blennorrhagia; but it incommodes him so little that he continues to pursue his occupations; he hunts; and even cohabits with women. An orchitis supervenes which requires treatment; the blennorrhagia is cured after six months. Seven years afterwards, an ulceration appears at the opening of the left nares, another on the internal surface of the lower lip. These ulcerations extend; the whole of the left half of the two lips is affected; afterwards they partially heal, in order to ulcerate at other points; the ulcerations are rounded, and the edges perpendicular; the cicatrices exhibit a thin, rosy skin, which is in folds. The patient, admitted to the wards of M. Biett, was cured in one month by the use of the proto-iodide of mercury. Shall we say that this patient, partly a physician, whom we saw scrupulously examine himself at the hospital, had chancres without knowing the fact?"

Yes, certainly, will I say that this patient had very well characterized chancres, from the description given of the case by M. Martins, who had neither seen nor recognized them, undoubtedly because of the abnormal seat which they occupied. As to the mode of contagion, M. Martins will not ask information of me, nor will I take it upon myself to indicate it. Besides, he knows as well as I how these accidents may supervene, without supposing any trickery in the practice of the profession, even of this fine apothecary.

You know, my dear *confrère*, that chancres with a strange seat, and difficult to be discovered, are less rare than has been thought. I cited you one example in my last letter. There are several others:—

Some years since, M. Lusterman, professor at Val-de-Grace, brought to me a lawyer, on whose lower eyelid, at the inner angle of the eye, was a hard, resistant, elastic tumor, with a red, granulated surface, tending towards cicatrization. This tumor had already been seen by several *confrères;* and, if my memory is faithful, men specially devoted to ophthalmology had been consulted; but its nature was still unknown. I was consulted to know whether it was dependent upon any more or less distant syphilitic antecedent. Prosecuting my examination farther than my *confrères*, I found the peri-auricular ganglia engorged, resistant, indolent, as well as those of the parotidean and sub-maxillary regions. The posterior cervical ganglia were themselves already tumefied. The surface of the body was covered with exanthematous spots, belonging to the best characterized syphilitic roseolæ. Lenticular dark red spots, leaving in some points, under the pressure of the finger, a tawny yellow spot. Absence of fever and itching.

To the great astonishment of M. Lusterman, my diagnosis was this:—

Indurated chancre of the inner angle of the eye (successive engorgement of the peri-auricular, parotidean, and sub-maxillary ganglia); secondary cervical adenopathy; syphilitic roseolæ; precocious secondary accidents.

To the great astonishment of the patient, I said to him: "It is at most two or three months since you carried to your eye the contagious matter which inoculated you with syphilis."

Upon recovering from his surprise, the patient replied: "In fact, I recollect that, while sleeping with a woman, and after certain touchings, I was seized with a vivid itching of the eye. I raised my hand to it and rubbed it some time. It is since that time, in fact, that my eyelid became affected."

Is it not true that if this gentleman had been affected with a blennorrhagia as antecedent or concomitant, both the chancre of the eye and the secondary accidents would have been referred to it? Very well! if I must say it, I believe that the nose of the apothecary of M. Martins was very probably in the same condition as the eye of our lawyer.

M. Cazenave ought to remember the history (it only dates from 1847) of a young and very intelligent medical student, in whom he diagnosed a constitutional syphilis *d'emblée*, characterized by a roseola without antecedents. This young man presented himself at the clinique of the *Hôpital du Midi*, and there we verified, before all the students, the existence of a clearly marked indurated chancre, situated on the left cheek, and concealed in a very thick tuft of whiskers. The sub-maxillary ganglia—irreproachable witnesses—were engorged and indolent; with that character of resistance peculiar to adenopathies symptomatic of indurated chancre. This ulceration, to which the patient had attached no importance, being revealed to him, he was able to fix its origin and date, which agreed perfectly with the appearance of the secondary accidents.

At the same time there was a patient, in the wards of the hospital, having a chancre (primitive accident) upon the sinciput. I showed to my clinique a woman who had an indurated chancre upon the left eyelid, with symptomatic engorgement of the peri-auricular ganglia, preceding by two months a nocturnal cephalalgia, the engorgement of the posterior cervical ganglia, and a roseola.

I would not finish were I to indicate merely all the cases, which have passed under my eyes, of chancres situated in unwonted places, and liable to be confounded by the careless observer with secondary accidents attributed to a more or less remote blennorrhagia. I have at this moment indeed, in the first ward of the hospital, a patient affected at once with a simple urethral blennorrhagia (negative inoculation), and with an indurated chancre of the lower lip, accompanied by indolent engorgement of the submaxillary glands; concomitant affections, but independent the one of the other.

I have adduced enough, it seems to me, to prove to you how frequent and insidious are the causes of error in this matter, as well as to establish the propriety of my skepticism relative to certain observations.

But I ought not to forget that my learned *confrère* of Lyons is in readiness with five observations which he opposes to my doctrine. I ought the more especially to notice these observations, since they have sufficed to convince the cautious mind of M. Legendre.

First, as I have already told you, one of these observations is to be eliminated; for the patient, who was the subject of it, had previously had chancres. There remain, then, four cases of *simple* blennorrhagia followed by syphilis. But of the remaining four cases I will still eliminate two, for in the latter M. Baumes did not inoculate; these cases, then, must enter into the large category of those blennorrhagias in which there has not been a rigorous diagnosis. One remarkable fact, which you will allow me to notice while passing, is that M. Baumes, who says he had inoculated the greater part of the patients presented to him, should have fallen precisely upon two syphilitic blennorrhagias, for the diagnosis of which he deprived himself of the precious assistance of inoculation. We are then reduced to two cases in which inoculation was practised with a negative result, and which were, nevertheless, followed by constitutional accidents!

In one of these cases, a question arises relative to a nose, which seems to me excessively suspicious. Here is the history of it as reported by M. Baumes:—

"Of the two patients who were inoculated, one remained at Antiquaille two months. His blennorrhagia was difficult to cure; he even had a whitish discharge when he left the hospital. He entered, three months afterwards, with a syphilide in red and coppery patches, partly furfuraceous and partly squamous, and a round ulcer with a grayish bottom, edges perpendicular, and the circumference erysipelatous, in the left nostril. At this time there was no discharge. The patient had had no new coition since he had been discharged."

This, you observe, is a very complete description of the primitive ulcer; but how does it happen that, in view of so important a fact regarding a contested question, M. Baumes did not attempt the inoculation of this chancre? I deeply regret this circumstance; but, in the absence of all rigorous diagnosis, I must place this nose in the category of the apothecary's.

Here, then, I am face to face with the last observation of M. Baumes. My learned *confrère* says that he inoculated from the seventh to the tenth day from the appearance of the discharge; but how long a time had elapsed from the infecting coition? M. Baumes knows perfectly well that this knowledge is not unimportant. He also knows, as well as myself, that the chancre, which is ordinarily followed by secondary accidents, usually spreads but little; that it is perfectly indolent; that its suppuration is so feeble that it may escape unperceived. Upon all this, I am sure, M. Baumes is as well informed as myself. These ulcerations in nowise hinder the production of a blennorrhagia at a subsequent period; and it is not astonishing, then, that this should give inoculable pus, inasmuch as the chancre had reached the period of reparation, or had completely disappeared. It is necessary, moreover, to suppose that, before he entered the hospital, or after his discharge, the patient had not experienced another contagion, and that in a way which eluded the sagacity of our *confrère.*

All these objections apply with equal force against the observation of M. Lafont-Gouzy, in which secondary accidents came on after a blennorrhagia which was inoculated without result. Nothing is said of the length of time which elapsed between the coition and the manifestation of the symptoms; a period which may have afforded opportunity for the cicatrization or reparation of the chancre.

Therefore, it seems to me that the proposition which my *confrère* of Lyons attempts to sustain, that *simple* blennorrhagia can occasion the same accidents as chancre, justifies me in retorting on him his address to me, to wit: "He assumes, as granted, the question in dispute, and advances an hypothesis unsupported by a solid basis."

Thus crumble, one by one, the apparently grave objections made to my doctrine. Accordingly, I still believe with Girtanner, "that syphilis, most commonly caused by chancres and buboes; and that it but very rarely succeeds a discharge." With Swediaur, "that the symptoms of syphilis rarely manifest themselves after blennorrhagias." With M. Rayer, "that cutaneous eruptions secondary to blennorrhagias are rare; that they are observed in much smaller proportion as a consequence of blennorrhagia than as a result of venereal ulcerations, both superficial and deep." The correctness of these opinions, as may be seen, is clearly supported by the rarity of concealed chancres compared with blennorrhoidal symptoms.

I could invoke many other authorities. But I have not finished the list of objections it is my purpose to examine. In my next letter, I will examine some of another nature.

Yours, RICORD.

LETTER VII.

MY DEAR FRIEND: From the mere circumstance that chancres have been submitted to a treatment called *methodical*, some writers have attributed to a blennorrhagia which has subsequently supervened the consecutive accidents which are the legitimate consequence of the constitutional affection. One of the five observations of M. Baumes is assumed to prove this hypothesis. But what is a methodical treatment? What is the treatment you can absolutely depend on to neutralize the syphilitic diathesis, so that it shall not return? For my part I know of none that is infallible. I know that a great number of very distinguished practitioners think that with a certain dose of mercury, administered during a determined period, one may consider the patient as radically cured. And, not to leave the bounds of my hospital, I will cite my very honorable colleague, M. Vidal, who has recently published the statement that, with one hundred and ten pills of Dupuytren, neither more nor less, the pox can be completely overcome.

Believe me, I am the most tolerant man in the world; no one respects, more than I, the religion of others. But I have the right, it seems to me, not to share in all their convictions, when

I daily see proof of the grave errors to which a blind faith may lead. M. Vidal must have seen many patients return; and if he has not witnessed this circumstance, will he allow me to say that I have seen a large number of patients myself, who have taken not only the one hundred and ten sacramental pills, but even one hundred and twenty, one hundred and fifty and more, without enjoying immunity from the reappearance of the accidents?

I will not insist any further upon this point; for I shall have occasion to return to it hereafter. All I wish to establish here is, that physicians have often deceived themselves when they have attributed to a blennorrhagia supervening on a chancre, accidents peculiar to constitutional syphilis, by reason of the simple fact that the chancre referred to was submitted to a mercurial treatment. I shall now, my dear friend, call your attention to a point which will excite your astonishment, which will take you unawares, and put your logic at fault.

My opponents have established several categories of poxes, according to their source!

Thus they admit—and in this they are perfectly right—that constitutional syphilis may be transmitted hereditarily.

They assure us, and they furnish pretended proofs, that constitutional syphilis may occur *d'emblée.*

They assure us, and they publish facts in support of the assumption, that sometimes we find no kind of antecedent to a constitutional syphilis; without, however, daring to attribute it to a syphilis *d'emblée.*

They pretend that an individual under the influence of the syphilitic diathesis, without actual manifestation, without apparent symptoms of the disease, may nevertheless, under certain circumstances, transmit syphilis.

They wish the duration of the incubation of syphilis to be considered unlimited. They assert that the contagion can be manifested as well after several days as after several months, or years—even twenty, thirty years, and more.

All these distinctions and categories you will find especially urged in the writings of M. Cazenave; but upon what grounds? This question I ask myself in vain. I seek to know by what process, by what means of diagnosis we are able to attribute a constitutional syphilis with which a patient is affected, to any one of these circumstances more than to another.

Has hereditary syphilis, after early infancy—and its effects may be prolonged, as we shall hereafter see—a special symptomatology?

Is constitutional syphilis *d'emblée* distinguished from other kinds by some pathognomonic symptom?

Do those cases, in which no antecedents have been found, give rise to disorders different from those of other cases?

What is a pox without antecedents, and which nevertheless is not *d'emblée?*

Finally, do we find that those cases which have supervened on a *simple* blennorrhagia, exhibit less severe forms, or less extended seats than others? M. Baumes has endeavored to establish this assumption in his book, but has been unable to confirm it by his practice.

I boldly answer No to all of these questions. Constitutional syphilis presents the same symptomatology in all cases, and it is not I, but my opponents themselves, who prove it. Read the following pages, and see if you can find, in the descriptions given by MM. Cazenave, Baumes, &c., a single characteristic trait which justifies these arbitrary distinctions.

Moreover, one thing in my opponents astonishes me. How does it happen that, in these cases of constitutional syphilis, or *d'emblée*, or without antecedents, when it has been impossible for them to assure themselves of the conditions of the contagion, to determine when and how it was contracted; when it has been proved to them that the patient presented no primitive accident; when they have found no door of entrance for the pox; when they have been perfectly convinced that the patient had not deceived himself—that he had no interest in deceiving; when, finally, they have been certain that they were not deceived themselves;—how does it happen, I say, that they do not admit the hypothesis of M. Cullerier, in order to explain the inexplicable cases—that is to say, spontaneous syphilis in man?

This great step was taken by M. Richond des Brus. Among other facts which led him to this conviction, he cites one which is very curious. A young man and a young girl gave themselves up to the pleasure of love. In his ardor, the young man excoriated himself with a hair of his mistress. He did not stop for so trifling a matter, and he ended in communicating his excoriation to his mistress. But the amorous couple were soon seized simultaneously with constitutional syphilis. M. des Brus, who examined neither the young man nor the young girl, none the less admits antecedent good health; but, unable to explain the pox, he declares it to be spontaneous.

I have not yet gone so far as the learned *confrère;* and the frequent occasions I have of witnessing constitutional accidents follow well-determined primitive accidents, induce me to rank the few exceptional cases, in which the patient does not know or will not tell me the facts in the case, and those in which I arrive too late to find the door of entrance of the syphilis, in the category of observations entitled by M. Cazenave *unknown antecedents*, and which I call BADLY KNOWN.

In the name of all that is proper, is it not more satisfactory to the mind, more conformable with our method of reasoning in medicine, to admit, with respect to the cases in which the syphilis has really succeeded a blennorrhagia non-symptomatic of chancre, that the antecedent has not been *recognized*, rather than to lose one's self in a crowd of subtle distinctions, of arbitrary categories, and of sterile explanations? How, besides, can my opponents prove to me their *ipse dixit*, how convince me of error? It is not my custom to defy any one; and farther, such a mode of argument ought to be banished from scientific discussions; but, I would really like him to prove to me, in a scientific manner, once, but once for all, that, in a matter with respect to which all my researches have been vain, he can substitute for the formula ANTECEDENTS BADLY KNOWN, something more affirmative.

From this long discussion, my dear friend, it will doubtless appear to you that I am justified in concluding:—

That where, in the great majority of cases, blennorrhagia is simple and benign, there also exists a virulent blennorrhagia;

That a blennorrhagia is virulent where there exists a concealed chancre.

Now, is there any means of diagnosing a concealed chancre? Is it possible to distinguish a simple blennorrhagia from one connected with a concealed chancre? Here is the great question; I shall attempt to solve it.

Some persons have attached but little importance to the diagnosis of blennorrhagia. Hecker, and several others who have followed in his footsteps, have thought this diagnosis unnecessary. Quite recently, I read in your estimable journal that it had only a relative importance. A certain number of physicians have held opinions concerning this matter which must surprise the popular mind.

Have you contracted your blennorrhagia from another woman than your own?

Virulent blennorrhagia.

For the lover the blennorrhagia is virulent, but for the husband it is benign.

You have contracted a blennorrhagia, and you intend to remain single:

Simple treatment.

But wish to marry:

Antisyphilitic treatment.

Therefore, the position of the bachelor, or of the future husband, causes the blennorrhagia to pass from the benign to the malignant state.

On so important and grave a question, I do not wish to insist upon the ridiculous side of these contradictions.

The whole world has felt the necessity of a more rigorous diagnosis. The most recent of my opponents, M. Vidal himself, with whom my methods of diagnosis have not found favor, has made some attempts to supply the desideratum. In the first edition of his *Treatise on External Pathology*, he gave the hope that it would be possible to distinguish a virulent from a benign discharge by the *odor* emanating from it. It appears—and the fact is to be regretted—that these hopes are not realized, for this passage has disappeared from the second edition.

I adhere more closely to my ideas than M. Vidal does to his. Will you permit me, then, once more to explain both my ideas and my experiments relative to the diagnosis of blennorrhagia, and to examine the objections which have been made to them?

But I cannot treat the subject in the short space which remains to me, being unwilling to-day to abuse the generous hospitality my letters have received.

This point will comprise the subject of my next letter.

Yours, RICORD.

LETTER VIII.

MY DEAR FRIEND: We must inquire to-day, agreeably to promise, whether it is possible to distinguish a simple blennorrhagia from a blennorrhagia with an urethral chancre.

You see I propose the question as boldly as my opponents.

In the study of this diagnosis, it is important to establish two conditions: in the first place, a diagnosis absolute, univocal, irrefragable; in the second, a rational diagnosis.

The absolute diagnosis can only be obtained by artificial inoculation. Whenever the muco-pus furnished by a mucous membrane gives the characteristic pustule, which we shall soon examine in studying chancre, it can be affirmed, whatever may have been the duration of the disease, that this is virulent, that a chancre exists somewhere; the chancre alone being able to give rise to positive results from inoculation. Here is an incontestable fact, established by my researches. Here is the absolute and univocal diagnosis in its utmost rigor.

When you obtain, by inoculation of the urethral muco-pus, the characteristic pustule, assert boldly, and without possible error, "It is a virulent blennorrhagia."

But require of inoculation, as of all other means of investigation, only what you have a right to expect. You must have variolous or vaccine virus to produce the effects of variola or vaccina. If by the side of a pustule of variola or vaccina an abscess is developed, and you should take the pus of this abscess for inoculation, you would no longer obtain the specific effects of the variola or vaccina. Take nasal muco-pus by the side of a variolous pustule developed on the Schneiderian membrane, and this pus will not produce the effects of variolous pus. If, then, your patient is actually affected with an urethral chancre, and at the same time with a simple blennorrhagia (a frequent complication); and if, in place of taking the pus of the chancre, you should inoculate the blennorrhagic pus, the result would be necessarily negative. There is no need of much talent to comprehend so simple a matter; and I am astonished that M. Vidal, who has much talent, should make this circumstance the basis of an objection against inoculation. I esteem his ability too highly to admit that he can believe the pus furnished by an urethral chancre, which coexists with blennorrhagia, to be necessarily mixed with the whole blennorrhagic pus; or that one drop of chancrous pus, acting like leaven, must necessarily render the other virulent. The complexity of the morbid elements undoubtedly often renders the analysis difficult; but an exact knowledge of each of these elements enables us to distinguish between them.

The urethral chancre, which never exhibits a very large surface, can furnish only a very small quantity of virulent pus. Even the secretion of the indurated chancre is sometimes almost null, and ordinarily it is insufficient to stain the linen of the patient. Every time, therefore, that you have to deal with a very abundant discharge, you will be right in supposing that it is connected with something else than urethral chancre. We must be careful, then, about drawing conclusions from the negative results of the inoculation relative to the absence of urethral chancre.

But, if the inoculation is many times repeated; if, especially, care has been taken to express the urethral secretion in order to obtain more immediately the product of the ulcerated surfaces, and the results have always been negative, there is a very great probability that the blennorrhagia is a simple one, without chancrous complication. Undoubtedly the diagnosis is here neither absolute nor complete; but does it not at least offer something more than the diagnosis which is usually made?

Farther, to draw a conclusion from the negative results of the inoculation, it is necessary to take into account the time at which the experiment is made. We will hereafter see, in studying chancre, that the virulent secretion has a term, and that there is a moment when the ulcer, passing to the state of simple ulceration, ceases to furnish specific pus. If, then, the experiment is made too late, less can be concluded from the negative result than though the inoculation had been made from the first to the second week succeeding the infecting coition.

In this point of view, does not inoculation offer all that the most rigorous mind requires? If the results are positive, it gives you the most absolute sign that diagnosis can furnish. If, therefore, the results are negative, they lead to a rational diagnosis of which they may form one of the most valuable elements. Let any one find, in human pathology, a surer and more fruitful diagnostical sign.

What! is not that a sign of supreme importance, which, when it exists invariably, assures us of the existence of a lesion attended with the gravest consequences; and which, when it does not exist, may lead us, with a sort of certainty, to a rational diagnosis?

And because this sign has also its uncertainties, shall we pay no regard to the circumstances in which it presents a mathematical value and precision? Are we then so rich in absolute diagnosis, that we ought to exhibit indifference, skepticism, or derision with respect to a sign, the existence of which smooths away so many difficulties? In legal medicine, what other means than inoculation will permit us to show positively that a blennorrhagia is symptomatic or not of chancre?

But, I am asked, is inoculation always applicable? Is the time to test its value always at our service? Can we, and ought we, on all occasions, to depend upon it? Is it necessary, in every instance, to have recourse to it? Certainly not. This I have stated and repeated a hundred times in my lectures; and it is incredible that I should again be called to account for objections which I have a hundred times made to myself. Inoculation, since I must repeat the statement, is an excellent means of diagnosis, but one of which we are frequently deprived. Is this a reason for renouncing the attempt to seek the methods of distinguishing between simple and virulent blennorrhagia? Undoubtedly not; and, fortunately, a minute and accurate study of all the elements of the disease gives, in the great majority of cases, whatever my opponents may say to the contrary, a diagnosis sufficiently valuable for the purpose of indicating the prognosis, and furnishing indications of a truly methodical treatment.

In fact, as we shall hereafter see, the mere presence of a primitive ulcer is not all that suffices to excite fear of a constitutional syphilis, and to require us to enforce a mercurial treatment. Other conditions exist which are usually sufficiently marked to enable us to recognize them.

Allow me, then, very briefly, to pass in review the ordinary elements of the diagnosis of blennorrhagia, to which we made a slight allusion when speaking on the subject of etiology.

You remember what I said concerning women considered as *foci* of infection, and the degree of value to be attached to this source of information in drawing conclusions relative to the virulence or simplicity of the blennorrhagia. On this point patients manifest a strange *naïveté*, and a singular idea of morality. How many times have young men entered my cabinet, and said to me with confidence: "The blennorrhagia with which I am affected must be benign, for I contracted it from a married woman. She is the wife of one of my friends, and I am very sure that this can be only a discharge." To this I am accustomed to reply: "Monsieur, if your wife had a lover, would you consider her a very honest woman?" This question troubles nearly all of them; and, moreover, they very soon see that, in order to establish my diagnosis, I have recourse to means a little more certain than those based on the morality of the persons from whom the affection was derived.

I have said that a perfectly healthy woman may be the focus of infection. Among the singular facts which have come under my observation, allow me to relate to you the following, which has its moral, as you will see:—

A young couple had invited a friend of the husband to breakfast. The repast was almost terminated, and the appetite was not satisfied. It was decided to add a morsel of cheese to the festival. The husband quits the table, descends the four flights of stairs, and runs to the neighboring grocer's to seek the complement of the amicable feast. Alas! he did not return soon enough. During his short absence his faithless half committed adultery with his perfidious friend. The husband returns; the repast is finished; they take coffee and its adjuvants; the friend retires, and the worthy husband in his turn consummates the conjugal act.

Three days afterwards the husband came to me with a urethral chancre, attended with blennorrhoidal symptoms. He was accompanied by his wife, and he affirmed that he had had intercourse with no other woman than his wife. The most attentive examination of the genital organs of this woman enabled me to discover nothing suspicious. My prescription being made, the couple departed. I was then left without any solution of the virulent blennorrhagia of this man.

But the next day the woman returned. She came to inquire whether I was perfectly sure that she had no disease. I examined her again, and again I affirmed that she was perfectly sound. She then related to me the history I have just given you. She added that she was accompanied by the delinquent, whom she begged me to examine. In him I found a magnificent chancre, at the specific period, upon the corona glandis.

This fact confirms the curious experiments made at Lourcine by my young and learned colleague, M. Cullerier. He has placed virulent pus in the vagina; has allowed it to remain a considerable time; has taken it upon a lancet; has inoculated it with positive results, and the vagina, submitted to injections, alone has remained intact.

You will conclude, with me, my dear friend, that the source whence a knowledge of the cause of a blennorrhagia is derived, can give no great certainty to the diagnosis.

I will not return to what I said concerning incubation as a means of diagnosis. Urethral chancre is sometimes developed very quickly, and may furnish pus in a short time. So that, far from considering as virulent the blennorrhagia which it has taken the longest time to appear, it is the contrary which must often be admitted as the fact.

Some have made *violence* a synonym of *virulence;* but the reverse is the truth. As a general rule, it is the least violent, the least painful blennorrhagia, which should inspire the most dread of urethral chancre.

The duration of the discharge is a sign not to be neglected. In general, it is not those discharges which are most tenacious that cause us to fear urethral chancre.

The nature of the secretion may prove of great value when we know how to appreciate it. The secretion which is the result of ulceration of the urethra is much less mucous than purulent; it is usually sanious, rusty, and charged with blood. The least pressure upon the urethra renders these characteristics very obvious. But to accord to this sign (the presence of blood) all its value, we must first be certain that the patient has not used a caustic injection; that no foreign body has been introduced into the urethra; that there has been no rupture of the canal in chordee; and especially that the sanguinolent matter has not been expelled with the last drops of urine, for in the last case the result would depend upon inflammation of the neck of the bladder with vesical tenesmus.

I will not speak of the value of the urethral speculum in the diagnosis of ulcerations of this canal. It affords an ingenious method of diagnosis, but it has not proved so valuable as it was thought to be. It enables us sometimes to distinguish a chancre, even when situated at a considerable depth in the canal, through the facility it furnishes us of separating the lips of the urethra.

Wedkind thought he found in the engorgement of the follicles in the neighborhood of the urethra, near the frænum, a sign of virulence; but these engorgements are usually only phlegmonous, and independent of other complication.

The most important sign consists in the engorgements exhibited in the course of the urethra, particularly in the balanic region, the most common seat of the urethral chancre.

I have already stated, it is not important so much to detect the presence of an ulceration, either by the aspect and nature of the secretion, or by inoculation, as it is to know if we have to deal with an ulceration capable of producing the syphilitic infection. This it is which all authors have had in view when they have spoken of virulent blennorrhagia.

Well! as you will soon see, it is the indurated chancre which is the fatal antecedent of constitutional syphilis. Now, nothing is usually easier than to diagnose an indurated urethral chancre with blennorrhoidal symptoms.

If there exists no blennorrhagic complication, the patients scarcely suffer during the emission of urine. The jet of urine is commonly diminished and twisted, on account of the diminution in the size of the canal. The erections are not painful when the chancre is in the balanic regions.

To clearly determine the presence of these ulcerations, it is necessary to explore the urethra by means of pressure from above downwards, from the dorsal to the inferior face. In practising this manœuvre, we feel a more or less extended cord, which some syphilographists have designated the *balanic cord.* It is easy to determine, in the greater number of cases, the side of the canal on which the ulceration is seated. Independently of the well-defined indurations on one side, you see the affected side form a convexity, while the healthy side, in yielding, forms a crescent. When the pressure is made from side to side, the induration ceases to be appreciable.

Undoubtedly, some engorgements of the balanic region, or of the follicles along the course of the canal, may be only the result of simple inflammation, without virulence. In these cases we must, in order to complete the diagnosis, have recourse to the accessories.

Then, too, engorgements of the glands are rare in blennorrhagia non-symptomatic of chancre. When they do occur, as I have already indicated, they are acute, and terminate readily by resolution; or, when they suppurate, they furnish simple pus.

In the urethral chancre, inflammations of the dorsal lymphatics and glands are much more common. If the chancre is non-indurated, they suppurate almost fatally; and when opened, they present incontestable characters of virulence. In the indurated urethral chancre, the adenopathies are fatal; several ganglia are seized at once; they remain indolent, and do not suppurate; conditions to which I shall hereafter recur.

Finally, if all these conditions have not been appreciated; if these signs have not been understood, either because we have been called to the case too late, or because we have misunderstood them, we may be certain that, if the patient was affected with blennorrhagia symptomatic of chancre, six months will not elapse without the supervention of accidents, if the constitutional infection has taken place.

We must next examine whether, in the last analysis, it is not better to wait, in order to make a late diagnosis, the expiration of this time, than to prescribe, during the same period, a mercurial treatment which, after all, does not give us certainty.

Yours, RICORD.

LETTER IX.

MY DEAR FRIEND: To-day it is my desire to bring to a close my remarks on blennorrhagia, by a few observations as to its treatment. You will understand that, in these letters, details would be idle and useless. I confine myself, in the questions which arise, to general points, the development of which will form the subject of a special and extended treatise, which I hope to be able to offer hereafter to the judgment of my brethren. In this work, I shall touch upon all the doctrines of the *Hôpital du Midi;* and I will close the subject of blennorrhagia by some considerations upon its treatment.

On witnessing the pertinacity with which certain syphilographists cling to old ideas concerning blennorrhagia, recognizing and admitting those only which are virulent, it would seem as though they at once subjected every discharge which they meet to mercurial treatment. Well! This is not the fact. Most of them are content with a rational treatment; and M. Vidal ranks himself among this number. His course is the same as my own, with a difference perhaps in my favor; for in what he has written on blennorrhagia, establishing nowhere an absolute differential diagnosis between virulent and benign blennorrhagia, he in no way speaks of an antisyphilitic treatment properly so called. Examine his *Treatise on External Pathology*, and you will be astonished, like myself, at finding my colleague adopt so mild a course of treatment, when his views relative to the virulence of blennorrhagia are considered.

I have already alluded to the extraordinary and ridiculous custom of giving copaiba and cubebs for the blennorrhagia of bachelors, and of reserving mercury for every one that would marry. This two-sided therapeutics reminds me of the history of one of my old colleagues at the *Hôpital du Midi.* He had, in his youth, like many others, contracted blennorrhagias. At a later period he was about to espouse the daughter of an old syphilographist, who was a believer in the doctrine of precaution. He obtained the hand of his bride only on condition of being subjected to a long-continued treatment with the Liquor of Van Swieten. The treatment finished, the marriage took place. All who were intimate with this colleague, and even those who have been present at his clinical conferences, have heard his oft-repeated and bitter recrimination against this preparatory treatment. Besides, it was very useless in the case of our colleague, for he retained an habitual discharge from the urethra, which he was accustomed to present as a final and peremptory argument to those persons whom he did not succeed in curing of a similar inconvenience.

Others, with more apparent rationality, in admitting the virulent blennorrhagia, while confessing their inability to distinguish it from benign blennorrhagia, nevertheless prescribe, at all hazards, the mercurial treatment. Of this number is Hunter, and the method of reasoning he adopts to explain the treatment of blennorrhagia is quite singular. If Hunter had no other title to the gratitude and admiration of the learned, his writings would not have come down to us; and M. Richelot, your learned and modest co-laborer and friend, would not have endowed France with his fine translation of the works of the great English physiologist. Listen to Hunter. The following passage is one of no common importance:—

"Whatever methods are used for the cure, locally or constitutionally, it is always necessary to have in view the possibility of some of the matter being absorbed, and afterwards appearing in the form of a lues venerea, to prevent which I should be inclined to give small doses of mercury internally. At what time this mercurial course should begin is not easily ascertained; but if the observation be just that a disposition once formed is not to be cured by mercury, but that mercury has the power of preventing a disposition from forming, as was formerly explained, we should begin early, and continue it to the end of the disease, till the formation of venereal matter ceases, and even for some time after. The mercurial ointment may be used where mercury disagrees with the stomach and intestines.

"This practice appears to be more necessary if the discharge has continued a considerable time, and especially if the treatment has been simply by evacuants, for in the former case there is a greater time for absorption, and in the latter we may suppose a greater call for it, such medicines having no effect in carrying off the virus.

"To prevent a lues venerea being produced from absorption, a grain of mercurius calcinatus taken every night, or one at night and another in the morning, may be sufficient, but should be continued in proportion to the duration of the disease.

"The success of this practice in any particular case can never be ascertained, because it is impossible to say when matter has been absorbed, except in cases of buboes; and where it is not known to be absorbed, it is impossible to say that there would have been a lues venerea if mercury had not been given, as very few are infected from a gonorrhœa, although they have taken no mercury. It is, however, safest to give mercury, as we may reasonably suppose it will often prevent a lues venerea, as it does when given during the cure of a chancre or bubo, where we know, from experience, that without it the lues venerea would certainly take place."—*Hunter's Complete Works.*

I beg your pardon for this long quotation; you know it is not my custom to make extracts; but it seemed the more necessary to do so in the present instance, because this doctrine still serves as a basis for the reasoning and practice of a great number of syphilographists.

Is it necessary to dwell on the manner in which Hunter supposes constitutional infection to arise from blennorrhagia? It is not the part actually diseased which infects; it is the secreted pus! Hunter evidently had not reflected on this singular mode of infection; nor, indeed, have those who have followed in his footsteps.

True, this doctrine has been strangely modified and elaborated. Thus, you will find a modern syphilographist admit that, in blennorrhagia, the infection is not derived from that portion of the mucous membrane which is diseased, but from the surrounding healthy mucous membrane, this alone having the power to absorb the virulent pus; whence we must, my dear friend, draw this extravagant conclusion, that, if the urethra throughout its entire length be affected, no apprehension of consecutive infection need ever be entertained.

The mucous cells of Hufeland are also a product of the Hunterian doctrine. He held, you know, that the reason blennorrhagia so seldom infects is that the pus is enveloped in small mucous cells, whence it is not always at liberty to escape.

Let us return to Hunter, and to the painful surprise with which we see this great mind attempting to prevent infection by a mercurial treatment. He assures us that the longer the disease may have lasted, the more chances there will be of infection, and the greater will be the necessity of prescribing mercury; not being aware that, if the mercury merely acted in such a manner as to prevent infection, its administration would be useless after a prolonged duration of the disease, since the infection would be already established and the mercury have no further influence on it. We are astonished at the confidence with which, in spite of his uncertainty as to the action of mercury against infection, he asserts its efficacy in doses so mathematically determinate. In the passage cited we find only a tissue of nonsense and of contradictions. The mercurial treatment most usually excites blennorrhagic discharges, and Hunter would have it continued up to the complete cessation of all secretion! How many patients, whose discharges do not dry up, would thus be condemned to a perpetual mercurial treatment? The colleague, of whom I have just spoken, would literally have been gorged with mercury. Under the weight of so prolonged a treatment, what would have become of an old soldier whom I treated, and who had contracted a blennorrhagia at the peace of

Amiens, and who still retained it in 1845, that is to say for more than forty years?

The whole of this doctrine of Hunter is deplorable nonsense. Allow me to produce as evidence of my correctness this strange confession: "The success of this treatment can never be established." And this still more singular admission: "We see but few who are affected with constitutional symptoms supervening on a gonorrhœa." Is not the whole question, dear friend, from the admission of Hunter himself, reduced to this: that mercury is only useful in the small number of cases in which blennorrhagia is due to an urethral chancre?

Thus even error itself confirms the correctness of the doctrine of the *Hôpital du Midi*.

Finally, in considering the treatment of blennorrhagia, we are led to notice the theory of the golden mean. M. Lagneau, who regards blennorrhagia as a slight syphilis, counsels against it a *demi-treatment*. Here we see peep out the demi-virus, the demi-virulence of our *confrère* of Lyons, M. Baumes.

Demi-treatment! Slight syphilis! Alas! my dear friend, there is unfortunately nothing slight in the pox, unless it be certain opinions of very grave men. Syphilis exists, or it does not exist. If there is pox, there is need of a complete treatment, as complete as possible; it is necessary to avail ourselves of all the guarantees which a severe and methodical treatment can supply. If the pox does not exist, *mon Dieu!* for what purpose is an anti-syphilitic treatment instituted?

How should simple and benign blennorrhagia be treated? I again repeat that, at present, I confine myself to the generalities of the question. First, one word as to the abortive treatment. You know all that has been said of repercussion; of the theory of the wolf shut up in the sheepfold. You know all the apprehensions which have been manifested relative to the metastases and peregrinations of the virus in the economy, occasioned by the abortive treatment of blennorrhagia. This doctrine has always astonished me, in view of the crowd of facts which practice daily presents to our notice.

First, it is incontestable that the greater part of the accidents to which blennorrhagia may give rise are scarcely ever manifested before the end of the first week. It is after the second, and usually later, that they are observed to supervene.

On the other hand (and those who frequent the *Hôpital du Midi* well know this fact) the greatest number of these accidents manifest themselves only in those cases of blennorrhagia which have been subjected to no treatment at all, or to treatment of a very insignificant kind. Will you permit me to give you a singular proof of this statement? At this point, my dear friend, let me tell you that I profess great confidence in medical statistics, that precious instrument of knowledge which, in the skilful hands of M. Louis, has rendered such incontestable services to our science. But M. Louis was the first to recognize and to proclaim the fact, that no task is more difficult or more delicate than to compile medical statistics; that, when misdirected or viciously applied, nothing gives rise to greater deception, or to more deplorable error. This profession of faith being made, no one can, I trust, consider, as an attack against statistics, or as raillery directed towards this precious means of research, what I am about to say relative to the causes of the accidents produced by blennorrhagia.

I said that the abortive treatment of blennorrhagia was very innocent of the accidents which may be manifested during the course of this disease. Do you know, in fact, what statistics, ridiculously interpreted, teach in this respect? That the most frequent antecedent of epididymitis is flaxseed tea! On this point I possess imposing figures. The students of my clinique every day await, with merry impatience, this final question, which I never fail to address to a patient affected with epididymitis: but you have taken flaxseed tea? The response is inevitably affirmative.

What conclusion is deducible from these figures and these facts? Evidently, that epididymitis, like the other accidents of blennorrhagia, is neither a repercussion, nor a metastasis, nor any of those chimeras by which some have sought to hinder the application of a speedy and abortive treatment of blennorrhagia.

I am profoundly convinced, by observation and by long experience, that a blennorrhagia arrested during the early stage of its existence, is far from being followed by those accidents which excite the fear of some physicians. On the contrary, the abortive treatment of blennorrhagia is the prophylactic treatment of the consecutive accidents. Thus, I have applied the abortive treatment from the first moment of the appearance of a blennorrhagia. This is a point of doctrine on which I cannot too much insist. The commencement of blennorrhagia is known; its termination and its consequences are always uncertain. Therefore, it is of the highest importance that the patient should be relieved as soon as possible of his discharge.

In spite of an old prejudice, of which the practice of Bell has been the pretext, I believe, my dear friend, that injections, which form one of the most important parts of the abortive treatment, far from producing strictures of the urethra, as some have asserted and still assert, constitute the best prophylactic treatment of these strictures. We may be assured that the quicker a discharge is arrested, the less are organic lesions of the urethra to be feared. These lesions, as well as those of all other mucous membranes, are the consequence of the duration of the inflammation. I am quite aware that on this point statistics have been invoked, and that a goodly number of cases have been exhibited in which strictures have manifested themselves after injections. But the relations of this case remind us of flaxseed tea in epididymitis; because injections are found among the antecedents of stricture, we are not necessarily justified in concluding that a specific connection exists between them. Analyze these observations well, and you will see that the blennorrhagias were of long standing; that they had resisted all treatment, even injections. It is precisely because the injections had not cured the inflammation, that the stricture supervened.

I am not willing to close this letter, my dear friend, without saying to you a word relative to the prize which my honorable *confrère* and friend, M. Diday of Lyons, has just founded. You know that he offers a sum of three hundred francs to any one who will furnish him with ten observations of simple blennorrhagia which shall have produced constitutional syphilis. This idea is good; but do you think the offer sufficiently generous? Thirty francs for an observation so difficult to meet with! Candidly, is this enough? For my part, I consider as priceless a single fact which shall show syphilis to supervene without a syphilitic cause. Consequently, I will found no prize on this point. Let my learned and ingenious friend permit me to say to him that he would have compromised neither his present nor his future fortune, had he centupled the value of the observations he seeks to obtain.

Yours, RICORD.

LETTER X.

MY DEAR FRIEND: I am now going to speak to you of the pox. As you may have observed, I have not for an instant lost sight of my starting-point.

What was it?

To seek the specific causes of diseases reputed to be venereal; to study with rigorous closeness their mode of action, in order finally to arrive at a more exact knowledge of their consequences and their treatment.

In the preceding letter, I have sought to demonstrate that if blennorrhagia has a special cause, it is not always easy, or even possible, to distinguish this special cause from that which commonly produces inflammation of the mucous membranes; I have sought to prove that this cause is not that which produces syphilis properly so called; that its consequences are wholly different; and that its treatment, unless empirical, cannot be the same as that which is requisite for the pox.

I shall be very happy should I, in all respects, merit the criticism of M. Vidal, who asserts that my efforts have only resulted in proving that "two and two make four." When I consider what is taking place with respect to syphilopathy, this proof does not appear equally easy in all cases.

The cause of syphilis not being found in blennorrhagia, where must it be sought? Do not ask me to plunge into the depths of history. This descent I have often made, and I declare to you, my dear friend, my belief that in these depths it is impossible to find the truth. The deeper we descend, the less light penetrates the gloom, and finally the obscurity becomes complete. In this obscurity, authors merely grope along. They stray unceasingly, and we stray with them.

Where did syphilis commence?

How did syphilis commence?

I am very fearful that these questions will forever remain unsolvable. We can merely affirm that syphilis, as it is understood at the present day, is not spontaneously developed in man; that it is always transmitted. Moreover, as we have already remarked, we do not recognize it in any other member of the animal kingdom. I am familiar with the fact that your journal has announced quite recently that syphilis has lately been found in horses in Italy. In order to credit this fact, I await more complete symptomatic descriptions. Still, it would prove a spicy incident should the pox, which is asserted to have been propagated in the human species for the first time in Italy, also appear in Italy for the first time in the horse.

Every man who reads history without preconceived notions is impressed with meeting in the works of ancient authors, and those who lived anterior to the epidemic of the fifteenth century, perfect descriptions of all that we are acquainted with at the present day, and which we class among primitive accidents. Could we now give a truer and more exact description than that furnished by Celsus? Galen goes so far as to find a connection between the accidents of the genital organs and those of the throat. William of Sallicet knew that primitive ulcers of the penis were contracted after connection with unclean women. He clearly established the relation which exists between ulcers of the genital organs and buboes, &c.

What the observers and the historians of the pox of former times lacked was a more exact knowledge of the filiation of the symptoms, of the relations to each other both of the primitive and constitutional accidents. But what was the leprosy of that period? Is the leprosy of the Greeks or of the Arabs, which we are familiar with at the present day, similar to the leprosy of the ancients? In nowise, for leprosy then was often contagious, and was frequently communicated by sexual intercourse. Evidently, it was not the present leprosy. The Bible, despite all the efforts of commentators, gives us but little light on its history. Probably the Divine Inspirer of the Holy Scriptures had weighty motives for leaving some obscurity on this point.

I make no pretensions to retrospective science. The labors of Astruc have frightened me, and I confess that I am but little tempted to undertake so great a labor for so slender a result. But whoever studies syphilis, however so little his mind may be tormented with the desire of knowledge, will ask himself the question I have a hundred times proposed to myself: What, then, was this terrible epidemic of the fifteenth century, and whence came it?

Some contemporary writers have derived it from the stars. Of the process by which cognizance of astronomical events at that epoch was obtained, I acknowledge myself to be ignorant. This much is certain, that the reign of syphilis is constant, although Jupiter may have acquired additional virtue, and Saturn and Venus may no longer yield themselves to conjunctions which were attended with such disastrous consequences to the human race. Hence, we are forced to seek our explanations on the earth, and to consider our subject from a less elevated point of view.

The origin of this dreadful epidemic of 1493, no contemporary at first thought of attributing to the New World. But this assumption found support in the writings and the active propagandism of Oviédo, through motives which it is useless to examine. The explanation of them may be found in the religious, political, and jesuitical history of the times.

This fable, it is known, forms the subject of the extensive romance edited by Astruc. Heaven preserve me from discussing it! This work has already been done, and well done, by Sanchez. I will merely allow myself to make a slight observation in a pathological point of view.

To have originated an epidemic on so grand a scale, all or almost all of the sailors of Christopher Columbus must have been infected with syphilis. During their long voyage, which was not then made by steamers, the primitive accidents must have remained at the period of progress or of specific *statu quo*, susceptible of furnishing the contagious pus, into the study of which we shall soon enter.

But, what is a very remarkable circumstance, the sailors of the fleet, on arriving at Lisbon and Bayonne, did not at first infect the women of these ports; yet, is it probable that, contrary to the custom of sailors of every age, these alone should, after a long passage, have been continent on reaching port? Very well! It is not to the women of Lisbon and of Bayonne that they communicate the disease. They depart for Italy, in May, 1495, in search of the army of Gonsalvo di Cordova; and it is there that they communicate the pox—to whom? We know nothing, excepting that in Italy, in the midst of three armies, Spanish, Italian, and French, a disease, already known since 1493 or 1494, raged with fury; each of the belligerent parties casting upon the others the shame of having communicated it.

I do not wish to dwell any further on this historical question, so perplexing and so obscure, and which I make no pretensions to elucidate. I only ask myself whether this epidemic of the fifteenth century resembles the venereal diseases of our day; and I find that it certainly does not. The accidents which we observe at the present day resemble infinitely more those which the ancients have described from time immemorial, than the epidemic of the fifteenth century.

Here, my dear friend, allow me to impart to you, but with the reserve and discretion which such things require, an idea which I believe to be of importance. I submit it, under the simple form of an indication, to some young and industrious *confrère* whose good fortune it is to exist at that happy period of life when researches can be prosecuted with facility.

In studying with care the descriptions of the epidemic of the fifteenth century, I have been struck with a fact which appears to me of exceeding interest. The mode of transmission of the accidents, their gravity, the predominance of the constitutional infection over the local phenomena, which either did not exist or were unperceived; all this seems to me to resemble much more what we see in acute glanders and the farcy, than in the pox. Van Helmont broached a similar opinion, which was considered perfectly ridiculous. He made the pox come from the farcy, the result of I do not know what ignoble and beastly intercourse. Aside from the disgraceful source whence his opinion was derived, Van Helmont perhaps was not far from the truth.

You may see, my dear friend, that the knowledge of the glanders and farcy in man is quite recent; and yet the aptitude of man to contract this disease, which has always existed in the horse, cannot be of recent date. How many men with glanders and farcy must have been taken for syphilitics!

The mode by which the epidemic of the fifteenth century was transmitted must have struck your attention. The disease was often communicated by the breath in churches, and in the confessionals; to such a point, indeed, that Cardinal Wolsey, accused of having the pox, was put on trial for having spoken in the ear of King Henry VIII. This mode of propagation is wholly inexplicable as regards syphilis, which requires immediate contact.

I well know that all the authors of the time do not recognize this mode of transmission. Fallopius quite merrily ridiculed Victor Benoit, who affirmed that the holy daughters of a convent caught the pox through the thick grates of the parlor; Fallopius believes there was a little *holy water* concerned in the matter. But may not the epidemic which certain authors, Paracelsus among others, considered as a mixture of the old venereal affection and the leprosy, be considered with more probability as a mixture of the former affections with the glanders and the farcy? The glanders arise spontaneously, and are easily reproduced in the horse, particularly in time of war, when it is exposed to so many inconveniences.

Study the symptoms, and you will see manifested at once, and as it were *d'emblée*, the gravest accidents; a circumstance which never happens in the syphilis of the present day. You will see inoculable pus produced in all parts of the body; a result which is also never witnessed in the modern disease. Possibly I deceive myself, but it seems to me that this subject is one of exceeding interest. Here there seems to peep out the first glimmering of a truth which up to this time has escaped our notice. We shall owe this truth to the fine works of M.

Rayer and of his school, and of M. Renaud (of Alfort), on this terrible disease by which man is so disastrously scourged, and between which and the epidemic of the fifteenth century I find such striking resemblances. How many fine results may be arrived at from this point of view! Does any one know what the glanders, transmitted from man to man, will produce, in proportion as it is removed farther and farther from its origin in the horse? Does any one know what is its hereditary influence? For individuals affected with glanders and with farcy can procreate; and we are completely ignorant of what the products of these procreations become. I should be happy to fire the zeal of some laborer in our science. Here, it seems to me, is an abundant harvest of glory to be reaped.

But, I must confess, all these ideas which are agitating my mind belong as yet to the vague domain of hypothesis. Your readers, I know, must desire to see me enter upon the field of reality. I am drawing nigh to it. Adopting the conclusion of Voltaire, I may say that, like the fine arts, the origin of syphilis is unknown. But this much I do know, that we find it to-day in a source, alas! too well known; and it is from this source that I will draw my next letter.

Yours, RICORD.

LETTER XI.

MY DEAR FRIEND: We are now to determine the source of the specific cause of the morbid poison which produces syphilis. This poison may at present be called by its name; that is, the *syphilitic virus*.

Well! the existence of this virus—I recall the circumstance, since some have appeared desirous that the fact should be forgotten—was contested and formally denied when I undertook my first researches in syphilopathy. At that time, many physicians dared no longer to give it this name for fear of compromising their reputation; and the learned Jourdan, in a fit of whimsical anger, cried out: "Call it what you please; but do not give it the name of virus!"

The source of this virus I have arrived at by means of the lancet; on this, however, I have not pretended to base the whole of science, as my honorable colleague, M. Cazenave, wittily accuses me of doing.

It is by entering into a comparative examination of all the reputed syphilitic accidents, that I have been able to demonstrate that only one of these regularly furnishes the purulent matter which, when placed in certain conditions which we are about to define, is capable of producing, by virtue of a special irritation, an ulcerating inflammation, identical with that from which it originated, and of reproducing in its turn the same special secretion, the same morbid poison, and this without limit.

The syphilitic lesion, the source of the secretion, which under favorable conditions produces the fatal phenomena we have just indicated, is the primitive accident, to which has been given the name of *chancre*—a name which it yet preserves. As I have already had occasion to say, whenever the surfaces, from which the morbid secretion has been taken, were visible, positive results have been obtained. These results have been reproduced only when chancre was present.

Is it necessary to repeat that my excellent colleagues, MM. Puche and Cullerier, of Paris; MM. Baumes and Diday, of Lyons; M. Renault, of Toulon; M. Serre, of Montpellier; M. Thiry, of Brussels; and M. Lafont-Gouzy, of Toulouse, &c., have, after numerous experiments, arrived at the same conclusions as myself?

Every time a secretion not taken directly from a primitive ulcer has produced a chancre, this secretion was furnished by a surface which could not be examined. The small number of apparently exceptional cases in which chancre has been reproduced with a purulent matter taken from a non-ulcerated surface, finds a rational and complete explanation in the facts analogous to those whose history I have related. How can it be supposed that the surfaces which it was impossible to inspect were not the seat of chancre, when the secretion they furnished was absolutely the same as that of chancre?

If it were proved that the primitive ulcer, the essential source of the syphilitic virus, could only be seated on external and visible surfaces; that the depths of the urethra and the cavity of the uterine neck could not be the seat of these hidden ulcerations, the question would be settled. But does there exist a single writer on syphilis who denies the existence of the primitive ulcer in all these regions; one who does not know and who does not believe that all syphilitic ulcerations are not always visible? How, then, deny the possibility of the existence of the deep and hidden chancre, when it furnishes itself the most irrefragable proof of the fact; that is to say, the secretion?

Some have said that inoculation proves nothing as to the existence of the specific cause of syphilis; that it is preferable to await the ordinary results of the contagion in order to obtain this proof; for that with any pus the same result could be produced which I pretend to produce only with the pus of chancre; whilst, by the mysterious means of common contagion, phenomena are observed which are not produced by inoculation.

Is it singular that these arguments should be employed both by the supporters of the syphilitic virus, and by those who deny its existence? What, in fact, say the physicians of the physiological school? That with any pus whatever the same result was obtained; that is to say, the production of every variety of venereal disease. And upon what basis did they attempt to sustain this doctrine? Upon grounds which then appeared reasonable; upon the uncertainties which usually exist relative to the circumstances in which venereal diseases are contracted; upon the absence of examination of the woman; upon the plurality of the accidents which one woman produces in several men, when she at the same time may leave other men wholly free from troublesome consequences; in fine, upon all the fables which we have already pointed out and combated, and on which it seems truly astonishing, when we consider the light which the speculum has thrown on the subject, that men of incontestable merit, like M. Cazenave, should still wish to rest superannuated doctrines.

But I am deeply surprised that the partisans of the syphilitic virus, those who claim for syphilis a specific cause, and for its virus a specific action, should maintain that with any pus, no matter what it be, effects can be produced analogous to those of the virulent inoculation *par excellence*. Do the advocates of this doctrine think that with any pus whatever vaccinia or variola may be produced? If we should furnish them with purulent matter for their experiments, with the origin of which they were unacquainted, what would be their criterion for determining its nature, apart from the effects which it should produce? And is it not by this means that I arrive at the distinction of the syphilitic pus?

But to this objection relative to the inutility of inoculation, I have a further reply to make. I have inoculated the same patient, hundreds of times, with the pus of chancre, the pus of balano-posthitis, the muco-pus of urethral blennorrhagia, the muco-pus of blennorrhagic ophthalmia, and with the pus furnished by phlegmonous inflammations of other regions; and while the pus of chancre invariably produced chancre, the other kinds remained inactive. What other proof can be desired? and what solid objection can be urged against it?

But it has been objected that the effects produced by inoculation on an individual already infected prove nothing as to the nature of the cause; in other words, that the inoculation of an individual with the secretion furnished by himself leads to no important conclusion, because, infection first assumed, every wound can and must become syphilitic.

Here is a singular error, which may be attended with serious consequences; a dangerous prejudice, which we are astonished to see still brought forward by those who make pretensions to accurate observation. The facts I am about to mention demolish this objection completely. I well know that the cases of leech-bites, which have taken on the characters of venereal ulcers, have been cited. But be persuaded, my dear friend, that these bites, like every wound made in a syphilitic patient, become venereal ulcers only in so far as they afterwards become affected by the contagion. Apply leeches where there is no contact of inoculable pus; bleed syphilitics as much as you will; make any experiment you please; and, if there is no virulent contact, virulent transformation will be impossible. Among the numerous observations I have made which substantiate the truth of this assertion, I will relate the following from the clinique of the *Hôpital du Midi.*

At the time I had a female ward, a patient affected with a phagedænic chancre of the vulva, with abundant suppuration, was seized with a pain in the tibio-tarsal articulation. Leeches were applied over the painful point. Some days afterwards, the patient complained of the bites; and it was easy to discover that some of them had undergone a veritable transformation, and had become true chancres. For a moment, this result might have been attributed, and some students did attribute it to the state of the general system. As for myself, I had not the least doubt as to the nature of this transformation. First, all the bites were not ulcerated. Again, the patient was seized with similar pains in the opposite articulation; a new application of leeches was made, due care being taken to guard against all injurious contact; and this time, therefore, none of the bites experienced the least syphilitic transformation.

I made a still more conclusive experiment. I have frequently experimented with the pus of a chancre on a patient laboring under the influence of a constitutional syphilis produced by a preceding contagion; various punctures were made; and here, as in other cases, the matter from the chancre alone gave rise to positive results.

Thus, whatever may be said to the contrary, it is unjust to compare a syphilitic patient to a leathern bottle full of virus, which is allowed to escape by the smallest puncture. The figure is poetical; but it is not true.

But in order that these results be invariably obtained, our reason assures us at once that the *virulent matter* must be taken from a chancre at a certain period; that is to say, at the period of progress. It is very easy to conceive this fact; and I am sure I shall not weary you in seeking to make you understand that, if you take the pus from the surface of an ulcer which is in the way of reparation and cicatrization, you will have a simple, inoffensive pus, which will give you negative results; and that the same accident, experimented on at two different stages of the disease, will lead to different results. You will conclude, then, with all candid observers, that there is no contradiction, no uncertainty, in the results of these experiments; and that I have not resorted to evasion, to subtlety of doctrine, for the purpose of explaining facts which seem to bear against the principles which I maintain, and which are maintained by Bru. When Bru failed to inoculate the pus of chancre, it was for one of two reasons: Either he made an error in diagnosis, or he took the pus from chancres *at the period of reparation.* There is no way of escaping from this dilemma; for I repeat, and am ready to prove the fact to the incredulous, if any such there now are, *the pus of chancre is* INEVITABLY *inoculable.*

Perhaps you find, my dear friend, that I indulge myself too much in the pleasure I derive from writing to you. But it is your own fault; you would never stop me. Profiting, then, by your willingness, I will say that, if the *virulent matter*, composed of the morbid poison and a vehicle, is ordinarily formed in a thin, ichorous, sero-sanious pus, charged with organic detritus, it does not invariably present the same characters; it may exhibit all the known varieties of pus and of muco-pus. It may be acid or alkaline; contain animalculæ or be free from them. These different conditions, which seem contradictory, and which have served as an argument to those who deny the existence of the virus, pertain only to its vehicle, and in nowise change its nature, which always remains the same. A circumstance which it is important to point out, which has been established by experiments on inoculation, is that putrid pus is no longer virulent. Gangrene *destroys* the virus.

Whatever be the seat of the chancre whence the virulent matter is drawn, it is not essential, to be effective, that it should have been recently secreted, and is warm. Preserved as the vaccine virus is preserved, it acts equally well. This fact was established by artificial inoculation, contrary to the opinion of Cullerier, which until then had prevailed among scientific men.

Inoculation proved the fact of different modes of contagion, about which there is more or less dispute, in so far as the necessity of a physiological action, an orgasm of the part furnishing the contagion, was believed in; in so far as it was believed that this contagious matter must be yet warm at the moment of its action. The cases of Fallopius and of Hunter, in which chancres were contracted by touching the seats of a privy; those of Fabrice de Hilden, in which accidents were contracted by sleeping in sheets in which infected persons had already lain; and many other similar cases, comprise a record of observations which have become incontestable.

You will yet allow me to say one word in relation to certain requisite conditions of the part about to be inoculated. Whatever this may be, whether skin or mucous membrane, or in whatever region of the body it may be found, all that is needed is a *slight solution of continuity, without the aid of any physiological act, to produce the inevitable effect.* No persons, as in vaccinia and variola, prove *refractory* to this primitive accident; there exists no privilege of idiosyncrasy; a lancet charged with purulent matter reduces all to perfect equality.

Thus, then, my dear friend, inoculation with pus from a primitive accident, from a chancre whose conditions I have just indicated, has always produced identical results, whether the subject of the experiment were the patient who furnished the pus, or whether, according to the practice of some experimenters, this pus were used to inoculate a healthy individual.

But it has been further said: It is rash, imprudent, impossible to draw any rational conclusion from artificial inoculation; you impose upon nature other conditions than those in which she is placed in the contagion which may, by contrast, be called *natural.* Some have anathematized this artificial inoculation, believing they could say of it, as was said of physiological experimentation: Torture interrogates, and pain responds.

Our celebrated physiologist, M. Magendie, to whom you have just addressed your first and very remarkable *Medical Letter*, will tell you what he thinks of this indignation of the poets. As for myself, who cannot speak with the same authority, I will say that I enter into no contest with nature relative to her mysteries; that I know she does many things by processes which she conceals from us; yet I still maintain that it would be unworthy weakness to attempt to render her still more mysterious, and thus to make yet more obscure the veil which covers her;

that it would be disgraceful to close our eyes, when she seeks to unveil herself.

Let us see, then, if there exists any real difference between the natural and the artificial contagion, to which reference has been made. I will tell you what I think of the matter in my next letter.

Yours, RICORD.

LETTER XII.

MY DEAR FRIEND: Does there exist any real difference between the natural and the artificial contagion of syphilis? This is the subject of our discourse.

Observation and the rigorous analysis of facts demonstrate to those who do not permit themselves to stray either by reason of prejudice, or preconceived ideas, that the contagion of syphilis, in whatever circumstances it may act, is propagated by a process of inoculation more or less analogous to the way in which it is produced by the lancet. The accident (chancre) which, from the confession of all, is the most inevitably contagious, is inoculated by the lancet. By this chancre likewise timely observations, carefully made, show syphilis to commence.

Apart from the evidence of the fact derived from artificial inoculation, chancre is found to be developed everywhere, without choice of seat; on the whole periphery of the body; on the whole of the external integument, or on the internal as far as accessible; and consequently, as far as the parts subjected to the contagion, or those furnishing the infecting matter are concerned, there is no necessity for special functions or a particular physiological state. Other conditions are necessary for contagion.

Examine with care all the parts which are affected, and you will find that they are those which present the most favorable conditions for mechanical lesions, for excoriations, for solutions of continuity of all kinds; you will find that the accident develops itself by preference where many and voluminous follicles exist, into which the virulent matter may be introduced.

Is it not true that, in man, the border of the prepuce, particularly where there is more or less marked phimosis, the neighborhood of the frænum, the adherent points of the semi-mucous membrane of the glans and prepuce—points which, not possessing the suppleness of other regions, are more readily torn; in the woman, the fourchette, the points of insertion of the nymphæ, the carunculæ myrtiformæ, are the points which by preference become contagious? In other regions, is it not also true that it is when excoriations exist that contagion is established? Thus, an excoriation of the finger is often the door by which syphilis enters. But the presence of the excoriation is necessary. If it were otherwise, would I ever leave the hospital without having a chancre on the end of every one of my ten fingers? Chancre frequently appears upon the lips; but the lips are almost always cracked; pleasure excites a smile, and the smile extends and dilates the lips. The nipples of nurses are often the seat of chancre; but these parts are usually cracked and torn. Chancre also takes up its residence wherever there has been a cicatrix; but in this case, too, there has been a loss of suppleness, which renders the production of cracks and excoriations easy.

In all this, my dear friend, you see nothing which is physiological, as it has been said; nothing which demands particular vital conditions. You see no special state of the organism, nor the exercise of any specific function. For you, as for myself, the phenomenon is regarded as traumatic and mechanical.

Practice, that criterion of all doctrines, comes, alas! too often to support me. Nothing is more common than to see the physiological act of generation remain free from every troublesome consequence, while other acts, which are connected with no peculiar physiological state, draw after them unpleasant results. The genital organs, so specially the seat of syphilitic affections, do not always draw the infection from the genital organs. It is not always the genital act, properly so called, which becomes the infecting cause. Coition becomes an infecting act only in so far as it coincides with certain material circumstances. Among the innumerable examples which I could cite in support of my opinion, I beg permission to mention two, which impressed me with peculiar force, inasmuch as they came under my notice in direct succession the same day. There is no physician who does not know that there are singular days, when curious facts come as it were in a series.

A gentleman brought me his mistress, whom he had diseased, and in a manner which greatly astonished him. He had on the penis a primitive ulcer at the period of specific progress. He had had normal connections with his mistress, and, during the same night, more culpable relations, *à prepostera venere.* The normal relations had been more frequent than the others. The mistress presented absolutely nothing suspicious in the genital organs; but she had a chancre at the anus. What does this tell? That the physiological and natural passages had yielded without excoriating, and had escaped contagion, while the abnormal passage, more resistant, was torn and infected.

Here comes another couple. Here again a struggle between a physiological act and a prelude which does not pertain to the human species, a prelude which is not placed at least among the genital functions of man. A gentleman, surprised at seeing a suspicious tumor appear upon one of his lips, without any disease of the genital organs, came to request me to examine the woman with whom he had had connection. In this woman I found a chancre at the specific period, situated near the meatus urinarius. The gentleman had had frequent sexual intercourse with her during the same night, during which he wandered to such a degree as to seriously compromise his lips. It is necessary to add that he was very subject to cracked lips, and that the season was winter.

These facts, which I could multiply, prove that the physiological conditions of the genital act are not without influence in the contagion of syphilis. Thus, so far as this point is concerned, the doctrine of physiologism is destroyed. Be perfectly sure that, in spite of the most intimate act, of a fusion the most complete, and of an orgasm the most voluptuous, one can safely escape, provided the skin is sound and the mucous membrane irreproachable, from the most compromising connections. Be perfectly certain, on the other hand, that a torn skin, an abraded mucous membrane, will render the lightest contact unfortunate, and we physicians have a thousand precautions to take in this respect, and surely our examinations are rigid. It is known, however, that physicians have furnished victims to the martyrology of syphilis; and that it was in the benevolent exercise of our art that the unfortunate Hourmann, and Delavacherie (of Liège), met slowly frightful deaths.

After what I have just said, what can you think of the pretended physiological inoculation, so far as regards blennorrhagia, of my colleague M. Vidal? You know when and how this is really inoculable by the lancet; that is to say, only when the pus emanates from a chancre, and this is of exceedingly rare occurrence, as M. Vidal admits. But, in the other conditions in which blennorrhagia is produced, is there, physiologically and pathologically speaking, anything resembling the contagion of chancre? Do we always know, as I have repeated to satiety, whether the blennorrhagia is due to a veritable contagion? And yet this condition of contagion has been considered as a proof of virulence, as a sort of physiological inoculation which the lancet is unable to produce. Hear M. Baumes. It would seem that successive blennorrhagias become his means of diagnosis; but he fails to tell us how many times blennorrhagia must be produced in order to become virulent. Thus one contracts the malady, and imparts it to another. Where does the virulence commence? M. Baumes does not say. Suppose a woman should be suspected of having contracted a discharge from a doubtful man. If we wish to assure ourselves of the nature of her discharge, is it necessary to make an inquest; and, in seeking to ascertain the different sources of the blennorrhagia of the man, to pursue the disease even back to the gonorrhœal flux of the Bible? But we shall not have made a single step in this inquest, without this difficulty, than which none is more common. We shall find two individuals who had commerce with the same woman, one of whom will, and the other will not have contracted a blennorrhagia. For the one, we must conclude that the blennorrhagia is benign; for the other, that it is virulent. All this is trifling.

Facts and observations do not then indicate, my dear friend, any difference between the inoculation called physiological, and that which is artificial. Let us now invoke analogy.

In every disease *incontestably* contagious, it is found that traumatic conditions predominate, and that, in ordinary circumstances, art can repeat what nature accomplishes. Thus inoculated vaccinia does not differ from ordinary vaccinia. Inoculated variola does not differ from spontaneous variola. The same holds true of the glanders and the farcy, of hydrophobia, of the malignant pustule, of anthrax, of hospital gangrene. This argument, from analogy, seems to me of incontestable value. Why should the syphilitic virus alone escape the common law?

But chancre, it has been said, is not the only contagious syphilitic accident. There are secondary syphilitic accidents in which the lancet has been unable to find *contagion*. Science, in fact, contains a multitude of observations which seem conclusive to a great number of physicians, but which leave the minds of many others in doubt. The mucous papules (flat humid pustules, mucous tubercles, flat tubercles, mucous patches) are considered contagious by a great number of syphilographists, and liable, consequently, to be transmitted.

When I have studied this accident by means of inoculation, carefully weighing all the circumstances, with the object of preventing error, the experiment has always been negative. But other observers have obtained contrary results; to these I can only reply by stating what has occurred in my own experience.

I inoculated the pus of mucous tubercles, situated near the vulva, in a young girl from Versailles, who had habitual and frequent intercourse with the garrison of the place, and I obtained a positive result. Much surprised, I examined with more care the surfaces from which I had taken the pus; and I then easily recognized the fact that, among the mucous patches, there existed a chancre still at the period of specific progress. Inoculation with the pus taken from this ulceration, and with the matter taken from the mucous patches at a distance, gave these results: the pus of the chancre exhibited the characteristic pustule; while the muco-purulent secretion of the mucous patches remained without result. This experiment seems to me to be decisive.

In the observations which have been cited relative to the mucous papules which have communicated syphilitic accidents, no account has been taken of the time which transpired between the time the disease was observed, and that at which the infecting coition took place. Three weeks, a month, two months, and even a longer period after the contagion, elapse before the patients present themselves to the physician. So that the knowledge of the disease at its commencement is not only wanting, but it is even impossible to tell the true nature of the accident which was the source of the contagion. Some persons forget, and others appear not to know that, by a succession of metamorphoses, which can easily be observed when requisite pains are taken to ascertain the fact, the primitive accident (chancre) passes *in situ* from the state of an *organ* of virulence to the conditions of a secondary accident, no longer furnishing specific pus. What observations do we possess of persons with mucous patches transmitting the disease to another person in whom, the second or third day after the infecting coition, the disease has been seen to begin in the same way as when the result of the chancrous contagion? In this case does the disease commence by a chancre or by a mucous papule? The latter alternative is not supported by one incontestable fact. However, there is no lack of cases of mucous papules. For my own part, I possess very numerous observations, as respects both men and women, of very marked mucous tubercles, which prove that the patients thus affected were enabled to indulge in frequent genital relations without imparting disease. Of these, there is one which will remain deeply impressed upon the minds of my readers, as it has upon my own mind.

A gentleman whom I had treated, two years before, for chancre, was about to marry. Before his marriage, he came to see me in order to submit to a rigorous examination. I found him in excellent health; and told him he could marry without fear. But, being highly scrupulous, he requested to be examined anew on the very evening of his nuptials. I again found him perfectly exempt from every symptom, and I gave him my patent with the utmost readiness. One month afterwards he sent for me.—"My dear doctor," said he, "my wife has tumors which distress her greatly. See what the disease can be." Before entering the wife's chamber, I made another examination of the husband, and found him as pure as he was on the day of his nuptials. But it was not so with his wife; I found mucous papules confluent and developed, in such a way as to render it certain that the starting-point of the accidents dated anterior to the marriage. Convinced that the husband had had no influence in this sad affair, and that he could not have given a disease which he did not possess himself, I said to the woman in a firm tone: "Madam, you are diseased; and it is not your husband who has rendered you so. If I become your confidant, I also become your accomplice. If not, I remain the physician of your husband."

I soon obtained a painful and distressing confession, which gave me the solution of this sad enigma.

I relate this incident, because it presents this interesting fact, that, since his marriage, the husband had not passed two days without repeated connection with his wife; and yet he had remained absolutely free from disease.

I have not finished my remarks on mucous papules. Allow me to return to them in my next letter.

Yours, RICORD.

LETTER XIII.

MY DEAR FRIEND: I return to mucous papules. You are aware that, according to many syphilographists, this secondary accident is contagious. Among the proofs invoked in support of this doctrine, it is necessary to note the hypothesis according to which the successive development of these mucous papules upon the parts of the skin contiguous to those on which the accident was first developed is considered the result of contagion. Thus, we see patients in whom these papules are at first developed on the side of the scrotum; if others chance to be developed upon the inner part of the thigh, the partisans of this opinion attribute them to contagion. If from one side of the anus, these papules gain the opposite side, they still cry contagion, and so in other cases. Those of my brethren who profess this doctrine—among whom there are some in high places—simply forget one little circumstance; they neglect to consider the cause which produced the first patch; that is to say, the state of constitutional infection in which the patient is placed, a state which may produce a second and a third patch, for they do not all appear at the same time. The consideration of the preference these patches exhibit for a given seat can in nowise come to the aid of the doctrine of contagion. If it is a fact that there is contiguity of skin where these patches appear, it is equally certain that there also the acrid secretions are more active; that the skin has, in these places, a tendency to mucous transformation, as in the vicinity of the genital organs, the anus, &c. How otherwise explain by contagion the development of mucous patches from one axilla to the other?

I shall therefore remain convinced, until the contrary is proved, that when *mucous tubercles*, which have been admitted to be primitive, are believed to have been contagious, there must have been an error in diagnosis. It may be useful to call to mind the fact that a chancre, at the period of reparation, often takes on the granulating aspect of mucous patches; that it sometimes undergoes a veritable metamorphosis, and becomes, *in situ*, a secondary accident the nature and physiognomy of which are those of mucous patches.

If its commencement has not been observed; if the evidence furnished by the neighboring ganglia has not been invoked, so modified may the remains of the margin of ulceration and the character of its base have become, as to render the differential diagnosis exceedingly difficult, especially to eyes unaccustomed to observe, and to fingers unskilled in manipulation. If to this difficulty, you add the fact that there are particular seats where primitive accidents are not usually observed, or where the transformation of the chancre is especially easy and rapid, such as the lips, the tongue, and the nipples, you will see how easy it is to be deceived.

All those poxes, occasioned by a lascivious kiss, or by the utensils of the table, by pipes, by razors, by masks, &c., have no other origin. And how frequently have these circumstances been made the *honest* pretexts for dissembling in relation to other contacts! The mask particularly has always—and even in our day—proved a very convenient means of dissimulating a compromising diagnosis.

Even in certain religious customs, my dear friend, proofs of secondary contagion have been sought. In this category have been ranged the syphilitic accidents transmitted to children by the process of Hebrew circumcision. But these accidents find their natural explanation in the presence of primitive accidents in the mouth of the peritomist. Allow me to say here that I have been among those who have most contributed towards inducing the Israelitish Consistory of Paris to reject the ancient and dangerous practice of sucking.

Many physicians are absolutely unwilling to take into account the facility with which chancre passes to the secondary state. They occupy themselves only with its seat; and when they see a chancre in the mouth, they are led, from this circumstance alone, to consider it as a secondary accident. This is a grave error, and it gives me occasion to say that primitive ulcers are much more common in the mouth than in the anus. The latter, indeed, I find more rarely than formerly, both in hospitals and in private practice. It appears to me that certain shameful practices are diminishing in frequency, and that this is a favorable symptom of progress in public morality. Be this as it may, do not conclude from the simple fact that a chancre has its seat in the mouth, that it is a secondary ulcer. Do not forget the famous genito-labial nerve of Voltaire—a pleasant witticism which it is sometimes necessary to regard in a serious aspect. I know a professional brother, in high position, who is firmly convinced that an ulcer of the cheek was communicated to him by a *secondary kiss*.

As I have told you that I had often seen persons, affected with different varieties of mucous papules of the genital organs, transmit no disease in their sexual relations, I shall also say that I have seen a number equally great with mucous patches of the lips, of the tongue, of the throat, live with their families, and indulge in all usual buccal contacts, with the same impunity. I know a gentleman, residing in the suburbs of Paris, whose lips and tongue have, for the last six months, exhibited mucous *tubercles*. This gentleman has had all possible relations with his mistress; and though he is very negligent in regard to his treatment, for he is convinced that his accidents cannot be contagious, he has continued his connection without ever communicating any infection.

It is in regard to the transmissibility of these secondary accidents from the nurse to the nursling, and *vice versâ*, that this question becomes especially important. The fact of this transmissibility is generally admitted. Hunter, however, denied it; and many close observers partake of his opinion. This is so grave a question that you cannot refuse me permission to give some developments in regard to it. It concerns public hygiene, and often has special relation to legal medicine. As fraud, bad faith, and cupidity may be brought into play, it is important to be on one's guard against all the causes of error, and not to accept with complacency the dicta of persons who may have more or less interest in deceiving us.

In consulting the archives of science, and seeking the basis on which the opinion of the transmissibility of secondary syphilitic accidents from the nurse to the nursling, and reciprocally, is found, we are astonished at the slight value of the facts which meet the eye, as well as at the meagreness of detail which has satisfied so many eminent men. M. Bouchut, for example, in a memoir recently published (*Gazette Médicale*, 20th April, 1850), has collected all the facts which appeared to him most positive. Now, read this work, in other respects interesting, and you will be convinced that the greater part of these assumed facts are inadmissible; that the observations which appear most conclusive are deficient in essential details, and are so incomplete that M. Bouchut himself is forced to acknowledge the fact; to such a degree, indeed, that he finally admits his conviction on this point to be rather moral than scientific.

Here are my own observations on this point: I have seen instances in which the nurse and the nursling were mutually charged with propagating the infection. Generally, I have been able to go back to a primitive accident in one or the other, and thus find the regular starting-point. Sometimes, I have observed merely simple coincidences. In cases where it has been impossible for me to go back to the primary cause, the infants had been presented to me only five or six months subsequent to being placed with a nurse.

For many years, I had a ward of nurses at the *Hôpital du Midi*, where I often received women affected with simple leucorrhœas; I gave them infants to suckle, sent to me from the *Maternité*. These infants had secondary accidents; but never, so far as my observation extended, did these nurses become infected.

On the other hand, nurses exhibiting very manifest secondary accidents, have given the breast to infants supposed to be affected with syphilis, but in which were observed, in reality, nothing but eczematous, impetiginous eruptions, or varieties of porrigo; yet in no instance did these infants become infected.

My learned and illustrious friend, Dr. Nonat, who had for a long period the care of the nurses dependent on the administration of the hospitals, has arrived at the same conclusions; and he does not believe in the transmissibility of secondary accidents from nurse to nursling, and *vice versâ*.

In my private practice, I have witnessed numerous facts of this kind. The following is one which I saw in connection with my friend, Dr. Chailly-Honoré. The subject was an infant born with an hereditary syphilis, in which, six weeks after birth, there supervened various accidents, mucous papules of the ano-genital regions, moist squamous papules of the body and members, and deep ulcerations of the lower lip. Well! this child was given to a nurse at the moment of its birth. We were able to observe both it and the nurse during the eighteen months that the suckling lasted. The ulceration of the lip lasted three months; this ulceration was scarcely cured, when, in spite of careful and prolonged methodical treatment, a new ulceration manifested itself upon the velum palati, which also resisted treatment several months. Now, this nurse remained free from all infection; she enjoyed, and still enjoys, perfect health.

Certainly, this fact is well worthy of attention. I have just observed an analogous case with my associate, M. Bassereau;

that of an infant, which, among other symptoms of hereditary syphilis, exhibited ulcerations of the lips. This child was suckled with perfect impunity by its nurse.

You see, my dear friend, how important it is, in order fully to appreciate the value of such facts, to take into account all the conditions of the case with respect both to the nurse and the nursling.

The nurse, when she takes an infant to suckle, may be under the influence of a syphilitic diathesis, which there is nothing to indicate. I ought to say that, when a nurse is received, she is not usually subjected to a complete and thorough examination. I may add that, though this examination were made, we might still be deceived, for the diathesis may exist when every trace of a primitive or successive accident has disappeared, especially when the chancre has occupied the neck of the uterus. I ought to add, still further, that the health of the foster-father is not always, alas! a sufficient guarantee that no disease exists. I have, for a long time, had my own opinion with respect to the proverb relative to the pure manners of the country.

The nursling may be born with an hereditary syphilis. In the nurse and nursling there is yet no apparent disease. So that, though the infant may at birth appear healthy, yet some weeks or months afterwards, secondary accidents may be manifested. These may appear in the child, coincidently with, or subsequently to, a similar manifestation in the nurse. The one in whom the first manifestation occurs is supposed to communicate it to the other; and, frequently, indeed, they are reciprocally accused with imparting the contagion. Both parties are wrong. There is simply simultaneousness, coincidence; and, with attention and patience, the truth may be arrived at.

It sometimes happens that nurses contract syphilis during suckling, in which case they may impart the disease from divers regions; oftenest from the genital organs. This is often the case with respect to those nurses who come frequently to Paris. Under these circumstances, the nurses infect their foster-children by means of their fingers, contaminated with the virus. They even infect their husbands; in which latter case the cause of the evil is always attributed to the *Parisian nursling*, to the *rotten* infants of the hospital, as these not over-chaste nurses are accustomed to designate them. It often happens that M. Cullerier and myself have made observations on what may be called a duplicate case, in our two hospitals; that is to say, he attends a wife at Lourcine, and I the husband at the Capuchins.* These poor rustic husbands manifest extreme candor relative to their disease. According to their account, the foster-child is the origin of their whole trouble.

Contagion is frequently propagated among nurses by inoculating the nipple with the syphilitic virus. Affected with a genital chancre, they place their fingers upon the diseased part, thereby contaminating them; and then, without previously cleansing them, they take hold of the nipple, more or less excoriated, and thus implant a chancre, which they do not fail to transmit to the nursling. The position of these mammary chancres, of which I have recently seen a very fine example in the service of M. Cullerier, at Lourcine, is well explained by the manner in which these women seize the breast in order to present it to the child. I have caused a fine specimen to be engraved in the nineteenth number of the *Clinique Iconographique*.

The following is another mode by which contagion is propagated among nurses. I have met with an instance in which a nurse contracted a chancre on the nipple through an individual affected with a primitive sore of the lip, who, with the object of rendering the woman a kind office, applied his mouth to her breasts, and thus emptied them. Quite recently, a young man was lying in my hospital, affected with a primitive ulcer of the nipple, with multiple and indolent engorgements of the axillary ganglia, followed, at the end of six weeks, by engorgement of the posterior cervical ganglia and a confluent roseola. This person was contaminated by his mistress, who, with a chancre of the lips, had, by eccentric kisses, indulged her inclinations on her lover.

Here is another instance. A nurse came from the country to Paris, all aghast, for the purpose of claiming damages for a syphilis which, she said, she had contracted from her foster-child. This woman had, in fact, an indurated chancre upon the

* The Hôpital du Midi was erected by the Capuchin Friars, and used as a monastery.—TRANS.

inner side of each breast; these chancres directly faced each other. As to the nursling—the *rotten* child, according to the nurse—it was simply affected with a common porrigo larvalis. The parents, but little satisfied with the truth of the accusation, and particularly with the justice of the demand, and being moreover in perfect health, resisted the pretensions of the nurse, from whom I obtained a formal confession. A man, *who was not her husband*, afraid of making her pregnant and altering her milk, had indulged himself with her in a manner which the pen refuses to trace.

A child may contract *chancres* at birth, if its mother is affected with them at the moment of parturition. Such a case is undoubtedly rare, but it is not impossible. These chancres, which are generally confounded with secondary accidents, on account of their varied and unusual seats, constitute, as may easily be conceived, foci of infection for nurses, and are afterwards given as proofs of the possible contagion of secondary accidents. What may, besides, tend to support this view of the case is the fact that the opportunity to trace the source of the child's infection may have passed by; the primitive accidents which the mother had at the moment of accouchement having had time to cicatrize, even without leaving any trace by which they may be detected. Then, if the *legal* father happens to have a remembrance of some blennorrhagia in his early youth, everything is placed to the account of hereditary infection! But what is to be said when no positive results are arrived at, and no confessions can be elicited?

Children at nurse may be infected by strangers who are themselves unsuspected. They may subsequently infect their nurses, and before the latter have noticed the disease in their foster-child, and especially before they have time to recognize its nature, and to observe their own symptoms, secondary accidents, so prompt to develop themselves in young children, may have supervened, and so masked the true starting-point as to make it finally undiscoverable. I remember a remarkable case of this kind in relation to which my learned associate and friend, M. Richet, surgeon to the *Hôpital de Lourcine*, consulted me some years since. The subject, a little daughter of a Parisian merchant, yet under the care of her nurse, was affected with syphilitic ulcerations of the *ano-genital regions.* The parents, as well as the nurse, were perfectly *healthy*, although the latter had been suspected. The question was, whence could this contagion have been derived? It was finally ascertained that a clerk in the house, who was really diseased, had been in the habit of seating this child naked upon his hands, which he had not taken care to wash, and which thus came in contact with the diseased parts. Had it not been for this discovery, how could the disease of this little girl have been explained, and who would have been accused had the nurse presented any trace of the existence of syphilis?

In all these cases, by means of skill and perseverance, the primary source of the accidents has been discovered. But the case is not always thus fortunate. The mother of the child is perfectly healthy; the *husband* of the mother is irreproachable; the nurse is beyond suspicion; yet, in the face of all this, a nursling becomes syphilitically diseased. Whence arises the contagion? Allow me to cite a case which may serve to elucidate this delicate question.

A young woman, accompanied by her husband, much older than herself, came to consult me relative to her child, which she had just taken from its nurse, and which was infected with a constitutional syphilis. The mother accused the nurse of having communicated the disease. The child was covered almost entirely with a humid squamous syphilide; the lips, and the circumference of the anus were the seat of ulcerated mucous papules. The child was six months old; according to the nurse, the first symptoms of the infection were observed six weeks after its birth.

The mother and the *husband* declared they had never been infected; and the most attentive examination enabled me, in fact, to discover no proofs either of present or of past disease. The nurse was in turn examined with the greatest care, and seemed perfectly healthy. Her child, which she nursed at the same time, was well.

I was much embarrassed in regard to the cause of this child's syphilis; but on the next day, I received a visit from a young cavalry officer, who came to consult me about a palmar and plantar syphilide with which he was affected. This officer interrogated me with touching solicitude concerning the disease of the child which had been presented to me the day before, and confided to me his reasons for being interested in the matter. But he was unacquainted with the laws which govern the hereditary transmission of disease, and was therefore surprised when he found that he had occasioned the birth of a diseased child; especially, said he, as he had no symptom of the disease, and thought himself cured when he had had intercourse with the lady, who had never been infected.

After all I have just said, my dear friend, you will see how much care and attention, how much reserve and prudence, are required before we can accept, as a demonstrated fact, the doctrine of the contagion of secondary accidents. Do you not agree with me that other facts than those at present recorded in the annals of art are needed before we can definitively establish this principle as a law of syphilography?

Yours, RICORD.

LETTER XIV.

MY DEAR FRIEND: What did I attempt to prove in my last letter? That, aside from primitive accidents, the transmissibility of syphilis from nurse to nursling, and from nursling to nurse, was far from being demonstrated by observation; that nothing is less established than this assumed contagion of secondary accidents; and that, in all the cases adduced as proof of the fact, either the details requisite to produce conviction were wanting, or the result merely indicated the existence of primitive accidents.

Observe, I pray you, that I do not absolutely cast aside this mode of transmitting syphilis; that I do not deny its possibility; and that, as I shall hereafter show, I have really no interest, so far as doctrine is concerned, in denying it. I simply assert, still clinging to the results of severe induction, and of the rigorous analysis of facts, that observation has yet failed to demonstrate its existence; and I may add that irrefragable proof of such transmission can be obtained only by means of inoculation.

"But," it is replied, "do you then forget the fact that some persons have pretended to prove, by means of inoculation itself, the contagious properties of secondary accidents?" I certainly do not forget the fact. I wish I could do so, indeed; for then I should not be under the painful necessity of doubting, with too much reason, I fear, the accuracy of experiments made by men for whose works I have high respect, but who appear to me to have drawn somewhat precipitate conclusions. Let us examine the matter.

Wallace published two cases of secondary inoculation, accompanied by what appear to have been positive results. This syphilographist states that, in healthy individuals, inoculated with pus taken from patients under the influence of secondary accidents, he has produced primitive accidents, followed by confirmed secondary accidents. It is very certain that, in relation to the effect produced, the observations appear, at first sight, to have some semblance to truth. But what is not at all demonstrated is the nature of the accidents reputed to be secondary in the patients from whom the inoculated pus was taken. In this respect, the most important details are wanting. Wallace contents himself by saying, with respect to the first case, that the patient had pustules of syphilitic *psydracia*, dating back fourteen days. In the second patient were also seen psydraceous pustules, of four weeks' standing, which formed little crusts. In the first case, the subject was inoculated on the shoulders; in the second, on the prepuce.

But, in the first place, it is not proved that the pustules from which Wallace took the pus were secondary accidents. The form, the number, and the seat of the pustules do not enable one to affirm that they were of this character. Some additional element is required for this purpose, and this element we fail to find in the observations of Wallace.

On the other hand, what precautions were taken, after the inoculations were made? If in a venereal hospital—where virulent matter is found everywhere, and where subsequent contact is easy—a puncture, after inoculation, is not protected from all contact by placing it under a watch-crystal, or by some equally efficacious means; if the instruments used have not been washed with the greatest care; if, in a word, the most minute precautions have not been taken to guard against contact, it is impossible, in circumstances which involve such important results, to deduce exact conclusions from our experiments.

I am so much the more particular with respect to the above observations of Wallace, since the results of inoculation were of an unusual character.

In the first subject, inoculated on the 15th of November, it was not until the 14th of the following December that a small papule formed over the punctures. This papule was immediately covered with crusts, below which a small superficial ulcer was observed. Thence was witnessed the evolution of the symptoms described by Wallace, and which clearly might have had another origin.

In the second patient, inoculated on the prepuce on the 1st of June, it was not until the 28th of the same month that a small, dirty, yellow crust, surrounded by an areola, was perceived at points which, up to that time, were unaffected. The glands of both groins swelled; the point covered with the crust was scarcely excoriated. On the 24th of July, the whole body was covered with an exanthema, which appeared to be of a syphilitic nature. At a later period, accidents were discovered at the anus, the origin of which was not determined. From the description, these accidents undoubtedly much resembled mucous papules; and these papules also existed upon the scrotum, upon the back of the tongue, and upon the tonsils. But the raphe of the patient was *red, and very much tumefied;* he asserted that, in walking, a *very considerable discharge escaped from the anus.* Now, tumefaction of the raphe, and intra-anal suppuration, are very often found in cases of chancrous ulceration of this region. The primitive accident, contracted *à prepostera venere*, is most generally developed at the anterior part of the anus where the raphe terminates. It is therefore more rational to infer the existence, in this patient, of a primitive accident at the anus, of which no previous notice was taken, than to attribute the commencement of the disease to the affection of the prepuce, which presented none of the signs by which syphilis is characterized. Finally, I may add that, in inoculations well performed, the evolution of the symptoms may be slow at times, but it is always continuous; and we never observe intervals of a month or twenty-eight days between the date of inoculation and that at which the accidents supervene.

Then, my dear friend, what grounds for doubt exist in relation to these two observations of Wallace? After the analysis of them which I have just presented, I cannot think that they still serve to support the doctrine that secondary accidents are inoculable.

I have just spoken of the possibility of the existence of an anal chancre in the second patient. This supposition seems to me the more plausible from the fact that in England chancres are seldom sought for in this situation. The medical habits of the English physician reflect the peculiar false modesty which characterizes the nation. I recollect that, while on a visit at St. Bartholomew's, in London, men and women supposed to be affected with secondary accidents were eagerly pointed out to me. Of these accidents no other explanation had been found than a contagion *d'emblée.* My friend, Dr. Acton, was present. You are aware that I have very little confidence in this contagion; so, profiting by my *right of visit*, I commenced my researches. I still smile at the frightened air of the surgeon and his aids when, carrying an indiscreet finger and a searching look into certain mucous folds, I succeeded in discovering a back door in the *perfidious Albion.* I ought to add that the surgeon immediately cast a veil, or, in less poetical language, let fall the sheets over these too visible stigmas of an easily explained contagion.

To return to Wallace. It is singular that the physician who performed so many inoculations should succeed in these two cases alone, and that even these he should have described so badly. These cases would constitute the exception to the rule; but there can be no exception to the rule. Secondary accidents are either inoculable or they are not. Will you call to mind what I said in relation to the pretended exceptional blennorrhagias of Bell? For these there could be no exception; and experiment has proved, in fact, that *exceptional cases* come under the law of inoculable chancre.

But if the statement of facts, observed on the other side of the Channel, is, as I think I have proved, liable to very reasonable doubts, there is one which occurred in my neighborhood, and which appears to be of greater value.

It was even at the *Hôpital du Midi* that this fact occurred. It might seem improper for me to mention it to you, had not an interested, a too interested party, given me permission to do so.

The point concerns the transmission of secondary accidents from a diseased to a healthy individual. The inoculation succeeded perfectly. One of our associates, who, though not *casuistical*, is nevertheless not favorable to experimental researches, inoculated the patient himself, planting upon each forearm of one of the internes of the hospital a chancre which became indurated, which produced an indolent engorgement of the axillary ganglia, and which, within the subsequent four months, gave place to perfectly characteristic secondary accidents: nocturnal cephalalgia, alopecia, scabby eruptions of the scalp, mucous papules of the velum palati (psoriasis of the mucous membranes), &c.; that is to say, to a constitutional pox of the most incontestable character.

But—and this comprises the whole question—what was the nature of the accidents which furnished the inoculated pus?

The patient from whom the inoculated matter was taken, according to information furnished me by the inoculated interne, was affected with an indurated chancre of six weeks' standing, which had cicatrized; he had mucous papules in the anus; rhagades on the great toes; large agglomerated pustules upon the thoracic region, which were covered with crusts, beneath which *constantly progressive* ulcerations were seen; some others also existed in the inguinal regions, and on the side of the chest where the principal group was situated.

Before the student was inoculated, the matter of these pustules had been used to inoculate the two thighs of the patient himself; and this inoculation gave a positive result, a circumstance which, *without a great love of experiment, should have prevented the inoculation of a healthy individual.*

This individual, then, had undoubtedly a constitutional syphilis, and presented characteristic accidents of an incontestable nature. But in him, *were all the accidents necessarily of the same nature?* Constitutional syphilis, it is well known, in nowise hinders the contraction of new primitive accidents—of accidents unlimited in number, and infinitely varied in their seat. In this particular case, the accidents from which the pus was taken were *constantly increasing ulcers*, covered with a very extensive crust, and occurred in an individual who was under the influence of a syphilitic diathesis only six weeks, and in whom was presented, in other regions, the regular evolution of the secondary accidents of this period. With respect to this case, then, permit me to urge a doubt, which, so far as the student who underwent the inoculation is concerned, is at present a certainty; to wit, that the accidents from which the pus was taken *were not secondary accidents.*

I did not see the patient who furnished the inoculable pus, for he left the hospital soon after the experiment was made, and the *interested interne was unable to find him.* But the importance of this fact, howsoever it may be contested, induced my honorable associate M. Puche, and myself, to recommence a series of experiments relative to the inoculation of secondary accidents. We have already made twenty experiments, all of which have yielded us, as formerly, merely *negative results.* The inoculations have been made with the pus of mucous papules, of ecythma, of rupia, of ulcerated tubercles, of secondary serpiginous ulcerations; but in no instance have we obtained any definite results. On this subject, I shall present two curious cases which were witnessed by the numerous students who attend my clinique:—

Two patients, Nos. 16 and 17, lay side by side. The one, No. 16, had a scabby eruption of the axillary region, which was progressive and serpiginous. No. 17 had an ulceration of the right posterior and lateral region of the neck, from six to eight centimetres in diameter—this ulceration healing in the centre, and extending in circumference. He was also affected with isolated rupia, and grouped ecthyma; and upon the greatest part of the body and members were characteristic cicatrices resulting from the pustulo-crustaceous syphilides.

These patients were inoculated on the thigh. In No. 16, the inoculation succeeded; success was predicted. In No. 17, we announced that the inoculation would be *negative.* Our prediction was verified. Why? Because the ulceration of No. 17 was truly secondary; whilst, in No. 16, the scabby ulcerating eruption of the axillary region, which appeared like the pustulo-crustaceous ulcerations peculiar to constitutional syphilis, had been itself the result of an inoculation, and I will tell you how. This patient at first had a scrofulous abscess of the axilla; the abscess was opened in the hospital, and was dressed with difficulty. One of his neighbors, affected with a phagedænic chancre of the genital organs, assisted him in dressing it; and with the fingers of his friend, which were contaminated with virulent pus from his own chancre, he had been inoculated with specific matter. Without a very precise etiology of this case, this accident might have been attributed to the syphilitic diathesis, and have been given as an example of secondary inoculation, inasmuch as the patient himself had formerly labored under constitutional syphilis.

How much care and precaution, then, are necessary in order to avoid error!

Yours, RICORD.

LETTER XV.

MY DEAR FRIEND: After numerous and careful researches; after many experiments made by myself; after the still more numerous ones made in imitation of mine, I am justified in drawing the conclusion that, *up to the present time*, secondary accidents have not been shown to be inoculable. I have told you of the experiments which I have recently made, and which have been confirmed by MM. Puche and Cullerier. But these experiments having always been made upon the patient himself, there was ground for a capital objection. It could be said to me: "Persons already affected with secondary accidents cannot be inoculated; but this is not the case with healthy individuals." This objection might be made by those who embrace my doctrines; but I do not think it has entered the mind of any one belonging to the school which is opposed to mine, and which, so far from teaching that a constitutional syphilis prevents a new contagion, considers that it is simply necessary to make a wound on a syphilitic patient, in order that this wound should immediately take on a venereal character. I have already spoken to you, and I will soon ask permission to repeat, my sentiments on this subject. Be this as it may, the first objection yet remains; and, if the observations of Wallace had been more truthful and less contestable than they proved to be, I should have had trouble in replying to them, for I did not possess any contradictory experiments.

It was under these circumstances that I observed the case of inoculation of which I gave you a synopsis in my last letter. I mentioned this fact on the express authority of the person most interested in the case—of him who voluntarily submitted to the trial, and who suffers the consequences of it. This person is not a patient in the hospital, and belongs to no service. With undoubted right, he considers this fact his scientific property, and thinks that he has become its absolute owner; and, thus believing, that he has the right to draw from it such scientific and practical consequences as he shall think fit, leaving to all the liberty of doing the same. It is under these circumstances, I say, that I thought it lawful and honorable to state what I think of this fact.

I repeat, then, that this fact appeared to me to be one of grave importance, and highly worthy of consideration; and therefore I wished to examine it with care. Common and valueless facts do not fix the attention. This one derives its importance both from the nature of the experiment, which has great influence in the elucidation of weighty practical questions, and from the person who submitted to the experiment. The latter was an interne in pharmacy, a distinguished and intelligent student, who is engaged in the study of disease, especially of syphilis. In my eyes, the fact merits our attention, in its relations to the experimenter, whose talents, scientific knowledge, and general character, especially, I have never desired, you are aware, to attack. This statement, were it necessary, you could attest. I have ever profoundly despised attacks of this nature, not only because they have often been directed against myself, but because they are repugnant to my nature.

In these letters, conceived and written hastily, my language may sometimes exhibit a want of kindness; but my intention is ever pure. Let this be said, once for all, in order to lull those susceptibilities which have no occasion to be touched.

I return to the scientific fact which is the special object of my inquiry. All the value which belongs to this fact lies in the diagnosis. Was a healthy individual inoculated with the pus of a primitive or of a secondary syphilitic accident? I believe, and I have given my reasons for my conviction, that from this circumstance alone, namely, that the patient was positively inoculated himself, the experiment enters completely into the domain of that class of experiments which I myself have made.

Success, then, in this case, according to my experience, depended on the fact that the experimenter had to deal with the pus of primitive accidents. At least, I do not contest the fact —*but it is one that is yet to be demonstrated*—that, for the inoculation of secondary accidents, there has been found a particular form, a special period, which up to the present time has escaped recognition, and which can be definitely determined.

For, in fine, this result cannot be the effect of hazard. If it can be determined under what circumstances secondary accidents can be inoculated, a great step will have been made in syphilogeny, and a great service will have been rendered to science. In any case, this experiment will confirm the law— that an accident really contagious is inoculable; and that there is no difference between artificial and physiological inoculation. It will prove that this means of experiment is not wholly devoid of value; and it is with genuine pleasure that I see even those who have best exhibited *the uncertainties and the difficulties of syphilitic inoculations* embrace this opinion.

Allow me to say, my dear friend, that I have no intention, as you clearly see, of changing positions. I do not attack any one; I simply defend myself. I do not criticize; I examine. I do not covet the success of the polemic; I confine myself to the more modest pretensions of the observing practitioner. No one is more willing than myself to receive light, from whatever direction it comes—to recognize the truth, whoever may proclaim it. I have always stated, with firmness and integrity, what I have known, or what I have thought I knew. My experiments have never been made with closed doors. As soon as made, they have become the property of all. Every one has had the privilege of witnessing them, of judging concerning them, and of discussing them; and, in so doing, they have committed no error, for the right to do so is common to all, without my permission. I have held opinions which time and experience have modified; and I will cite an example which is appropriate to this confession.

With all distinguished syphilographists, of past and present times, I believed that syphilis was not transmissible to animals. I made experiments which, like those of Hunter, of Turnbull, and especially of M. Cullerier, who made the greatest number of them, always led to negative results. All these experiments gave me the right to infer the non-transmissibility of syphilis to animals, until the contrary should be proved.

Nevertheless, I *was not in too much haste to teach and to publish these negative results*, as M. Robert de Welz has imagined, when I had with me the essays of Hunter, of Turnbull, of M. Cullerier, and particularly the numerous publicly established failures of M. Auzias-Turenne. M. Auzias had, perhaps, experimented more than all of us, and had arrived at more numerous negative results. But, more persevering in his researches, he studied the conditions opposed to the inoculation of animals. These he recognized, and has finally been able to transmit primitive accidents from man to the monkey, and reciprocally from the monkey to man. M. Auzias at first observed that one of the principal causes of failure depended upon the fact that the animals licked the wound after inoculation. He believed in the doctrine that the saliva neutralizes the virus; but this opinion could not be sustained in view of the numerous cases in which the seat, the lips, the tongue, and divers points of the buccal cavity, are observed to be the seat of primitive accidents. The whole secret was this: the animals licked, and of necessity thus cleaned, the wound.

But the true reason of the failure of the experiment, and the one on which M. Auzias-Turenne at present especially insists, is the greater plasticity of the blood in animals, which allows the lymph to interpose between the bleeding part and the virulent matter. By taking care to apply pus constantly to the puncture, the inoculation has succeeded. I witnessed these experiments, and can guarantee their authenticity. I am glad that I can rectify this point in the history of syphilis, by means of my clinical lessons.

Up to this period, I had taught, with my predecessors and with my contemporaries, that syphilis was the sad privilege of man, and that in him it was not spontaneous. I have always strongly insisted upon these two seemingly contradictory facts—speciality of the disease relative to man, and non-spontaneity. I have always thought that syphilis had an origin somewhere; and that it was necessary to search for this origin. Is the problem solved? Monkeys have not always escaped evil designs. Linder and Overcamp had already accused them of imparting the syphilitic disease to the human species; but, prior to the time of M. Auzias, Linder and Overcamp were considered calumniators of the monkeys. Were they right?

It is at least an incontestable fact that, since man has been acquainted with the habits of monkeys; since he has seen them flourish at the Garden of Plants, in Paris, and in other capitals; since he has observed and studied them, either in a state of nature or in captivity, nothing has been noticed among them which resembles primitive syphilis, and nothing especially which resembles constitutional syphilis.

Nevertheless, M. Auzias has succeeded in planting a primitive ulcer upon the ear of a monkey. The pus which was used in the experiment having been taken from a patient in my ward, I ought to mention with care the circumstances under which it was collected. The patient was affected with confluent chancres of the glans, of the sheath, and of the scrotum—non-indurated chancres at the period of specific progress. These chancrés resulted from a recent contagion, in an individual already under the influence of a constitutional syphilis, at the secondary period; and it is important to note this fact, for, according to the principles I have laid down, it explains why the chancres were not indurated in this patient. Moreover, these chancres, by their *multiplicity and by their variety of seat*, might, by inattentive or superficial observers, have been confounded with other constitutional accidents, and have furnished a pretext for inferring the possibility of the inoculation of secondary accidents. A previous attempt to inoculate had been made with success. The monkey was inoculated, the first time, with pus from the pustule of inoculation; and was subjected to a second inoculation with pus from his first pustule; the latter inoculation was also successful.

At this period, one of our young associates, M. Robert de Welz, assistant professor to a German university, asked to be inoculated. He was inoculated, first with the pus of the first pustule of the monkey, and afterwards with that of the second. Both experiments succeeded.

But, up to this time, the patient who originally furnished the pus had no specific induration. The monkey, whose pustules were somewhat thickened, *did not present the specific characters of this induration; the neighboring ganglia were not engorged.* Finally, our German associate, who had voluntarily submitted to this perilous experiment, and in whom the pustules of inoculation were only tardily destroyed, had no *specific induration.* The pustules of inoculation presented at their base a very common sub-phlegmonous engorgement; but one which may be often confounded, by inexperienced observers, with specific induration. The axillary ganglia (the punctures being made on each arm) were not engorged.

For the experiment, at which I was present, and which was made upon M. Robert de Welz, a new lancet was used; but the spatula, with which the pus was taken from the monkey, was an old one. Afterwards, M. Robert de Welz made another attempt to inoculate, which succeeded; both of the instruments being new.

Thus far, then, there were only purely primitive accidents, essentially local; but these do not constitute the pox. In the monkey, was the inoculation merely the transplantation of the chancre? We have the right to think so until constitutional accidents can be produced in him. This opinion is so much the more warrantable, from the fact that many syphilographists, especially in England, assert that the chancre which does not become indurated is not a syphilitic accident. Will M. Auzias's experiments confirm this opinion? I will tell you my opinion hereafter, and also what I think of the induration of chancre.

Meanwhile, I will state that, if *primitive accidents*, which are incontestably transmissible from man to man, can be transmitted to monkeys, *secondary accidents* must also be transmitted, if, perchance, they have recently become inoculable.

May we assume, then, for each specific disease, as for epidemics in general, a versatile genius? Or, is it the genius of the observers which changes?

Yours, RICORD.

LETTER XVI.

My dear Friend: It is clearly impossible to please the whole world; and this old maxim is especially worthy of remembrance when the question concerns medical science.

The monkeys have brought misfortune upon me: I have failed to satisfy those experimenters who assume to have inoculated them with syphilis; and yet much less have I satisfied those who do not believe in this pretended inoculation.

Nevertheless, see how one may be deceived. I had the simplicity to think that from both parties I deserved encomiums. You are about to know in what consisted my error.

The young Bavarian *confrère*, who has recently connected his name with the inoculation of syphilis, reproaches myself and others with *being hasty in our conclusions relative to the non-transmissibility of syphilis to animals.* However, if I am not mistaken, a little more than *twenty-four hours* have elapsed since the time of Hunter, and we have certainly had enough time to reflect and to prevent precipitancy.

On the other hand, associates whom I love, and who usually entertain the same opinions as myself, have cast upon me almost the same reproach. They find that I was too hasty with the monkeys; they believe—they have told me so—that I permitted myself to be carried away by their tricks. My learned and skilful colleague of the *Hôpital du Midi*, M. Puche, is still in a state of perfect incredulity relative to the transmission of syphilis to animals; and that persevering experimenter, M. Cullerier, has equally little faith in the reality of the experiments.

What I related to you in my last letter, I saw with my own eyes; I stated to you also the extenuating circumstances which I was unable to conceal; satisfied, however, both of the earnestness and of the good faith of M. Auzias-Turenne. But, after all I have told you relative to the inoculation of the monkey with virulent pus from man, *I am surprised at the premature conclusions which our German confrère has drawn from the circumstance;* and I must frankly state that he who exacts of others so much maturity of reflection, is not an example of that which he requires. After all, the promptitude of his conclusions may find excuse in the very inoculations to which he so courageously submitted, and which he does not wish to consider that he has made in vain.

Our German *confrère* places much confidence in this proposition: "*A single positive experiment has more value than an innumerable quantity of negative results.*" Undoubtedly, but upon one condition—that this experiment be *positive;* that it be incontestable; that it present every guarantee of certainty and exactness; and farther, that *it can be repeated.* Unless these conditions are fulfilled, it amounts to nothing at all. The Academy of Sciences well understands the value of this proposition, which is so often repeated, and which rash and new experimenters periodically bring forward to overturn the laws of physics. This argument has been put forth in favor of all human deceptions.

What says the magnetizer, who pretends to be able to trans-port the sense of sight to the nucha or to the epigastrium? Precisely what our German *confrère* says relative to the value of a single positive experiment.

What says the homœopath, who maintains that an atom of bryona, diluted in the immense waters of the ocean, can cure pneumonia? Absolutely the same thing as our German friend.

In the physical and natural sciences, *an isolated fact amounts to nothing, unless it is susceptible of repetition.* This is the opinion of all who know what constitutes the philosophy of the sciences. Such an hypothesis would prove a dangerous stumbling-block to progress, should patient and laborious observation only succeed in proving that the assumed fact was merely a sophism or an error.

My honorable colleague and friend, M. Cullerier, will tell you his own opinion of the experiment of M. Auzias. As for myself, I established this fact: Virulent pus was transmitted from a man to a monkey, and it was then used to inoculate a man. I proved nothing more and nothing less. That is the crude fact; then comes its interpretation.

I asked in my last letter: "*In the monkey, was the inoculation merely the transplantation of the chancre?*" This is my conviction; for, observe what happened: The puncture of inoculation made on the monkey exhibits scarcely any irritation or inflammation, and suppurates very little, although saturated with virulent pus. It constantly tends towards cure, which takes place with astonishing rapidity. We fail to see, in the inoculation of the monkey, that progressive ulceration which characterizes chancre in man, especially the non-indurated chancre. We do not even find that prolonged and tenacious specific *statu quo* which nature maintains in man, and which art ordinarily destroys with so much difficulty. Never in the monkey is there exhibited the least phagedænic tendency, or anything which resembles the specific induration. A puncture, scarcely a slight suppuration, a scab, and the cure!—these are the products of inoculation in the monkey, and succeed one another as rapidly as his gestures. It is evident that this is refractory and foreign ground for chancre; the virulent seed is exotic; it is well to take precautions in sowing it, in *watering* it, in putting it in a greenhouse or under glass; it dies before sending out roots, and with greater reason before having borne fruit.

M. Auzias explains all this by the greater vitality of monkeys; by the greater rapidity of their circulation. It would be easier to explain the fact by the antipathy of their nature to syphilis, on which I congratulate them. It might even be believed, with respect to the pustule which is produced with such difficulty, that the virulent pus acts only as the pea of an issue, which irritates and occasions suppuration, but does not combine with the tissues. It is mixed with the pus produced; that is all. It would, in fact, be essential, in order to arrive definitely at another opinion, that the pustules produced upon the monkey should be ruptured; that the ulcerated surfaces should be washed, in order that none of the chancrous pus may remain in the mixture; and that the suppuration subsequently furnished by these surfaces be inoculated. What takes place in man is well known. The surfaces of chancres may be washed; medicinal agents may even be applied to them; and still the virulent secretion continues to be produced. Inasmuch as this experimental programme fails to be thus filled up, a single experiment is insufficient to destroy all that distinguished men, from numerous and carefully studied facts, have established. With respect to this matter, the only acquisition which remains to science consists in the fact that we can inoculate the monkey with virulent pus, and afterwards make use of this pus to inoculate man, in the same way that a plant may be transplanted from one soil to another. This is all which appears to me to be established, and the only deduction I have been able to draw from the facts in the case.

Consequently, our Bavarian *confrère* may consider his inoculations in the same light as though they had been made with virulent pus preserved in tubes or between glass. This leads me to describe the effects which result from the inoculation of man with virulent pus, and to show what this inoculation teaches us concerning the pathogeny of chancre.

Yours, RICORD.

LETTER XVII.

My dear Friend: I think I have paid quite enough attention to monkeys. For the present, I shall cease further to notice them. If, hereafter, it should be proved that they can contract something different from what I have stated, I shall be always ready to acknowledge the fact. Thus far, I see no reason for changing my opinion.

In the mean time, let us return to poor man, whose right of possession to the pox is contested by nobody.

Yet permit me, before proceeding farther, after what I have already said, and perhaps even on account of what others have recently advanced, to establish the following proposition, which seems to me irrefragable:—

Chancre (the primitive ulcer) at the period of progress, or of specific *statu quo*, is the only source of the syphilitic virus (the inoculable morbid poison).

I have already told you what conditions are essential for the activity of virulent pus. You also know what conditions are requisite to enable the parts to undergo this action. Let us now study the effects of this action, or, in other words, the pathogeny of chancre.

This is a serious, but somewhat dry subject. I trust that your kindness will induce you to follow my developments; but do not expect to derive any interest from what I shall state, except such as shall grow out of the strict consideration of the question.

If, with a lancet charged with virulent pus, a puncture is made under the epidermis, this puncture, which ought scarcely to bleed, soon reddens and becomes prominent; its summit is raised by serosity, which soon becomes cloudy, that it may afterwards take on the characters of pus.

Thus a puncture, a papule surrounded by an areola, a vesicle, a vesico-pustule, and finally a pustule, are, consecutively, the constant phenomena produced by inoculation.

All this takes place without interruption or pause, from hour to hour, from day to day, forming, so to speak, a pathological ribbon, which is incessantly unrolled, in order finally to reach a regular and inevitable conclusion; that is to say, the production of an *ecthymatous* pustule of the most perfect and typical character.

This pustule is often depressed at its summit, and umbilicated even at the point corresponding to the puncture, where a small drop of dried blood is usually perceived.

If the pustule is not broken, the pus which formed it dries, producing a conical, brown, greenish, or blackish scab. This scab tends to increase at its base; for it covers an ulceration whose circumference itself tends to increase. In this increase of the ulceration under the scab, the epidermis of the areola which surrounds it is successively raised by suppuration, which, in turn, dries to form a new disk of scab; whilst a new areola is formed about the circumference; and this succession of phenomena is constantly going on.

Tell me, without ceremony, my dear friend, whether I am sufficiently clear in this description; it is highly necessary that I should be well understood.

The red circle (the areola) which borders the scab is usually tumefied, and encloses it in the same way as the ring of a watch enchases the crystal. But, as there is here a progressive ulceration, as new pus is constantly produced, and as the circumference of the scab is always softer than its centre, this scab is not ordinarily very adherent.

Sometimes the scab is very soon formed. At others the pustule persists in a purulent state for a period of greater or less duration.

This pustule may not attain to a very large size. At first, it is often no larger than a lentil. At a later period, its extent may equal that of a five-cent piece; or even that of a piece four times as large: but it is not rare to see it acquire much greater dimensions.

The pustule, then, exhibits those transitions which are so often observed in other forms of pustular disease, and which give it the aspect of rupia, either before, or subsequently to the formation of the scab. Moreover, as we occasionally observe in rupia, the only difference we perceive among these pustules is that of size.

If the pustule is broken on the second or the third day, in cases of rapid evolution, or later in ordinary cases; or, if the scab is detached, there is found underneath it, occupying the whole thickness of the skin, a perfectly rounded ulceration, with perpendicular edges (*tailles à pic*), as though it had been made with a punch.

The borders of this ulceration, a little undermined, notched, and turned up, remain surrounded by the red areola which constitutes its margin; they are covered by a diphtheritic layer, a special adherent pyogenic membrane.

The surface of the ulceration secretes an unhealthy pus, which is sero-sanious, often reddish, and loaded with organic detritus. This is the virulent inoculable pus. When this surface is cleansed, a diphtheritic layer is found, which is more marked than that observed at the edges. This layer, likewise formed of a special pyogenic membrane, is of a grayish color, of a lardaceous aspect, and which cannot be detached from its adhesion.

Moreover, the bottom of the ulceration rests on a base more or less thickened, and more or less engorged, according to the course which the ulceration is about to take—a course determined especially by the *soil* in which the *syphilitic seed* has been sown.

The ulceration which I have just described, and which has pursued an onward course, may be arrested, when it reaches the extent I have already indicated, for a period of greater or less duration—one month, six weeks, and more; or, it may continue to increase, and acquire greater dimensions, thus presenting important modifications.

In the numerous inoculations which I have practised, the following sequence of phenomena has always occurred: Incessant evolution from the time of the puncture; the constant production of an *ecthyma*, with an ulcerated base, which, in turn, presents, in a marked manner, the typical characters of chancre; ulceration with a *progressive tendency*, or obstinately remaining *stationary*.

You already see, my dear friend, how artificial inoculation overturns all that has been habitually taught and believed for centuries; how it strikes in the very breach the physiologism of Broussais; how also it reduces to its just value the more recent doctrine of *physiological contagion.*

Can the theory of incubation be maintained, in view of the known effects of inoculation; in view of results which can be subjected to daily proof? For, be it observed, it is not a unique, exceptional fact which I relate, but a mass of identical facts, which always give rise to the same phenomena. The proof of these facts is at all times available.

The *electric expansive mode* of Bru is exploded. It is no longer possible to believe that the syphilitic virus penetrates the economy like a flash of lightning; that it is a shock given by the infecting to the infected individual. Chancre, the primitive ulcer, is no longer the result of *a return stroke.*

No one who is not blind can admit, at the present day, that the virulent pus traverses our tissues, by a solution of continuity or otherwise, to infect first the entire economy; that it broods at a distance; and that it afterwards retraces its steps *to be hatched* in the *nest* where it was at first deposited.

The syphilitic virus is a specific seed, and grows where it is sown. It is a *specific ferment,* and the parts which it immediately touches are the first that enter into fermentation. All this takes place with greater or less rapidity, as we have already stated, and depends on the nature and aptitudes of the soil: but the point at which it takes place is, at first, very circumscribed; and this limited sphere we shall henceforward, perhaps, succeed in still further limiting.

The non-existence of a period of incubation is not yet an accepted, though a very evident fact. Prejudice has acquired so much age as to have the force of law; and it is now a difficult task to overcome it.

Those, however, who believe in incubation, and who think it an essential element in the virulence of syphilis, have, by way of objection, said to me:—

If you obtain direct and instantaneous effects by artificial inoculation; if you have observed merely a local evolution; if you have perceived nothing which betrays a general participation of the organism in the syphilitic affection, it is because you operate upon an already infected organism; it is because you inoculate patients who have been already inoculated.

This objection, my dear friend, comes, you see, within the famous theory of the *virulent bottles*. I have already refuted it; I told you what must be thought of it when viewed in relation to wounds, cuts, and operations made upon those affected with syphilis. I cannot be forever returning to it. Permit me to refer you to what I have already said on this subject. But I have another reply to make to this objection, independent of that which is based on experiments practised upon patients themselves. I will reply to it by adducing cases of inoculation of healthy individuals with matter taken from those who have been diseased, and I shall especially invoke the recent inoculations made upon man as well as the monkey. Now, in these cases, the results of inoculation have proved to be identical with those which I have just described to you; namely, immediate action, uninterrupted evolution, and the production of the ecthymatous pustule.

But, does artificial inoculation always give place to this consecutive series of phenomena? Are there not cases in which some time elapses between the period of inoculation and that at which the symptoms begin to manifest themselves—some interval corresponding to that observed in inoculation with the vaccine virus? In ordinary contagion, does not a tolerably long time seem to elapse between the inception of the disease, and the manifestation of its effects?

Undoubtedly, and such cases may have justified the inference that, in some respects, the theory of incubation is a legitimate one. But, when we take the trouble to examine these facts with attention, we find that they have not been carefully appreciated. It is my present intention to attempt to reduce them to their just value, and to bring them under recognized laws.

I have already stated that I have never observed similar cases in my numerous public experiments. This result evidently depends on the uniformity of my method of experimentation. My honorable colleague, M. Puche, who has experimented no less, if not more, than myself, has only once or twice seen accidents manifest themselves after the second or third day from the puncture. Every one who has studied the inoculation of syphilis knows that, when it does not immediately succeed, it is because it is negative.

Nevertheless, we can conceive that a superficial puncture, in which the virulent pus is deposited on scarcely denuded surfaces, may require a somewhat longer period to produce its effects. The first puncture made by M. Robert de Welz was very superficial, and it failed to produce its effect after the first day; so that, in this case, there was something which might be said to resemble incubation. But the second puncture, which I made myself, followed the regular course. "What does that prove?" may be asked by those who support the doctrine of the influence of the general state. "The first puncture had a slow development, because the organism was not sufficiently impregnated. The effects of the second puncture were, on the contrary, rapid, because the virus had then invaded the whole economy." "This is all very fine," I will reply; "but here is a circumstance which slightly mars this beautiful theory. M. de Welz made a third puncture, which, too superficial like the first, gave the usual result."

This, my dear friend, is the explanation of the incubation. Without the assistance of this key, we understand how, in cases of ordinary contagion, virulent pus, applied to more or less denuded surfaces, soon gives rise to a more or less rapid morbid action. We know, by daily observation, confirmed irrefragably by the recent experiments of M. Cullerier, that virulent pus may remain in contact with healthy surfaces without producing any alteration in them, or without undergoing alteration itself; but we also know that surfaces constantly bathed with virulent, acrid, and irritating pus, which excoriates before it acquires a specific character, finally become eroded; and thus the pus itself produces those conditions required for successful inoculation. In this sort of vesication, a period of greater or less duration must elapse before those specific effects, which simulate incubation, appear.

For example, virulent pus is inclosed in a fold of the vulva, of the vagina, of the prepuce, or in the interior of a follicle;

it is only after the pus has been thus deposited, for a longer or shorter period, that, passing through the successive stages which I have just indicated, it acquires the power to inoculate. In all this, we observe no specific effect, but a physical and material fact, which visual observation daily demonstrates to those who are competent to discern. How many patients are there who, at first thought to be affected only with a balano-posthitis, finally exhibit symptoms of chancre! If to this circumstance, you add the negligence of patients; their well-known lack of observation relative to matters which concern themselves—in consequence of which they are prone to regard as the period of *incubation* the time which simply elapses between exposure to the cause of infection, and its apparent manifestation—you will find, my dear friend, with respect both to chancre and blennorrhagia, the explanation of those pretended incubations whose duration is of so elastic a nature as to vary between a few hours and several weeks, or even months.

You see that I am entering deeper and deeper into these important syphilographic questions. In my next letter, I shall treat of the different forms which chancre may assume.

May your favor, my dear friend, and that of your honored readers, still accompany me. This is my most valued encouragement.

Yours, RICORD.

LETTER XVIII.

MY DEAR FRIEND: I have related the history of positive inoculations in my last letter. When the inoculation fails, the punctured part is sometimes slightly irritated; but this irritation soon subsides.

Nevertheless, without seeking to deprive inoculation of any element of exactness which it may possess, it is necessary to recognize, with respect to syphilis, as well as vaccinia and variola, the existence of *false pustules.* If the examination of the case be slight, their existence may lead to error. My learned colleague, M. Puche, with honorable candor, acknowledges that he was deceived by these *false pustules*, when he formerly practised inoculations with the muco-pus of balano-posthitis. Consequently, he now accords less value than formerly to the observations contained in the *memoir* which he published on this subject. These observations he has studied with greater care, and for him they have changed their signification. You ought to understand, my dear friend, that I would not commit so great an impropriety as to speak thus without the formal authorization of M. Puche himself. My critics, then, who created so much commotion about inoculation with the muco-pus of non-ulcerous balano-posthitis; who used this supposed fact as a weapon against my doctrines; and sought to prove from it that chancre did not alone furnish inoculable pus, and that the blennorrhagia which inoculated might not be ulcerous, can no longer make use of this argument apart from the above verification, which its author deems indispensable.

These *false pustules* acquire but little development. They are most frequently only simple bullæ, beneath which there is perceived a superficial vesication of the skin. Here we do not observe that complete boring of the derma, like that made by a punch, which we find in true inoculation. In some rare cases, a more serious inflammation may supervene, and produce something analogous to furuncle; but even in these cases, its progress is always very rapid, and its duration ephemeral, from three to five or six days at the most. Not only so, but its cure is effected without treatment.

Be this as it may, I have stated and I still state that, when the inoculation succeeds, the chancre always commences by a pustule. This fact is incontestable. This pustule can be reproduced at will.

Those syphilographists who have associated with primitive accidents so many phenomena which do not belong to them, would have done well had they also placed among them this *ecthyma*, which is developed under the circumstances I have just indicated.

It is true that our learned *confrère*, M. Cazenave, says that ecthyma may sometimes be primitive. In his *Treatise on Syphi-*

lis, he even cites a very fine example of primitive ecthyma of the lip, the direct and immediate consequence of a contagion. But what M. Cazenave says of this case, so common in my experience, proves that neither Biett nor himself knew the true nature of this accident. Read the treatise of M. Cazenave, and you will be convinced that he does not consider the ecthyma, in the case referred to, as merely a stage of chancrous disease. According to him, the ecthyma which he calls *primitive* is always a *syphilide*—that is to say, the product of a constitutional infection; in a word, what I call a *secondary symptom*.

But M. Cazenave endeavors to establish the fact that ecthyma is always the result of a previous general infection, although this is the only accident by which syphilis commences. He confounds chancre—the true primitive, *contagious*, and *inoculable* ecthyma—at its ecthymatous starting-point, with the secondary, constitutional ecthyma.

After having stated so truly that this accident might be the first and only result of the contagion, which, "aside from the influence of the virus, requires particular conditions for its development"—conditions *which necessitate the inoculation of primitive accidents;* M. Cazenave—wishing, I say, against his own reason, to bring ecthyma among the syphilides—gives, as examples of primitive pustular syphilides, two observations in which this accident was perfectly secondary, and regularly preceded by a primitive accident of the finger.

This error is very common among persons who are unacquainted with all the varieties of chancre. Was not this error committed by one of our unfortunate *confrères*, to whom M. Cazenave alludes? Was he not supposed to experience a constitutional infection *d'emblée*, and to present an example of primitive pustular eruption?—and yet our unfortunate *confrère* had had a chancre on one of the fingers of his right hand, which was followed by a sub-epitrochlean adenitis. To this adenitis supervened, in regular order, secondary accidents. All this I verified myself; and my verification was confirmed by my learned friend, M. Nélaton. It is true that a person who has not an extensive acquaintance with venereal diseases, although he has written much in relation to them, and was cognizant of the ulceration upon the finger, pretended that it was only an *anatomical tubercle* which had given passage to the virus, without being itself inoculated. I am very much afraid that the brain of this person may have given passage to this fine story without being itself inoculated, in the transit, with a little semblance of truth and common sense.

I have not yet finished my remarks on primitive ecthyma. You, who read everything, sometimes from duty, often from taste, and always with profit to those who in their turn read your productions, must have been surprised to see in a *Manual* of syphilitic diseases, the author of which, as well as the work itself, we hold in great esteem, that the possibility of the production of a pustule by artificial inoculation, but not otherwise, was even admitted. In fact, M. Gibert resolutely denies that chancre, not artificially inoculated, can commence by a pustule. He asserts that this has been considered a stage of chancrous disease, through an error in diagnosis. I think that you already see on which side lies the error. I will say to M. Gibert: "If you admit that a pustule may be produced with the point of a lancet, confess that no great effort of the imagination is needed to find in the processes of ordinary contagion something which may act in the same manner—a nail, a hair, for example—without taking into consideration other circumstances, of which, as a syphilopathist, you must receive the lascivious and disgraceful confessions."

See, my dear friend, how many observers in high station are yet subject to error! Assuredly M. Cazenave and M. Gibert know as well as myself what an ecthyma is; and yet how does it happen that both always obstinately refer it to a state of general infection, and deny it to be a product of chancre? Because theory too often throws a deceptive gauze between the observer and that which he observes; because it is not sufficient, as another observer has just told us, to pass ten years in a venereal hospital, in order to see aright what transpires there, inasmuch as there are eyes which, alas! always gaze, but never see.

I ask your pardon, my dear friend, for my lengthened remarks on the pustular form of chancre. I have detained you on this subject, because, in my opinion, the time has at length arrived when it becomes a duty to turn aside from that *parrotage* which always assigns the same characters to the primitive accident, as though the forms of this accident were immutable and eternal. Nothing is more false and more contrary to daily observation than this doctrine. The primitive accident, in fact, presents numerous varieties, as well at its commencement as during its progress. Permit me to mention here the result of my observation and experience.

In ordinary cases, chancre begins by a superficial or more or less deep ulceration. It does not always destroy the entire mucous membrane, or the skin. Thus, on the semi-mucous membrane of the glans and prepuce, it may be so superficial as to give rise to a belief in an ulcerating balano-posthitis, and to justify certain successful attempts at inoculation.

The ulcer *d'emblee** is produced when the virulent pus has been deposited either upon a surface recently denuded, or upon a bleeding wound; or, what is more rarely the case, upon a wound in suppuration.

Chancre is sometimes observed to commence in the form of an abscess; and this fact has been denied by persons who are in the habit of denying everything. Thus, leech-bites, which have been inoculated, often present, it is true, an ecthymatous form; but it happens that, in this case, the virulent pus inoculates the bottom, and not the edges of the wound. These edges may then reunite, and inclose the virus. At the bottom, a small virulent abscess of the subcutaneous cellular tissue is developed, which, when opened, exhibits a chancrous surface. The fistulæ of virulent pus in the subcutaneous or sub-mucous cellular tissues give rise to the same phenomenon.

These results I have obtained in my ordinary practice at the venereal hospital. I know well that, from this simple theory of abscess, an argument has been drawn in favor of the exist-

* The literal meaning of the expression *d'emblée* is, *at first*, or *from the first*. This explanation is made to prevent confusion in the minds of those readers who may not be acquainted with the French language, and who might consequently be led into error in consequence of having met with the expressions *bubo d'emblée* and *syphilis d'emblée*, the existence of both of which is so ably controverted by M. Ricord.—Trans.

ence of the bubo *d'emblée*, a fact which I do not admit, and which contradicts the truth of my doctrine. But I will hereafter return to these buboes *d'emblée*, and in such a way, I trust, as to satisfy my opponents.

Whatever may be the fact in regard to the different varieties of chancre at its earliest stage, it is clear that these varieties have no influence on the ulterior form which these ulcerations will assume.

This point is one of much importance, and has some bearing on the question concerning the unity or plurality of the syphilitic virus; a question sufficiently obscure in itself, but rendered more so by the vagueness and want of precision indicated in the statement of the facts which are assumed to elucidate it. The following is the result of my experience on the subject:—

When the experiment is made on the patient himself, the beginning of the chancre being always the same, the ulceration which follows the inoculation assumes the same form, and presents the same varieties, as the accident which primarily furnished the inoculable pus. Thus, if the pus has been taken from a phagedænic chancre, the ulceration will assume the phagedænic character; if from an indurated chancre, the ulceration will become indurated, &c. Such has been the result of my experience. But has this result been the same with respect to healthy individuals inoculated with pus taken from diseased persons? We cannot tell, for in inoculations thus made by other experimenters, no notice was taken either of the form of the accident from which the pus was taken, or of the form of the accident which they succeeded in producing. The observations of these experimenters have been accompanied by no detailed description; so that, in fact, they are of but little assistance in the elucidation of the question.

Common observation shows us that one form of disease in one individual can produce a different form in another. But, as we are never perfectly sure of the source whence the infection has been derived, the result may be liable to doubt. It may be supposed that the individual who exhibits the different forms of disease thus described may have contracted the infection from another party than the one whom he accused. The results of the experiments which have just been made on healthy individuals are so well balanced that nothing favorable or adverse to the question can be considered as settled. In the case of M. de Welz, the pus was furnished by a non-indurated chancre; the result was a non-indurated chancre, which may have depended on a want of aptitude in the experimenter. In the experiment made on the interne of the *Hôpital du Midi*, the chancre was indurated; and yet the pus with which the patient was inoculated must have been derived from a primitive and non-indurated ulcer, attributable to an anterior constitutional syphilis under which he was suffering.

You see, my dear friend, that this question concerning the plurality of viruses, so clearly laid down by some English physicians, is yet far from being solved. Up to the present time, we are justified in denying the existence of more than one virus. Inasmuch, therefore, as chancres always commence in the same way, it appears much more rational to admit that they depend on an identical cause, the ulterior effects of which are determined by certain conditions appertaining to the individual in whom they manifest themselves.

In fact, the numerous varieties which the primitive ulcer presents at the period of progress, and which are manifested with more or less rapidity, may be thus recapitulated:—

Simple chancres;

Inflammatory chancres, with an evident tendency to gangrene;

Phagedænic chancres;

Indurated chancres.

These varieties appear to depend upon secondary causes, and are not due to the specific cause. I am not attempting to make a complete work; I am not writing a book on special pathology, and consequently cannot enter into ample details. But, in order to strengthen my proposition, allow me to mention some of the adjuvant causes which impart to chancre a specific physiognomy, and influence the degree of its progress.

For example, observation demonstrates the effects which result from the abuse of alcoholic drinks, especially in warm weather. Under the influence of alcohol, the most simple chancres rapidly become inflammatory; and inflammation in certain regions, as the genital organs, where the cellular tissue easily becomes œdematous, soon gives rise to gangrene. The action of alcohol, in these cases, of which the English people have afforded us such fine examples, is so marked that the resulting ulcer may be called *œno-phagedænic.*

Of the other varieties of phagedænic chancre—such as the pultaceous, the diphtheritic, the serpiginous, &c.—the cause may often be found in certain hygienic conditions: as unhealthy dwellings, bad nourishment, and uncleanliness; in the abuse of rancid mercurial ointment in dressing; in the peculiar diathesis of the patient, as where he is affected with tubercles, scrofula, and scurvy; and frequently in the various circumstances which favor the production of hospital gangrene. To the influence of these must be added, as we shall hereafter see, that of an anterior syphilitic diathesis.

At all events, the conditions which it is of most importance to recognize, inasmuch as these alone almost constitute the pox, are those which are essential to the *induration of chancre.*

But, as *indurated chancre* constitutes one of the most important elements of the doctrine which it is the purpose of these letters to maintain and defend, you will allow me to make it the subject of my next letter.

Yours, RICORD.

LETTER XIX.

MY DEAR FRIEND: If I have been well understood in my last letter, you have seen that I admitted the unity of the syphilitic virus, although the fact has not yet been incontestably demonstrated; that I did not, like some syphilographists, seek the explanation of its varied effects in its greater or less activity, or in its different degrees of acrimony. These effects, on the contrary, I attributed to certain conditions in the individual subjected to its action; so that, in spite of several cases of Bell, and of analogous cases still occasionally met with in practice, in which there is only a simple coincidence, we can draw no inference from the form and gravity of the primitive accident of an individual, as to the form and gravity of the disease of the person by whom it was communicated. Finally, we can no longer say, as we formerly said, to a patient: "If your disease is of a grave form, it is because the person who communicated it to you was seriously affected;" inasmuch as the contrary is very often observed.

This law with respect to the unity of the virus being laid down, I am about to occupy your attention, as I promised in my last letter, with the most important variety of chancre—namely, *indurated chancre.*

The induration of chancre—a condition which certain primitive ulcers assume—was not unknown to writers of former times. Some authors even pretend that traces of the doctrine may be found in Galen—a circumstance which does not astonish me the least in the world, inasmuch as I believe in the antiquity of the pox. It is certain that, after the great epidemic of the fifteenth century, some of the first syphilographists of the times described this remarkable symptom. This fact did not escape the attention especially of Jean de Vigo, who has other titles to our esteem than that based on the invention of his famous plaster.

Nevertheless, you know that to Hunter is awarded the honor of having first described indurated chancre. This symptom has even received the name of the great physiologist. The *Hunterian* chancre, in fact, is nothing else than indurated chancre. And yet Hunter scarcely touches on this subject. You remember what he says in relation to it: "Chancre has usually a thickened base; and although the common inflammation extends much beyond it, still the specific inflammation is limited to this base." But, as you see, Hunter does not make this thickened base a constant condition; and he was right, for the greatest number of primitive ulcers do not present this peculiarity. Nor does he make it the condition of the constitutional infection—an important and inexplicable omission in a man of Hunter's sagacity, instinct, and divination.

The syphilographists who came after him—even Bell, with his illustrations of a *split pea*—did not appreciate the whole value of the induration.

Since the time of Bell, most syphilographists have paid no attention to this symptom. M. Lagneau, in his treatise, appears to attach no importance to it. But I must do M. Lagneau the justice to state that, with Bell and others, he recognized the fact that chancre might have a pustular period. But, aside from this circumstance, you will be struck, like myself, with the confusion which pervades his description of the chancres which he calls *primitive*, and those which he calls *secondary*. In no respect can he be said to have correct views relative to the induration of chancre.

M. Cazenave, "whose work is all alike, and who cannot be considered in earnest"—expressions of courtesy which he has but recently used in regard to myself, and which I return him that I may keep nothing which belongs to him—has a method of appreciating primitive accidents which is truly incredible. Does he acknowledge the existence of any other primitive accident than the *infecting act?* According to him, in fact, other accidents must be either *primitive secondary* or *secondary primitive*. Escape from this dilemma, if you can, notwithstanding all the wit with which you daily regale us. At all events, the induration of chancre—the capital phenomenon in the disease—does not appear to exist on the *other side of the river*, as Lisfranc observed.

And yet, who can now misconceive the importance of this phenomenon? In view of all that I have done to elucidate this subject; in view of the judicious observations of the learned Professor Thiry, of Brussels—of those of my pupil and friend, M. Diday, of Lyons—of those of M. Marchal (de Calvi)—of those of my learned friend and too kind partisan, M. Venot, of Bordeaux—of those of MM. Acton and Méric, of London—of those of my learned colleagues, MM. Puche and Cullerier; and, finally, in view of the observations of my hospital patients themselves, whose education is such as to leave few chances for inattentive physicians to commit error, I am justified in concluding that they who do not recognize the value of this phenomenon have eyes which do not see.

Therefore, as this induration, which may *line* as well as *surround* chancres, merits the utmost attention of the practitioner, allow me to study it carefully.

All chancres do not become indurated; at present, only a small number of them become so; and, if my doctrines are true, this number will constantly diminish.

But what is the specific condition, ulterior to the insertion of the virus, which causes the chancre to indurate?

This is one of the most interesting problems which the study of syphilis can present; and it is also one which it is exceedingly difficult to solve. Nevertheless, I believe I have found one of the unknown quantities.

When we interrogate the age of the patient relative to the cause of the induration, we receive no reply.

The sex, the temperament, the hygienic habits of the patient, are interrogated with no better result.

Anterior or concomitant diseases, under which the patient has suffered, do not enlighten us any more than the specific medication to which he has been subjected.

Thus far, then, we have been forced to content ourselves with the common explanation, which, you know, refers everything to aptitudes and idiosyncrasies.

In fact, it is found that the first chancre developed in certain individuals does not become indurated, while a second one does; and that those which may be contracted subsequently do not indurate.

What, now, is the cause of this mysterious phenomenon?

One reason for this difference, which has thus far escaped notice, we shall seek in the general and constant laws of virulent diseases; in the striking analogies which exist between variola, vaccinia, and the pox.

We are now in the true path.

Vaccination, for example, may fail for the first time; this failure will be due to some want of aptitude of which we are ignorant; but, if it succeeds, the unsuccessfulness of subsequent vaccinations is explained. The effect of the diathesis produced by the first vaccination is not yet worn out; and a certain period, which modern observation is tending to determine with accuracy, must elapse before the organism again acquires aptitude for a vaccinal impregnation.

Very well! We have thus arrived at a capital fact in syphilogeny; a fact which long experience has demonstrated—a fact which has been also observed by two persons, whom it is always a pleasure to cite, MM. Puche and Diday. The fact is this:—

As a *general rule*, A PATIENT WHO HAS ONCE HAD AN INDURATED CHANCRE WILL NEVER HAVE ANOTHER.

With respect to vaccinia and variola, it is probable that this law must present exceptions; I will add that it is even desirable that these exceptions should exist, inasmuch as they show that the syphilitic diathesis may be destroyed.

But, one thing is very certain; these exceptions are far more rare with respect to syphilis, for MM. Puche, Diday, and myself are still in search of indisputable proofs of their existence.

This circumstance is due, my dear friend, to the fact that, when there is indurated chancre, there is of necessity *constitutional pox.*

With the induration, the syphilitic *disposition*, as Hunter called it, is acquired; the syphilitic *temperament*, as I have elsewhere stated, becomes established; and, finally, a specific *diathesis*, which gives rise to ulterior manifestations, is developed.

Neither the disposition, the temperament, nor the diathesis can double or triple itself any more than the analogous disposition in vaccinia can thus double or triple itself.

The indurated chancre is to the pox what the *true* variolic pustule is to the variola; what the *true* vaccinal pustule is to the vaccinia.

The *non-indurated* chancre is the pseudo-pustule; it is a *false* vaccinia.

Here you have, my dear friend, an admirable law; a law which brings the pox under the general laws of virulent affections; a law which guides us in the study of syphilis, as the variolic and vaccinal inoculations guide us in the study of variola; a law which satisfies the mind, and gives it a sure resting-place after a painful and tedious voyage amid deceptive hypotheses and contradictory theories; a law which arithmetic—the first rule of which was so much outraged by one of your former correspondents—will serve to establish, if, to obtain the real sum, similar values be added together.

But I am not charged, at present, with the special education of your honored correspondent, the provincial student; with the duty of teaching him to distinguish the difference which exists between a diathesis and the manifestations of this diathesis; between the diathesis, properly so called, and the resulting cachexia; to all of which matters I shall undoubtedly have occasion to return, and with respect to which I am very much afraid the mind of this poor student is in much trouble.

For the present, let him be aware—he will excuse this magisterial style of speaking—that the diathesis acquired by the infected patient prevents the induration of another chancre which he may contract; and that this immunity from a new general infection must also be hereditarily transmitted. By means of this fact we are able to understand the remark which was made but a little while ago: This transmitted disposition may well have an influence in the diminution of indurated chancres, and therefore on the diminution of constitutional poxes. Variola and vaccinia also present in this respect a curious field for study. This idea, which originated in my school, has been carefully studied in a remarkable thesis maintained by a distinguished pupil of Val-de-Grâce, whose name I cannot just now call to mind.

Therefore, the non-induration of chancres contracted at different periods, subsequent to the development of an indurated chancre, is a proof, which can easily be verified by statistics, of the *unicité*—a neologism for the introduction of which I am not to blame—of the syphilitic diathesis; a fact implicitly admitted by Hunter, when he said that the formation of the syphilitic disposition could be prevented, but that this diathesis could not be destroyed when once established; a fact which M. Cazenave did not suspect he had proclaimed in accordance with our views when he wrote, in his *Treatise on Syphilis*, "We are not aware that the syphilitic temperament has ever been destroyed." M. Cazenave would certainly not admit, as a sound principle in physiology, the assertion that there exists a double sanguine, and a double bilious temperament, any more than he would admit the assumption of the existence of a double glanders, a double variola, and a triple hydrophobia, to be a sound principle in pathology. The *non bis in idem* is thus, so to speak, a pathological law; I trust I shall be able thoroughly to elucidate this question, in all its bearings, while studying the evolution of constitutional accidents.

These points of doctrine relative to the etiology of the induration being thus established, let us now study this phenomenon with respect to the period of its appearance, and with respect to its seat, its peculiar symptoms, its nature, and its progress, that we may finally arrive at a true exposition of its consequences.

This important question will be the subject of my next letter.

Yours, RICORD.

LETTER XX.

MY DEAR FRIEND: I propose to entertain you with a still further description of indurated chancre. The subject is somewhat uninteresting, though of much importance; and I need all of your kind attention while I attempt to elucidate it.

This important variety of the primitive ulcer is regularly rounded, in proportion as it is seated upon homogeneous tissues. Its edges are scarcely ever *décollés*. They are not always perpendicular (*taillés à pic*). A little prominent, they are continuous with the bottom, which is hollowed, as it were, *in the form of a cup*. The surface of the ulceration, which is grayish and lardaceous, is sometimes *irised*. Its centre is of a somewhat deep brownish color, which has given rise to the designation of *partridge's eye*, sometimes applied to it.

But at what period does the induration, which constitutes the principal character of this variety of chancre commence? What time elapses between the act which effects the contagion, and its first manifestation?

The solution of this question is highly important, inasmuch as, from the moment the induration takes place, the disease ceases to be merely local.

I have sought to determine this period; but this is not always an easy matter. Patients seldom present themselves until a long time after contagion has been contracted; and not aware of the importance of the pathological state in question, they have failed to notice its commencement.

In the majority of cases, this want of attention on the part of patients is explained by the fact that indurated chancre is essentially indolent; that it is of slow progress; that it suppurates little; that it is not perceived until some time has elapsed; and that frequently the induration escapes notice. You are aware that I have already cited examples of this fact. I mention it again in order that you may recall it to the minds of those who are firm believers in the miracle of constitutional poxes *d'emblées*.

We are not always sure of the date of the coition or contact to which the chancre itself is to be referred; consequently, it is very difficult to ascertain when the induration actually commenced.

However, in cases where it is possible to arrive at a true knowledge of the state of affairs, it is not before the third day that the induration manifests itself. In all cases, it becomes manifest in the course of the first or second week. It would even seem certain—unless more precise observations should prove the contrary—that, if a chancre exists for more than three weeks without becoming indurated, it will not indurate at all. Induration is a precocious phenomenon. Certain conditions may so deceive us as to induce belief in subsequent indurations. Let us examine these conditions.

The specific induration is not always easily recognized. After ordinary contagion as well as after artificial inoculation, the infected part frequently becomes the seat of an inflammation—that which Hunter called *common inflammation*—which, for a given period, incloses and masks the specific induration; so that it is only in proportion to the degree in which the simple œdematous, sub-phlegmonous, or frankly inflammatory engorgement is absorbed, that the specific induration is well described, and is found exhumed, as it were, from the inflammatory atmosphere by which it was surrounded. Thus far, the engorgement, whether œdematous or inflammatory, has been so prominent an indication that the specific induration is only thought to commence from the moment it begins to be appreciated; and some persons have been thus led to believe in tardy indurations, in chancres which have not begun to indurate till three weeks, a month, and even a longer period after contagion.

Certain local applications and cauterizations sometimes give rise to factitious indurations, which may be produced at different periods, and thus give rise to misconceptions. These factitious indurations may even be complicated with specific indurations, and render the latter unrecognizable. It is known that unbelievers in a specific virus formerly stated that corrosive sublimate will produce an ulcer similar to the Hunterian chancre. Similar! ay, they were right; but not an *identical* ulcer. In fact, with corrosive sublimate; with chromate of potassa; with liquid acetate of lead, so often employed in vulgar practice; with hot ashes from a pipe; and sometimes simply with the nitrate of silver, accidents may be produced which so closely resemble indurated chancre, as to constantly deceive physicians who have not an extensive acquaintance with the disease. Such errors alone have induced the belief that indurated chancre is not invariably followed by constitutional accidents.

There is another source of error from the influence of which several syphilographists—among others, M. Babington, the commentator of Hunter—have not escaped. Patients may preserve an induration which has resulted from a primary contagion, and subsequently contract a new chancre on the same spot. Without a clear knowledge of the history of the patient, it might be supposed that the previous induration was the starting-point of the latter chancre, and was the first symptom of the contagion. This is a great error; in all cases, the induration consecutively follows the ulceration.

Such cases as these—in which no account has been taken of an induration resulting from an anterior contagion—have induced the belief that the new chancre contracted by the patient, occupying the same spot as the prior induration, becomes, in turn, itself indurated; an error which might occasion the admission of more exceptions to the law of *unicité* than really exist.

You know that several syphilographists assume that all primitive accidents may be followed by secondary accidents; and that, if this remark applies to any accident in particular, it applies to blennorrhagia. Very well! these syphilographists admit, with stronger reason, that non-indurated, as well as indurated chancres, may be followed by constitutional accidents. It is, therefore, very important to ascertain how far this supposition is true. You have already seen that common inflammation can so mask the specific induration as to induce the belief that another form of chancre exists. It also happens, though more rarely, that the ulceration, after having been indurated, becomes phagedænic. Therefore, if one has not seen the commencement of the disease, he may still be deceived, and believe in the possibility of constitutional accidents succeeding non-indurated phagedænic chancre.

On the other hand, the induration, without losing its immense value as a symptom, is not always well formed; it does not, in all cases, attain the same development. Sometimes it is superficial. In order to discover it in the thickness of the skin or mucous membrane, it is necessary to clearly understand its nature. Sometimes it imparts to the touch only the sensation derived from feeling a fold of parchment; I call this form, at the *Hôpital du Midi*, the *parchment induration*. Indurated chancres, then, are frequently taken for simple excoriations, and for simple balano-posthites, when they do not wholly escape attention; for they are on a level with the healthy parts, and sometimes even slightly prominent.

The induration ordinarily invades the whole base of the ulceration; but in some rare cases, it only affects the edges, in which case it is annular. For this form of indurated chancre we might apply the designation of *primitive annular syphilis*.

When no complication exists, the induration is suddenly circumscribed by the healthy tissues; it is much more extensive than the ulceration of which it forms the base. It is composed of a hard, somewhat cartilaginous, resisting, elastic, indolent, perfectly rounded nodule, which raises the ulceration above the level of the surrounding healthy parts, and thus constitutes a variety of *ulcus elevatum*.

The induration is sometimes presented under the form of a more or less prominent *crest*, when the plastic infiltration which constitutes it is not formed of homogeneous tissues, and when it meets resistance at some points, as at the reflection of the prepuce to the groove at the base of the glans, at which situation, indeed, the best characterized indurations are found.

If the skin or mucous membrane which covers an induration be compressed, these tissues become pale, presenting to our view something analogous to what we perceive when, turning over the eyelid, we compress the conjunctiva upon the tarsal cartilage.

The induration is usually produced in a slow and gradual manner. Sometimes it increases by *saccades;* in some cases, it remains for a long time but slightly perceptible, then subsequently assumes considerable development. The tissues often become extensively indurated. I have seen the entire length of the base of the glans undergo a cartilaginous transformation which might have given rise to the belief in its cancerous degeneration. One of the most curious cases of this kind was sent me by Professor Andral.

The induration, after having diminished or even disappeared, is very liable to return. It is not rare, then, to see it acquire dimensions more considerable than it at first assumed.

The duration of the induration is unlimited. In those cases in which it is superficial, in which it resembles *parchment*, I have seen resolution take place so completely as to leave no traces of its existence within a less period than a month. At other times, on the contrary, it persists for months, and even years. Where it is developed on the groove at the base of the glans, at which point, as I have said, the most marked cases occur, its duration is greater than at any other spot. It may quickly disappear from the glans, from the neck of the uterus, and from the vulvar ring, where it is but slightly marked, and difficult to detect. At the urethra, especially in women, and at the vagina and the anus, its existence is quite ephemeral. Much attention is required to prevent mistakes. On the tongue, and particularly on the lips, it remains a tolerably long time. When the induration begins to disappear, the ulceration has, in all cases, been healed for some time.

When resolution takes place, the induration undergoes modifications. It loses its resistance and elasticity. It becomes, as it were, gelatiniform; and a wrinkled spot, of a coppery brown tint, finally occupies its place.

The indurated chancre, which is less often multiple than the other varieties—the specific ulcerating period of which is soon limited *sua sponte*, or by the aid of art—nevertheless assumes, at times, considerable dimensions. It extends itself, and excavates the neighboring tissues; so much so, indeed, that we might expect it to occasion great loss of substance; but when cicatrization is complete, frequently no traces of its existence will remain, for it is the plastic exudation alone which serves as aliment for the phagedænic influence, thus securing the surrounding tissues from the effects of the ulceration. A knowledge of this very common condition of indurated chancre is important, in view of the etiology of constitutional syphilis; for it is not the most numerous nor the deepest cicatrices which prove that the poisoning has taken place.

The specific induration of chancre is absolute proof that the constitutional infection has occurred. It is the intermediate state between the primitive and the secondary accident. In fact, the indurated is that variety of chancre which soonest loses the distinctive character of the primitive accident—to wit, the power of furnishing inoculable pus. But, if the induration of chancre demonstrates the existence of infection, and if the degree of its manifestation always bears a definite relation to the gravity of the accidents which are about to succeed; if this induration can be considered—permit me to use the expression—as a *syphilometer*, it may also serve as an excellent guide in treatment; for this form of accident is the one which is commonly most amenable to mercurial treatment. Nevertheless, there are cases in which the induration resists treatment. But in this case the induration is generally no longer specific, but proves to be an organized tissue, that is to say, an internodular tissue, which has succeeded it. In this way I was able to account for an induration presented by a patient who entered the hospital to be treated for caries of the frontal bone, which came on thirty years after the development of a chancre, at the base of the glans. This induration persisted under the form of a very marked nodule. In a great number of cases, it is exceedingly difficult to distinguish between the internodular tissue and the specific induration. The thickness of the skin and mucous membranes, the subcutaneous and submucous cellular tissues, constitute the anatomical seat of the specific induration; but it would seem that it usually selects for its seat the lymphatic capillaries. It is in those regions, in fact, where the lymphatic networks are most prominently exhibited, and where they are most abundant, that the induration is most completely formed, and acquires the greatest dimensions. This opinion receives still farther support from the manner in which the induration is propagated; that is to say, it is seen to follow the course of the lymphatic vessels, which are delineated in the form of cords, in proportion as they become more voluminous.

As to the intimate nature and constitution of the induration, organic chemistry, which has furnished so many marvellous results, more perhaps than will bear careful investigation, has taught us nothing; while the microscope, which generally promises more than it reveals, has thus far shown the specific induration to be only fibro-plastic tissue, which proportionably quite abundant, does not differ from that met with in non-specific conditions. Such, up to the present time, is the result of the researches undertaken by one of my highly distinguished disciples, M. Acton, of England, and of those subsequently prosecuted in Paris by MM. Robin and Marchal (de Calvi). The same results have been obtained by our learned and industrious micrographist, Dr. Lebert, to whom we owe the production of so many fine works.

Such, my dear friend, are the results of my researches and observations on indurated chancre. I present them to you here simply as indications; for, as I have often been obliged to repeat, I am not writing at present a didactic treatise on syphilis. I am only calling your attention to the principal points of my doctrine, on account of the objections still made to it, and which are addressed more or less directly to myself. Ampler developments form the subject of my oral instruction, and especially of an extensive work which I am preparing, and of which these *Letters*, to tell the truth, are merely the *summary*. I am here presenting the general principles, the essential points of doctrine embodied in the latter work, indicating the principal grounds on which they rest; and the present *Letters* have no other merit than that which the character of your readers may impart to them—readers who are no longer students, but learned and enlightened practitioners, to whom the indications I have pointed out merely serve the purpose of calling to mind the careful studies and researches of previous years.

Yours, RICORD.

LETTER XXI.

MY DEAR FRIEND: *How do chancres cicatrize?* Allow me to say a few words on this important subject.

The period of reparation is indicated by the disappearance of the areola of the ulcer. Its borders become disgorged, sinking and resting on the bottom; and the undermining ceases, if it existed. The margin becomes of a pale, grayish-pearly tint, and finally assumes the normal color of the surrounding tissues. The bottom cleans off; the gray, lardaceous, diphtheritic layer is at first pierced, as it were, by granulations which, finally occupying the place of the layer, give to the ulceration a granular aspect, and a healthy rosy tint. The pus becomes less abundant, and of a healthy, creamy character; and it may, in this case, justly be said to be *laudable*, for it ceases to be inoculable. In proportion as the parts fill up, the epidermis spreads from the circumference to the centre, and the cicatrization is completed in the same way as in any wound which has suppurated.

The *cicatrix of chancres* may remain more prominent than the surrounding parts, sometimes being on a level with them, but most frequently depressed, according to the thickness of the tissues affected. In a great number of cases, it is indelible, while, in others, it completely disappears, as frequently occurs after indurated chancre, or when the chancre is seated upon a mucous membrane.

But, as those whose experience has been extensive well know, the period of reparation is subject to various irregularities. In *serpiginous chancre*, one extremity often cicatrizes, while the other becomes more diseased; sometimes one side heals, while the other continues to ulcerate. Finally, the cure often takes place at one or many points of the centre, while the circumference is constantly augmenting its vicious circle.

You know, in fine, that, in certain individuals, when a proper course of treatment has not been pursued; when the physician has been ignorant of the means of repressing granulations by cauterizations; or when foolish prejudices have prevented the employment of this remedy, the granulations are said to become luxuriant and vegetating, and to give to the ulceration certain aspects which have obtained for it the name of the *budding*, *fungous*, or *vegetating* chancre. True vegetations, of varied forms, may then be produced; but, as these vegetations may be considered an accidental, epigenic tissue, they are not of a syphilitic nature, as we shall hereafter see.

At this period, as I have already said, that is to say, when the chancre has infected the economy, it may itself undergo a transformation *in situ*, and finally present the characters of mucous papules, and thus give some countenance to the opinion of those who, from failure to analyze the subject, are unacquainted with these metamorphoses—of those who have admitted, besides, that these accidents could be sometimes primitive, and sometimes secondary, and that they were, in all cases, contagious; an opinion which I have already controverted.

But here a point of doctrine arises on which I insist, and to which I must again call your attention. It is this: That form of chancre which may undergo relapses at different times never *returns when it has once cicatrized*. If a new inoculable chancre develops itself after cicatrization has become complete, we can affirm the chancre to be the result of a new infection.

From what I have stated, it is very certain that, when we take into account the morality of patients, as far as we can arrive at a knowledge of the fact from a due acquaintance with the conditions by which they are surrounded; when we take into consideration the seats which chancres seem to select for their development, as well as their usually limited number; when we likewise know how to appreciate the variations which different chancres present with respect to their period of progress and of specific *statu quo*, and with respect to their course and duration, and the different aspects which they may assume at the period of reparation and even of subsequent cicatrization; when, finally, we consider the more or less marked influence of the mercurial treatment in certain cases, we can usually arrive at a rational diagnosis which is almost absolute.

The physiognomy of the primitive ulcer is ordinarily so expressive (permit me to use the word) at the specific period that we are able, by seeing it, to name it. It is even necessary to distrust this first impression, inasmuch as it may occasion indiscretions which can scarcely be repaired. You have allowed me to illustrate my remarks by pathological anecdotes, and I shall avail myself of your kindness; happy if I can relieve the aridity of my previous descriptions.

One of our distinguished *savans* entered my cabinet one day, and without any preamble showed me—a diseased member, saying: "What is that?" I immediately replied: "It is a chancre." "Very well, sir; my wife gave it to me." "Then, sir, it is not a chancre." "And why not, if you please?" "Because," I replied, "that which distinguishes simple ulcerations resembling chancres from true chancres, is the source whence they have been derived." My patient was not the dupe of an argument which would have been sufficient for certain physicians whom you know; and he contented himself with replying, with much dignity and resignation, "Cure me."

But is the diagnosis always so easy as some of our classical authorities have assumed it to be? I appeal to M. Lagneau, who is so worthy a representative of these authorities at the present time. Observe now whether, despite all the care he exhibits, he succeeds in distinguishing the primitive chancre from what, with so many others, he still considers the secondary chancre. Glance again at his synoptical and comparative table of the ulcers which are liable to be confounded with those produced by the syphilitic virus, and tell me whether he is successful, and especially whether he enables others to be successful in esta blishing this difference with certainty.

Mercury, that infallible touchstone in the eyes of believers —a touchstone which, in England, has been the basis of the division of syphilis into the *true* and the *false*—is a deceptive reagent. It often cures non-syphilitic accidents, while it aggravates those which are syphilitic, and which sometimes get well without treatment.

How many chancres exist which are unrecognized by skilful practitioners! How many errors are committed with respect to the different varieties of indurated chancre, the most dangerous form of all! Sometimes simple excoriations are mistaken for the disease; at others, the disease is supposed to be a true cancerous degeneration. My friend, M. Vitry, of Versailles, must recollect the case of a patient to whom I was called by a physician in Paris, not to judge of the nature of his disease, but to amputate his penis. I recognized the existence of an indurated chancre, with considerable development of the plastic exudation; and pills of the proto-iodide superseded the knife.

One of our learned professors belonging to the Faculty of Paris, who excels in diagnosticating syphilis as well as other diseases, cannot fail to recollect the case of a Russian nobleman whom we saw together at the house of our honored and regretted master, M. Marjolin, and in whom he was unwilling to recognize the existence of a primitive accident, because nothing was observed but the specific induration, and because the nobleman could not explain how he had contracted the accident; yet, a short time afterwards, as I had predicted, we obtained the most striking proofs of a constitutional affection.

If you will allow me, I will relate a short anecdote. Cullerier, the nephew, one day sent to me a popular writer in order to obtain my opinion relative to an ulceration situated upon the corona of the glans; an ulceration with an indurated base, and not then presenting those characters at its edges and base which are authoritatively assumed to constitute chancre. Nevertheless, I recognized an ulceration with the specific characters of induration already described, and with the ganglionary prolongation which we are shortly to study. Cullerier was not of my opinion, because he had examined the two women accused of imparting the contagion, and had found them healthy. Admitting neither mediate contagion nor spontaneous syphilis, and placing confidence in the word of the patient, he could not admit the existence of a primitive ulcer. I, who admit all rational sources of contagion, and often doubt until I obtain the most certain proof, remained convinced either that the patient had been deceived, that he deceived himself, or that he deceived us. In fact, scarcely six weeks had elapsed when very marked constitutional accidents—so marked, indeed, as to be exceedingly difficult to cure—were manifested. But, while Cullerier was yet pondering the question how and why this patient had contracted the pox, I was called to the house of a great lady.

I arrived, knowing nothing of the purport of my visit. This lady was mysteriously seated in her boudoir; and, in spite of the softened light of the room, I could perceive on her face the evident tokens of a secondary affection. "Doctor," said she, "I have to speak to you on a very delicate matter." Wishing to cut short a painful avowal: "I see what it is, madam," said I to her, "for your face tells me plainly enough why I have the honor to be here." "What do you say?" replied she, with astonishment. "That you are diseased, madam, and for that purpose desire my attentions." "Not the least in the world; and I have sent for you in order that you may assist in curing M. X—— (the writer sent to me by Cullerier) not only of his disease, but also of his dangerous *liaisons*." And she then drew a portrait, which was but little flattering, of the two women whom Cullerier had examined and found healthy, and who, according to this lady, were the cause of the whole trouble. I had much difficulty, as you may imagine, in making her understand that the source of our poor author's trouble was much closer to me, and in obtaining the avowal that the pressing interest she manifested relative to him was not altogether based on a pure Platonic affection.

It is ever thus, my dear friend; and here is the moral of this anecdote—that people of the world never make you complete avowals. By reason of their relations with great ladies, or others in whom they have confidence, their mind is a thousand leagues from the truth; their thoughts are not fixed upon the true source of their disease, and they seek it where it is not.

You see, then, how difficult, frequently, is the diagnosis of chancre, and how wrong we should be in denying its existence, when patients will not aid us in tracing it to its source.

I am acquainted with all the difficulties of diagnosis in many cases, and I have seen persons of the greatest skill commit frequent errors in relation to it; and for this reason I have said, and I still assert, in spite of contrary opinions, that the only positive, univocal, pathognomonic sign of chancre, at the period of progress or of specific *statu quo*, is the inoculable character of the *pus which it secretes*. From this fact I have drawn the following conclusion:—

Inoculation furnishes the most certain sign of the specific nature of the ulcer.

I stated, in the work which I published in 1838, that, if mercury must be given in all cases where a primitive virulent accident exists, it is essential to be assured of the fact of virulence by means of artificial inoculation. But compose yourself, my dear friend; this operation, so repugnant to some persons, and even dangerous if not properly managed, is unnecessary in practice; and I have never advised its performance as a general rule. The prognosis and treatment of the affection depend on other indications. The induration of chancre, with its accompaniments, in relation to which inoculation furnishes us no assistance, form the conditions whence we deduce the state of the constitution, and point out to us the specific treatment which the disease requires.

This fact, I trust, I shall be able to demonstrate.

Yours, RICORD.

LETTER XXII.

My dear Friend: It would afford me much pleasure to say a word relative to the treatment of chancre; but you know that, according to the plan I have proposed to follow, I cannot, in this connection, enter into many details.

Perhaps you will permit me to say something here in relation to prophylaxis. Medical police has advanced much of late years, especially since I and others have made examinations with the speculum, in private hospitals and in the public dispensaries.

It is very certain that, since this mode of investigation has been generally employed, a great amelioration in the health of public women has been observed. Thus, in 1800, according to Parent-Duchâtelet, one woman in nine was found diseased; since 1834, the proportion has been reduced to one in sixty. The speculum has had a great share in this amelioration.

But, to be thoroughly successful, I have always insisted that women should be visited, every three days, without distinction of rank; whether they be *en maison*, or *en carte;* whether they dwell in Paris or at the *barrières*. You remember that inoculable pus may be formed after the second day of an artificial inoculation. Swediaur admitted that chancre may be developed within twelve hours. Frequent examinations with the speculum are therefore indispensable, if we expect the surveillance of public women to furnish a certain guarantee of freedom from disease.

I write this word *guarantee* with special design; for there are some people who, after contracting an accident in their adventurous loves, think they have the right of claiming indemnity from the government. Perhaps you think I am not serious. I shall offer a fact in proof of my assertion. Some years since, I received a visit from a merchant of Lyons, who was in a state of great exasperation against the prefect of police. He came to get a certificate, setting forth the fact that he had contracted a chancre in a house of prostitution, the cha racter of which he imagined to be *guaranteed* by the authorities. His intention was to prosecute for damages. He did not know that *toleration*, like all commissions, receives no guarantee from the government.

I pass on to state that the ameliorations daily introduced in the surveillance of prostitution, and the zeal exhibited by the brethren on whom devolves the painful service of the dispensary of public health, and of the hospital of Saint Lazarre,* will furnish yet more auspicious results.

Public women are a necessary evil; this fact is generally admitted. I do not wish to examine the reasons favorable or adverse to the proposition, for this is not the place to examine it. But if the evil is necessary, it does not follow that its quantity, so to speak, should be extended, as a learned Belgian brother lately seemed to wish; but it is especially necessary to inspect it well in relation to its quality.

In insisting that public women shall not communicate disease, it should be so arranged that they shall not be liable to contract it from those who have commerce with them. How is this result to be accomplished? Is it necessary to institute an examination of the persons who visit them; and to prevent these visits, should they prove diseased? But, apart from the difficulties of such an institution, the danger which we should thus seek to prevent would be augmented, for, instead of falling into a sewer, which the police could cleanse, the unclean would go elsewhere.

The establishment of lazarets and of quarantines, suggested by my friend Diday, of Lyons, in a moment of laudable philanthropy, where a clear patent of immunity from the pox, along with a certificate of vaccination—a patent that should be as indispensable as a passport, a patent without which no one should be admitted to any public office—could be furnished, cannot be thought of at this day. Whatever may have been said by the author of this ingenious proposition, the difficulties in the way of such an institution seem insurmountable.

* The prison of St. Lazarre is divided into three sections, one of which is set apart for prostitutes who have committed some offence against sanitary laws, and who are detained until cured.—Trans.

There was a time, you know, when those affected with pox were banished from Paris, and condemned to the rope if they returned; a time when, at Bicêtre, patients were scourged at their entrance and exit. All this did not diminish the number of the infected. On the contrary, the scourgers perhaps deserved, in their turn, to be punished. These barbarous measures have fallen into disuse.

It is undoubtedly necessary to subject to rigorous surveillance all persons whom we can reach—soldiers, for example—and to sequester every patient over whom we have control; but a certain degree of toleration, the pardon of a fault which is sufficiently often involuntary, and excellent hospitals, with such attendance as may at present be found there, and which time will still further improve, are the best means of general prophylaxis, or those at least which will tend to diminish the gravity of the disease.

Moreover, all who are acquainted with the conditions to which women are subjected in the present state of society, with respect to labor and its remuneration, have for a long time acknowledged the fact that herein lies one of the most fruitful sources of prostitution, and consequently of the propagation of syphilis. Therefore, to ameliorate the condition of women with respect to labor is to do a kind office as well in relation to humanity, as to morals and public hygiene.

You remember what I said with respect to the manner in which chancres are produced. It is necessary to remember this fact, in order to shun the sources of contagion pointed out. The most important fact which science teaches us relative to prophylaxis is the necessity of avoiding exposure. This remark, doubtless, appears a little *naïve;* but let debauchees remember it, for the fact is what I have stated. I am now about to touch upon a delicate subject, and one which is full of shoals. It is still an unsettled question in morals and medical deontology, whether the physician ought to give advice, with respect to preservation from evil, to those who voluntarily expose themselves to the liability of contracting disease from immoral persons. I do not pretend to be more severe than the austere Parent-Duchâtelet, who treated this subject with a purity of intention of which you are well aware. Besides, am I not encouraged by the very character of the journal which gives such liberal hospitality to my letters? I address the learned—those who are physicians; and was it not yourself, my dear friend, who said that science is chaste, even stark naked? Be not alarmed; for, after all, I shall only glide over this ticklish subject.

There is no sure and absolute preservative against chancre. This is my declaration.

If, in spite of a knowledge of this fact, one is still willing to run his chance, some precautions may be taken. It is especially necessary to bear in mind the precept of Nicholas Massa, so energetically rendered by the elder Cullerier: "The connection should not voluntarily be prolonged." At this moment, indeed, it is necessary to be egotistical, as was remarked by Hunter, but not egotistical after the manner of Madame de Staël, who said that love is the egotism of two.

The most minute attention to cleanliness on the part of suspected persons ought to be required in houses of prostitution. A fact with which we have been for a long time familiar, namely, that a deposit of virulent pus may be held in reserve in the genital organs of women, demonstrates the necessity of this precaution. This is a means of always preventing mediate contagion. I have told you that numerous experiments have proved to me that, by decomposing the virulent pus, we can neutralize it. Alcohol in water; water mixed with one-fifth part of Labarraque's liquid; all the acids diluted with water, so as not to be caustic; wine; solutions of sulphate of zinc and acetate of lead, destroy the inoculable power of virulent pus: while, if this pus remains unaltered, excessively minute quantities of it—homœopathic quantities, if you please—retain their power to act. M. Puche informs us that, at the *Hôpital du Midi*, the effects of inoculation have been obtained by him from one drop of pus mixed with half a glass of water!

Fatty substances are very useful, especially to medical men who are obliged to practice the touch upon dangerous localities. Astringent lotions, which slightly tan the tissues, have frequently prevented contagion.

But, if cleanliness is necessary before intercourse, on the part of the one who may impart the disease, it need be minute only subsequent to the act in the one who is simply exposed to infection.

There is another means which the moral sense repudiates, and in which the debauchee has great confidence; this undoubtedly often serves as a security against infection, but, as was observed by a woman of much *esprit*, it is a cuirass against pleasure and a cobweb against danger. This mediate *process* is an article which is often porous, or may already have been used; it frequently becomes displaced; it fulfils the office of a bad umbrella, which the tempest may rend, and which, protecting badly enough from the storm, does not prevent the feet from getting wet. I have seen, in fact, numerous ulcerations of the root of the penis, of the peno-scrotal angle, of the scrotum, &c., in those who had taken these useless precautions.

Many patients believe themselves safe from contagion when they do not terminate the venereal act. A lady who consulted me on her own account was very much astonished when she found that she had communicated a disease to her lover, because, said she, *he did not complete the venereal act.*

Some syphilographists believe that the urethral infection is especially effected after ejaculation, because a vacuum is thus created, and because nature abhors a vacuum. But numerous facts have taught me the reverse of this statement. Ejaculation, on the contrary, must be considered as a powerful injection from behind forward, which thus cleanses the urethra; and, if urethral affections, already so common, are not more frequent, we must perhaps attribute the fact to this condition. Thus, an old and excellent precept recommends a prompt emission of urine after every suspicious intercourse.

The circumcision of the prepuce, the excision of nymphæ that are too long, also constitute hygienic measures relative to the genital organs, inasmuch as these appendices greatly favor contagion.

I ask your pardon for this digression; but it is the duty of science to destroy the influence of charlatanism with respect to the dangerous employment of a deceptive prophylaxis.

It would be necessary, were it possible, to indicate all the measures that prevent contagion and therefore the propagation of syphilis, not in order to protect or to encourage libertinage, but to protect the virtue and the chastity which so often become its victims.

I am yet to speak of cauterization as an abortive remedy against chancre. But I will make this the subject of my next letter.

Yours, RICORD.

LETTER XXIII.

MY DEAR FRIEND: I promised to call your attention, to-day, to the cauterization of chancre.

This remedial measure, which I have so ardently sought to incorporate among the therapeutics of the venereal disease, has not yet been generally adopted. It has even been expressly condemned by some practitioners, and I am sorry to add that a very unfavorable opinion of it was given by the Academy of Medicine before I had the honor of being a member of that honorable body.

You will recollect that one of the members of this learned society treated cauterization with so little favor as to disdainfully return the remedy to the *corporal's guard*, with whom, he said, it ought ever to have remained. The author of this apostrophe, in his character of military surgeon, should at least have informed us as to the effect of the measure in the corporal's guard; for it is important to be satisfied with respect to its efficaciousness. If the means be good, the source whence it originated is a matter of indifference; and we make this remark without reflecting in any degree whatever on the corporal's guard.

The cauterization of chancre did not originate with me; but I am a firm supporter of its value as a remedial measure; and, in this capacity, you know, my opponents have not failed to attack me. It is, therefore, my purpose to defend the principles which I advocate.

Let us first invoke analogy, in illustration of the question.

We cauterize the bites of the viper and of the mad dog, as well as anatomical wounds, anthrax, malignant pustule, and often with success, when our services are timely invoked. No one would be inattentive to a puncture made with an instrument soiled with the pus of farcy or glanders. The surgeon who would fail to cauterize in these cases would be highly culpable. And yet the very men, whose hand in all such cases is armed with iron and with fire, pause when the disease happens to be chancre! Why? Because they either cease to reason, or cease to reason with effect.

Let us prove our statement.

Does chancre, whatever may be its variety, always produce accidents at a distance? Does it always infect the economy?

With respect to this question, you know, there are three parties with distinct opinions:—

One party, which appears to believe in nothing that is not incredible—a party which is still numerous—is convinced that chancre is not a primitive accident in the strict acceptation of the term; but that it simply constitutes the first manifestation of the general infection, or, as I have already stated, a primitive secondary, or a secondary primitive accident!

Another party, which already begins to have a glimpse of the truth—and the school of Hunter must be ranged in this category—admit chancre to be at first a local accident; but thinks that it must inevitably infect the economy unless specific medication is employed in time.

Finally, the most rational party—that which has observation, experience, and the evidence of facts on its side—affirms that chancre is always, *at the commencement*, a local affection, which art can arrest, and which, even without the intervention of art, may remain local in certain well-determined circumstances, whatever may be the extent of the chancre with respect to its sur face or depth. The last observers maintain—and this is one of the consoling points of the doctrines which I profess—that, even when the chancre is about to infect the economy, this result does not take place instantaneously, but only after the lapse of an interval sufficiently complete to enable us to destroy it.

I say nothing with respect to the physiologists whom I have elsewhere opposed, and who do not admit a general infection either before, or during, or after exposure to the cause. This doctrine is now duly interred. And what is very singular, some of its advocates have since become more favorable to the true doctrine than myself. I could cite instances of some who, despite their previous incredulousness relative to the virulence of the disease, are now so thoroughly convinced of the fact as to believe in the dangerous effects of the most minute quantity of pus.

It is not my purpose to enter here upon the discussion of the manner in which buboes are produced, and of the time at which the constitutional infection takes place. We shall return to this subject hereafter. I only wish to consider the reasons favorable or unfavorable to the value of cauterization as a means of curing chancre, and especially the reasons which have induced me to employ this remedial measure.

What is the object of cauterization?

1. To prevent constitutional infection,
2. To hinder the production of buboes;
3. To retard the progress of the primitive accident, which occasions greater or less deformity, and sometimes the loss of important organs;
4. Finally, to destroy a focus of contagion.

Those who believe that the constitutional infection always precedes chancre, not only state that it is useless to cauterize the accident, since the disease which we seek to prevent already exists, but they further add that it would be dangerous so to do, inasmuch as the chancre is *an emunctory* by which the economy frees itself from the virus. If this opinion were well founded, it would follow that it is not only imprudent to destroy the chancre, but that, on the contrary, it is necessary to preserve and extend it, in order to furnish the virus with numerous and easy doors of exit. This is a logical sequence. But you know, my dear friend, that these logicians do not act in this way; and we must admit that it is very fortunate for their patients that their practice is inconsistent with their professions.

The difference is not great between this school and that which, as I have already told you, believes that chancre, at first local, inevitably produces general infection. The disciples of this school tell us that the activity of the infection is proportional to the number, the extent, and the duration of the primitive accidents. But, alas! after the statement of these fine doctrines, there comes a contrary element, which leads to the direst practical nonsense. What do the physicians of this school prescribe? Listen to them; they tell you: "Be careful about destroying the chancre. Do not seek its rapid cure; for, by so doing, you will throw back the virus into the economy. Inclose the wolf in the sheepfold, and finally render the infection more active."

Do you not admire the manner in which all these deductions are linked together?

The virus is thrown back through the drying up of the virulent source! The wolf, shut up in the sheepfold, is so much the more dangerous, from the fact that it is dead! The infection becomes more active, when the elements are destroyed which must augment its virulence!

My intelligence cannot ascend to the sublime heights of this reasoning. Are you more fortunate than myself, my dear friend?

But this is not all. The partisans of this doctrine further say to us: "Be careful with respect to the chancre: it indicates the actual condition of the patient, and shows us what will be the subsequent stage of the infection."

They add, still further: "Do not cure the primitive ulcer too quickly; it serves to direct, and obliges the patient to follow your general treatment."

What do you still think of these doctrines? What satisfaction, in fact, is to be derived from knowing beyond a doubt, each day, that your patient really has a chancre, and from being assured that it is this chancre which determined the other accidents which you are subsequently required to combat?

The primitive accident, they say, serves to direct us in relation to the depuratory treatment; but you know, as well as myself, that not one of those who inculcate this doctrine suspends the general treatment when the chancre is cured, even by his own method. Their treatment is almost identical in all cases. It consists in administering a fixed dose of mercury, within a given time, whatever may be the nature of the primitive accident, or whatever may have been its duration. And then, what do you say as to the precaution of letting a chancre progress to such an extent as to require the amputation of the penis, in order to oblige the patient to follow a certain treatment—a precaution that is truly admirable and prudential!

Cauterization has been said to be a frequent cause of bubo; and, in proof of this assertion, the statistics of Bell have been cited—statistics which a single visit to the venereal hospital, at Paris, would suffice to reduce to naught.

The law, with respect to the matter, which you may verify whenever you please, is this: When chancres are cauterized, fewer buboes are developed than when this means is not resorted to. Cauterization does not always prevent their production; but, while it never determines specific buboes, it often prevents them.

It may prevent, but it never favors constitutional infection.

I am familiar with the fact that many observations have been cited in support of the heresy which I am combating; but they prove about as much as the case somewhere to be found in the works of Van Swieten, in which the patient had been affected with a chancre *for more than a month, and after cauterization, became affected with secondary ulceration of the throat, as a consequence of the assumed repercussion!* Oh! pox, when wilt thou be understood?

M. Lagneau, who is opposed to cauterization, because, among the other inconveniences connected with it, *it destroys the primitive accident in too short a time*, cites against it an instance in which it had a wonderful result. But, in order to form a better judgment of the case, let us allow M. Lagneau to speak for himself:—

"In 1807, an officer of high grade, who was called for a short time to the imperial head-quarters at Varsovia, exposed himself to venereal contagion. Shortly afterwards, two chancres were developed at the base of the glans. Before appropriate treatment was commenced, the army was unfortunately commanded to march. The patient was unwilling to leave his regiment at a period when everything indicated the occurrence of great events, in which he was anxious to participate. Being attached to a cavalry corps of the advanced guard, his duties were exceedingly arduous, on account of the extreme severity of the cold. I was therefore unable to resort to the usual treatment in such cases. For many reasons, such as the irregular administration of the remedies at my disposal, I could not hope to be able to prevent the development of accidents, when so many powerful causes capable of producing them were in operation. *I yielded then to the reiterated requests of this officer, and touched his ulcers with the nitrate of silver*, carefully warning him, however, of what he would be likely to experience in the future. *The chancres cicatrized very promptly, and the patient finished the campaign without experiencing the least inconvenience.* Shortly after the battle of Eylau, the army having taken cantonments upon the Pasargo, he informed me, according to agreement, of his condition; and I engaged to prevent, by a methodical treatment, the results of a general infection. He followed my advice, and has not since experienced the slightest venereal symptoms."

After so conclusive an illustration in favor of cauterization, you will not, I trust, expect me to adduce the thousands of similar facts which I have been enabled to collect in twenty years' practice. The above seems sufficient for every purpose.

In order now to clearly explain to you my views on cauterization, you will allow me, in my next letter, to present to your notice some important propositions.

Yours, RICORD.

LETTER XXIV.

MY DEAR FRIEND: At the close of my last letter, I asked your permission to mention some important propositions, before stating to you in what way I understand, and how I practice, the cauterization of chancre.

Here, then, are the propositions to which I referred:—

Chancre, as I have sought to prove to you, is at first an absolutely local affection, and may remain definitively local.

Chancre may be cured spontaneously, or by local treatment.

It is only after the lapse of a certain time that chancres assume an aggravated form, and are able to produce accidents in their immediate neighborhood, or at a distance.

If chancres are soon destroyed; if an abortive treatment is applied to them at the first stage of their existence—for instance, from the first to the fourth or fifth day of their appearance—the patient is almost certainly placed out of danger, as far as they are concerned. But, if we are called to the case at so late a period as no longer to feel certain with respect to the efficacy of the abortive treatment, cauterization may still abridge the duration of the primitive ulcer.

These principles established—and I am yet to hear of a really serious objection to them, based on experiment or clinical observation—the whole value of cauterization, as an abortive method, is at once understood. So important is the remedy, in view of its beneficial results, that, with M. Ratier, I should wish the following precept to be posted wherever there is exposure to syphilitic infection; namely, that no erosion, no suspicious discharge should be allowed to exist an instant, when, by this means, it can at once be destroyed.

But, in order to obtain the good effects of cauterization, as an abortive treatment, and to prevent all ulterior accidents, several conditions are requisite:—

First, the *age* of the chancre must not be reckoned from the moment when the patient first perceived its existence, but from the contagious contact which must have produced it. By taking this precaution, it will be seen, as I have stated, that the chancre, destroyed before the fifth day of its existence, is truly dead, and produces no consecutive accidents.

In order to render the cauterization effective, it is not sufficient to touch an ulceration with any caustic whatever; for, unless, on the fall of the eschar, a simple wound shall be found in place of the virulent ulcer, the caustic will not have accomplished its purpose. It is after unsuccessful cauterizations, or such as have been performed at too late a period, that accidents have supervened; and these accidents ought not to be attributed to the application of the caustic. In fact, if buboes already exist; if the chancre is indurated; if the syphilitic diathesis is therefore established; and if secondary accidents, in consequence, have supervened, cauterization can only serve to modify the primitive accident, to hasten the reparative period, to repress the granulations, to modify the cicatrix, and finally to abridge the duration of the ulcer.

As an abortive method, as a means of neutralizing the results of the venereal disease, cauterization can be studied to great advantage in artificial inoculation.

And here it is important to state that, from the moment a puncture has been made with a lancet charged with virulent pus, or from the moment when, by any other means, the morbid poison has been made to penetrate the tissues, simple lotions no longer suffice to prevent contagion. Not only so, but we cannot arrest its effects by applying to the contaminated part the various agents which, as I recently told you, are capable of neutralizing the virus, when mixed with it prior to inoculation. These mixtures may easily destroy the *syphilitic seed*, when the latter is separated from the soil in which it is sown; but as soon as the seed is sown, they are powerless to prevent its germination. Timely cauterization or excision is alone adapted to effect this result.

On this subject, I have made numerous experiments. At the moment of making *artificial punctures*, I have placed on them either plasters of *Vigo cum mercurio*, as has been advised for the abortive treatment of variola, or pledgets of lint covered with strong mercurial ointment. But I have not been able, by this means, to prevent inoculation.

I have never succeeded in preventing the development of chancre except by destroying the part contaminated.

It is necessary to remember the fact that, when the pustule is perfectly formed, or when the ulcer exists, the range of virulence is not wholly limited to the secreted pus; that it is not even limited to the diphtheritic layer which covers the chancre; for, if the ulcer is opened, and the pus it furnishes removed, and if its pyogenic pseudo-membrane is destroyed, virulent matter is still reproduced with all its specific qualities. Hence we deduce the conclusion that there exists *a sphere of virulent activity*, the radius of which is proportional to the extent and duration of the ulceration. Consequently, and this fact it is important to bear in mind, the caustic must penetrate to a point beyond the area of specific inflammation, if we expect it to prove efficacious.

I have stated that every chancre, whatever may be its extent, is bounded by tissues which are not in a state of *virulence*, and that on these tissues a simple wound may be made which will easily cicatrize. This limit, beyond which the caustic must penetrate, it is difficult to determine. I can at least state that I have always been successful when I have cauterized the parts to an extent double that of the ulceration, and including the whole thickness of the tissues. It may easily be conceived that the extent of certain ulcerations, and their situation, may be such as not always to allow us to operate in this way. Consequently, cauterization frequently fails; almost always, in fact, when nitrate of silver is used. This caustic, the action of which is very superficial, is only adapted to the lightest and most recent accidents.

The Vienna paste is the best caustic which I have used. I have never failed with it when I wished to destroy a pustule only five or six days old. In this case, a single application almost always suffices to produce a dry eschar, which is detached little by little by a cicatrix that forms beneath it. If the eschar falls too quickly, or if it is forced off by the suppuration, nothing is left but a simple sore.

The arsenical paste has also yielded me very satisfactory results; but I have always employed it in a positive manner, that is to say, allopathically; for you know that this therapeutic agent, when used homœopathically, has been said to be successful in the hands of a learned *confrère*.

The actual cautery is likewise an excellent remedial agent, the best perhaps that can be employed, were it not so frightful in the eyes of the patient, and were it not that we are unable to use chloroform whenever we wish to make use of this means of cauterization.

I am experimenting, at this time, with the mono-hydrated nitric acid, in view of the excellent results obtained from it in Belgium and England, as an abortive agent, not only in phagedænic, but also in simple chancres. In a great number of cases, in which I have perfectly succeeded with it, it would appear that the ulcerations can be neutralized without the necessity of destroying so much of the tissues as with the other caustics. Yet it must be acknowledged that its action is very painful; that the pain it occasions endures longer than that produced by the Vienna paste; and that, if the primitive ulcer be somewhat extensive, we are ordinarily obliged to make several applications of it at an interval of two or three days.

Whatever be the caustic employed, its application must be repeated as often as, on the fall of the eschar, there is perceived the lardaceous base peculiar to the period of progress. Afterwards, recourse ought to be had to a less powerful caustic, with the sole object of directing the cicatrization.

Hunter, who, as you know, was strongly favorable to the cauterization of chancre, also advised its excision. Whenever we can excise too prominent nymphæ which serve as the seat of primitive ulcerations; whenever we can remove a too lengthy prepuce, whose edge is affected; and whenever we can cut sufficiently far from the diseased parts, this operation ought to be preferred, because at the same time that we remove a disease we cause a deformity to disappear. But this operation is inadmissible whenever, as is usually the case, the seat of the chancre is such as not to allow us to cut to a sufficient distance into the neighboring parts.

Excision, like cauterization, is useless in indurated chancre. The most early excisions of the *specific induration* have never prevented the manifestation of the symptoms of constitutional infection.

In all cases, whatever be the means employed for the more or less rapid destruction of chancre—whether excision or cauterization—we should never forget to fulfil all the other indications which may be presented.

But allow me to finish this letter, my dear friend, or, if you prefer, this *postscript* to my last epistle, by repeating the statement that the cauterization of chancre is an admirable remedial measure, and that it is yet, in a social point of view, the most powerful prophylaxis of the syphilitic disease that we possess, since, by its certain and prompt destruction of the contagious accidents, it obliterates the foci of infection. All the conclusions I have presented on this subject are deduced from many thousand facts, and from rigid and patient experimentation.

Permit me further to add, relative to the prophylaxis of chancre, that it would be a great error to suppose that, in proportion as accidents are developed, or contagion after contagion successively acquired, the chancres which supervene are less *active* than those which have preceded them, and that they gradually lose their intensity, as far as their number is concerned, and finally become incapable of further reproduction.

The reverse of this is frequently observed. The chancres which are contracted latest may be more *active* than the primary ones, and may even take on the *phagedænic form*, which most generally occurs when there exists a *syphilitic diathesis*, or *syphilization* (to use the expression of those who dislike the accepted term). So true, indeed, is this statement, that I consider the *syphilitic diathesis as a cause of the phagedænic form of the disease*. The proof of this fact I promise to furnish you, whenever you desire it, at the venereal hospital. Hereafter, however, I shall return to all these doctrinal points.

In the mean while, the laws deduced from experiments on animals would prove that the inoculation of the syphilitic virus gives essentially different results, according as it is practised on man or animals. These laws, if indeed they are laws, have, in no respect, thus far, invalidated what I have said on this subject. Therefore, let us await further developments.

You recollect, perhaps, that Frike, of Hamburg, who has also experimented on inoculation, thought he observed that successive inoculations gradually lost their intensity, and that their effect became *null* at the *sixth* experiment, when practised on the same individual. I have made eight consecutive experiments in the inoculation of chancre, and have never noticed the least difference between the effect of each experiment.

Frike, to whom I exhibited these results, was forced to confess that he had deceived himself.

In my next letter, I shall commence the exposition of my doctrine upon bubo.

Yours, RICORD.

LETTER XXV.

MY DEAR FRIEND: First of all, let me give my excuses and regrets for my prolonged silence. I do not dare to call to mind the date of my last letter. It is better to acknowledge one's errors than to give a bad excuse for them. I confess, then, that a long time has elapsed since I promised to speak of buboes. Give me credit at least for being logical; for, you know, I do not admit the bubo *d'emblée*.

Buboes, with due submission to Astruc, are as old as man, unless the first man was without lymphatic ganglia. They were well known to the Jews, who, according to Apion, were subject to them during their sojourn in Judea. Good King David appears to me to have been much afflicted with them. They form a very interesting and important subject of study.

You are aware, my dear friend, that for the ample treatment of this subject, the epistolary is not so well adapted as the didactic style; but I must restrict myself to the limits which I imposed upon myself, and which were accepted by you.

That which I am about to set forth, I have inculcated for so long a time that it may now be considered, in great measure, the property of the profession; and yet there are still those who have not forgotten what they learned at the school in 1828, and what is contained in the last edition of M. Lagneau.

Be this as it may, can bubo, regarded as a venereal accident, be developed unless another accident has preceded it? Can it be considered as the first consequence of an impure contact? Can it be assumed to supervene *d'emblée?* On what is this opinion, which dates back to the time of the mysteries, based? What is there to prove its truth? Analyze, my dear friend, all the observations adduced in its support, and everywhere you will perceive a want of appreciation of causes, false analogies, errors in diagnosis, ignorance of the laws of evolution and their possible consequences.

Any contact, or connection, provided it be *suspicious*, no matter at what period it is developed subsequent to the appearance of the bubo, is considered a sufficient cause of its existence. The same elasticity is exhibited in the method of drawing conclusions relative to what is designated the period of incubation. With respect to the previous relations of the patient, it is always to the person who inspires the least confidence that the disease is traced, in order to explain a ganglionary engorgement whose cause cannot be found, and this is done without knowing, in most cases, what the infection of the accused person really is. According to this method of reasoning, every ganglionary engorgement which exists may be considered of a venereal nature. But if the simple contact of virulent pus with non-denuded surfaces is sufficient to occasion buboes, without previously producing other accidents, it follows that the bubo *d'emblée*, the rarest of all forms of bubo, even according to the partisans of this doctrine, should be the most frequent of all; inasmuch as the case in which excoriation follows contact with contagious parts are far more numerous than the instances in which contact produces excoriation.

In the multitude of patients whom we see in such large institutions as the venereal hospital of Paris, and in whom there frequently exist numerous chancres, furnishing a large amount of specific pus which soils the neighboring parts, are buboes ever seen to supervene, excepting by means of the lymphatics connected with these ulcerations? True, in cases of this kind, it is essential to guard against the illusions which misled M. Schals, of Strasbourg, and those who have so *naïvely* cited him.

To those who, like myself, have rejected the bubo *d'emblée*, the following query has been addressed: "Why will you not admit that the venereal virus may traverse the skin and the mucous membranes, so as to reach the ganglia, without necessarily inflaming or ulcerating them, since many other matters are absorbed without requiring the existence of a previous lesion?" It would be useless to manifest unwillingness to admit this supposition; for, if it were true, it would be necessary to acknowledge the fact; but it is not true.

Because mercury can be made to penetrate the economy by simple friction, without solution of continuity, does it follow that caustic potassa can also be made thus to penetrate the skin? Does cadaveric matter where there is no excoriation, or the foam of a mad-dog unaccompanied by a bite, or the venom of the viper where there is no puncture, ever thus act? Would our excellent colleague and learned vaccinator, M. Bousquet, place much reliance upon applications of the vaccine virus without the production of the vaccine pustules? Have vaccinal adenites ever been seen unaccompanied by the pustules? From the time variola was first inoculated up to the present time, have variolous adenites been developed without variola? Undoubtedly not. Do not then invoke the aid of false analogies. If certain causes act in a given manner, we must not thence infer that all act in the same way. That which distinguishes these affections, and which shows syphilis to have a specific nature, is the fact that the latter does not penetrate the tissues without lesion, and the surface which it at first affects, preserves, for a longer or shorter time, its usual appearance before the disease extends itself.

All the authors who admit the existence of the bubo *d'emblée* tell you that they have met patients affected with engorgement of the *inguinal* glands, who had neither blennorrhagia nor chancre. Bell perhaps saw twenty of these cases. He ought to have seen hundreds, if their existence was real. M. Lagneau, who, in imitation of his predecessors, gives several observations of this kind, adds that they are always to be met with at the venereal hospital. Just so; and it is because a tolerably large number of these pretended buboes *d'emblée* are always to be seen at the venereal hospital, that I have been able to understand how the error has been so often committed.

I will here make a curious observation. It is this: The history of buboes *d'emblée*, aside from the case of Dr. Schals, in which an axillary engorgement, the result of a felon, was mistaken for a bubo supposed to be the effect of the absorption of blennorrhagic vapors through a recent cicatrix on the finger, presents no instance in which the partisans of the doctrine have cited cases in which the disease was situated in any other than the inguino-crural region. It has never been asserted that these buboes have been seen under the jaws—a portion of the body which has received so many kisses of a doubtful character.

Before we admit that a ganglionary engorgement is venereal; before we are justified in considering it as the result of a more or less recent contact—the result of the passage of *the virulent pus in substance* through cutaneous or mucous surfaces which remain healthy; before we can admit that this engorgement is the first manifestation of the syphilitic disease, and that it is in fact a bubo *d'emblée* and not a secondary bubo—for the advocates of this doctrine admit the existence of secondary buboes—we require the differential diagnosis between these two varieties. Now, you know how they are distinguished: If the patient has had some anterior affection, the bubo is considered constitutional. When no antecedents have been discovered, the physician does not push his inquiry beyond the last contact; and the bubo is then ranked among the primitive accidents; for, in relation to the seat, the form, the symptoms, the progress, and the termination of the disease, no absolute distinction is made between the two forms of the affection.

But are the lymphatic ganglia subject only to venereal influences in general, and to the syphilitic virus in particular? Undoubtedly not. These ganglia are affected in various ways, which it is unnecessary to indicate. But I ought to mention that, in cases in which syphilis does not exist, they are affected by causes which are not always discoverable; and this occurs in many other diseases whose causes elude our notice. In these cases, the adenites are said to be essential, idiopathic. But may not these same adenites, with their hidden cause and nature, be presented in individuals who have experienced suspicious contacts? Unquestionably. Very well! Has any one succeeded, by the signs with which you are acquainted, in establishing a difference? Certainly not. Not one incontestable pathognomonic sign has been furnished. Most frequently it is the particular seat of the disease which is considered to settle the question. What M. Charles Dupin did for the departments of France relative to instruction; what Parent-Duchâtelet did for the quarters of Paris with respect to prostitution, has been done with respect to the inguino-crural regions; that is to say, a gloom has been cast over them. The ganglionary engorgement, which is there regarded a venereal bubo, would be considered innocent if situated in the axilla, and especially on the side of the neck, as though all lymphatic ganglia were not equal in the human species; as though the same causes could not affect them, wherever their situation may be, and to a degree corresponding to their position.

Not only is there no differential diagnosis of these simple ganglionary engorgements made by the ordinary symptomatology; but no marked difference has even been established between strumous and venereal adenites. What do you think, in fact, of such elements of diagnosis as the knowledge of the patient's temperament, and the particular aspect of scrofulous buboes, which are commonly *soft*, *œdematous*, and of a *violet-red* color! If to this you add the special *elasticity* of scrofula—a doctrine inculcated by my learned friend, M. Boyer, who, moreover, has the good sense to reject buboes *d'emblée*—you will comprehend that, with such means of establishing the differential diagnosis, it is not surprising that a primitive bubo has been proved to exist. But the really primitive thing, which should attract our notice, is the doctrine itself.

We shall hereafter see what venereal buboes are in general, and syphilitic buboes in particular. For the present, let us finish this letter by stating that, neither by experiment nor by incontestable observations, has the existence, or even the possibility, of buboes *d'emblée* been demonstrated; that the reign of the imagination in pathology has passed away; and that, consequently, these buboes have disappeared from nosology. It will be sufficient to cite, on this subject, the condemnation pronounced against them, in a moment of *abandon*, by one of their most glorious supporters, Hunter, who says, speaking of bubo *d'emblée:* "If the parts were much more carefully explored, if the patients were minutely interrogated, it is probable that a small chancre would often be found to be the cause of the infection; this fact I have more than once observed. Indeed, when it is considered how rare is absorption in gonorrhœa, where the mode of absorption is the same, we can scarcely admit the infection to be the result of the simple contact of venereal pus, when this pus has been applied for so short a time. It might be supposed, it is true, that the repetition of the contact is equivalent to the period of its application; but such an opinion is inadmissible, for this repetition itself would be likely to develop a local affection."

After this statement of Hunter, I have nothing to say.

Yours, RICORD.

LETTER XXVI.

MY DEAR FRIEND: This letter will perhaps seem to you a duplicate of the discussions of the *Société de Chirurgie*, the report of which is given in the *Union Médicale;* but you know it is not my fault if I am forced to repeat the same thing several times. The circumstance is rather attributable to the fact that some are unwilling to understand, for I will not say that they have an interest in not understanding, the real merits of the question. My adversaries I believe to be actuated but by one motive, that arising from the love of science and of truth; but I also have the right of claiming that no other motive shall be assumed to actuate myself. I am about then to continue my remarks on buboes.

After having controverted by reasoning, by observation, and by experiment, the existence of the essential venereal bubo of some syphilographists, or the bubo called *d'emblée*, I ought now to explain to you what I understand venereal adenopathies to be. This is certainly one of the clearest points of pathology to those who are blessed with a transparent pupil, a sensitive retina, and a brain unaffected by prejudice. In this examination, it is necessary first to observe the state of the patient, and afterwards the stage of the disease. With respect to the *inculpated* subject, it is necessary to know what ganglia are affected, and to what degree they are affected, and what their condition was prior to the contagion, so as to be able to distinguish them from those which have only become diseased subsequent to the development of the supposed venereal accident. This being laid down, and in accordance with the law that venereal diseases are not the only causes of ganglionary affections, with which they may often be complicated, let us see what really takes place in subjects who have no other pathological indications.

In the largest acceptation of the term, venereal accidents, virulent or otherwise, and blennorrhagia and chancre, may give rise to sympathetic adenopathies; this word is here used in its proper sense to represent those diseases which are the result of sympathetic influence. These sympathetic buboes, by nature essentially inflammatory, ordinarily occupy but a single superficial ganglion; and are easily controlled by antiphlogistics and resolutives, and in those rare cases in which they suppurate never furnish *inoculable pus*. It is these alone which accompany blennorrhagia, when the latter is not symptomatic of a urethral chancre. It may be said that *a blennorrhagia, which, during its whole course, has never furnished inoculable pus, will never give rise to a virulent bubo.* This is another of those laws against which anarchists contend in vain, and which the power of the lancet, which they begin to recognize, will cause them to acknowledge on occasions of necessity.

But these sympathetic buboes, these inflammatory adenites, which may be produced by so many other causes, such as ineffective or inopportune cauterizations, or other irritants, do not therefore constitute a special accident. So far as these buboes are concerned, venereal diseases act only as common causes, and only pertain to them in an indirect manner, or as a simple complication.

The special buboes, which it is our purpose to study, are distinct from the other diseases of the lymphatic ganglia, and can only be the consequence of virulent venereal affections; that is to say, of syphilis. They are either the mediate, or, if you please, the consecutive product of the contagion; or the result of the constitutional infection. Here we have two perfectly distinct species, which it is very important to recognize.

The first species of syphilitic bubo contains two varieties, which are almost always confounded by most syphilographists. You can convince yourself of this deplorable confusion, by reference especially to certain recent treatises.

The first variety of the mediate or consecutive bubo is that which succeeds to the *non*-indurated chancre and its different phagedænic varieties. This bubo of *absorption* does not *invariably* take place. Every non-indurated chancre does not inevitably give rise to it. It may even be said that more non-indurated chancres exist without it than with it. These buboes are essentially the terminations of the *direct* lymphatics, the orifices or extremities of which open into the chancre, either at the same side, or at the opposite side, when the vessels cross the median line. This relation is an essential one, and when it does not exist, buboes are not to be found. I can thus explain their frequency after chancres of the frænum, for example, and understand why I have never seen them supervene after the numerous inoculations which I have made at the upper part of the thigh.

The bubo which is observed in connection with the non-indurated chancre is not only never developed previous to this chancre, *which ought not to be the case, if it could occur independently of the latter*, but it ordinarily does not show itself until after the lapse of some time—often the first week, in the course of the second, and in certain circumstances at a much later period; and, if the primitive ulcer obstinately remains at the specific period, it does not manifest itself until after the lapse of months, or even years. In a patient of my colleague, M. Puche, a serpiginous chancre existed for three years before it occasioned a virulent bubo. It is only when the ulceration happens, sooner or later, to meet the desired relations, or when it *has not counteracted them in its progress*, that its virulent pus passes into the lymphatic vessels, which convey it directly to the ganglia, without themselves becoming infected.

With the non-indurated chancre, which is patent, or concealed in the urethra, in the anus, in the vagina, or in the mouth, the adenites, provided the chancre is unique, is most frequently mono-ganglionary. *It affects only the superficial ganglia*, so that the division of buboes into superficial and profound can in nowise be applicable to the virulent sort. The adenitis of virulent absorption, symptomatic of non-indurated chancre, is inflammatory, and usually very acute; it must inevitably tend to suppuration. Whether the virulent pus furnished by the chancre at the specific period be arrested in a lymphatic vessel, or reaches a ganglion, it produces a kind of inoculation which, owing to individual dispositions, gives rise to accidents analogous to those from which it emanates; that is to say, to chancres of the lymphatics or of the ganglia, with a progressive and suppurative tendency. But in this *intra-lymphatic* and absorbent inoculation, if I may so express myself, there supervenes, as in cases of inoculation on the skin and mucous membranes, a *common* inflammation of the neighborhood or of the periphery. And while the infected lymphatics and ganglia suppurate specifically, their phlegmonous atmosphere only furnishes simple pus. The existence of these two layers, at first so distinct and independent, and so easy to understand, was for some time unknown. You remember that one of your recent correspondents, who is so apt to confound everything, was astonished that they should be distinguished. Well! these two concentric layers have the various properties with which you are already acquainted, and which explain the reason why some experimenters, such as MM. Cullerier, the uncle and nephew, were enabled to assert that the pus of buboes is never inoculable. In fact, if, on the day we open a bubo in which the pus has not remained too long a time, we inoculate with the first pus which escapes—that is to say, with the pus of the phlegmonous layer—the result is negative; while, if we happen to take the pus from the deeper layers—that is to say, the virulent pus furnished by the ganglia—the result is positive.

I have observed cases in which the infected ganglia, a kind of virulent cysts, were dissected and laid bare by the peripheric phlegmonous suppuration; I then inoculated the pus in their *neighborhood* without result; and on opening afterwards the ganglion, I obtained a pus which produced a specific action. When we have for a time long delayed to open a virulent bubo, so that the ganglionary pus is effused into the phlegmonous pus, and has had time to mix with it, as well as when the bubo has already been opened a given time, all the pus is inoculable.

13

Hunter had already shown that the virulent pus of the bubo of absorption is identical with that of chancre, and, like it, is inoculable; the bubo in this case being a *ganglionary chancre*, contagious after the manner of other chancres. It was even the pus of a virulent bubo which he compared to that of a reputed secondary accident, cited among the cases recorded by the *Société de Chirurgie*, and from which the end was so conveniently *disarticulated.*

But it is a remarkable fact that the virulent, *primitive* pus is never met with, beyond the first ganglia, in direct relation with the chancres from which the contagion originated. Inoculable pus is never found in the deep ganglia, in the lymphatics which emanate from them, or in their termination. There is a barrier which the *primitive* pus has never passed. It is experiment, artificial inoculation, my dear friend, which taught me this fact, with due submission to those who, after having so much calumniated it, are so ready, at the present time, to acknowledge its value. Now, if it should happen that we are in doubt concerning the matter; if the effects of the pus from the base of the ulcer upon the lips of the spontaneous or artificial opening of a bubo do not enable us to establish, in the great majority of cases, a certain diagnosis, the incontestable pathognomonic signs will be negative inoculation in cases of inflammatory and scrofulous buboes, and POSITIVE IN THE SINGLE CASE OF VIRULENT BUBO.

Yours, RICORD.

LETTER XXVII.

MY DEAR FRIEND: The second variety of the *mediate*, consecutive bubo is that which succeeds the indurated chancre. This form of symptomatic adenopathy merits the greatest attention and should be studied with care. It differs as much from the preceding variety, as the indurated chancre itself differs from the other varieties of the primitive ulcer.

The engorgement of the ganglia, in this variety, is, perhaps, generally more precocious than that which succeeds a non-indurated chancre. It generally manifests itself before the lapse of the first week; and it may be said that its appearance is almost never delayed beyond the second week. If it has not been perceived within this period, it is because the observer has not known how to seek it. In cases of indurated chancre, the adenopathy is inevitable from the commencement. It is never tardily developed—which, as I have stated, is occasionally the case with the other forms of the primitive accident.

I have never observed a case of *specifically* indurated chancre which has not been attended by the symptomatic engorgement of the neighboring ganglia. This engorgement is so regular and characteristic, that it may serve to indicate the nature of the chancre which has preceded it, when the latter has already disappeared, or when it is concealed in some profound regions, or when its base is very indistinctly marked.

By those who are well acquainted with this form of adenopathy, the seat of the primitive accident—an obligatory door of entrance, so to speak, to constitutional pox—can always be found with ease, provided we see the case in time, for chancre alone is its cause. Any one may easily convince himself of the truth of this statement by observing patients in whom secondary accidents are developed, and in whom this variety of ganglionary engorgement is perceived only in the neighborhood of the primitive accident. An acquaintance with the fact enables us to recognize certain transformations *in situ*, which are complicated, as it were, with certain secondary accidents, and thus to find their true starting-point; as, for example, in certain cases of papules, or mucous patches, which are considered primitive, and which have succeeded chancres *in situ*. I can now affirm that it is from want of rigorous appreciation and precise analysis, and because the physician has not seen the disease from the commencement, or because he has allowed himself to be deceived by simple coincidences, that mucous *tubercle* (a secondary accident) has been thought capable of giving rise to an engorgement of the neighboring ganglia. Whenever this accident, like all other secondary accidents, develops itself in many regions at the same time, we may easily convince ourselves that it is only where chancre has existed, that the ganglionary engorgement, which I have just described, is found.

As may be observed with respect to the acute, virulent adenopathy, symptomatic of the non-indurated chancre, a lymphangitis may precede and accompany the ganglionary engorgement in question. Here, the lymphatic cord is hard, indolent, sometimes knotted over the valves; it can easily be raised and circumscribed, when it is seated upon the dorsal face of the penis. At the corona glandis, under the *præputial conjunction*, the cords are found flexuous and serpentine; and, if the semimucous membrane be slightly stretched over them, it is discolored, and the cords remain whitish, a circumstance which does not occur in inflammatory lymphangites. This state of the lymphatic vessels, as a result of indurated chancre, might be confounded with other lesions of these vessels, if the indurated chancre whence the diseased vessels emanate, and the affection of the ganglia in which they terminate, did not enable us to recognize it. Moreover, in this species of lymphatic angiopathy, the neighboring skin, without changing color, is frequently œdematous; but it is a variety of œdema in some respects gelatiniform, on which the finger leaves no impression.

The ganglia, as in the other varieties, are much more tumefied on the side corresponding to the chancre, when but one accident exists; this side alone may remain affected, or the opposite also may be seized. Whether the one or both sides be affected, the infection is very rarely limited to a single ganglion. In the very great majority of cases, the adenopathy is multiple. As a very general, if not an absolute rule, what may be called ganglionary *pleiades* may be seen to form in the lymphatic radiation of indurated chancres.

At first, it is a simple *indolent* tension which almost always escapes the notice of the patient, and even of the physician, as may be proved by the observation of M. Boudeville, which was called in question at the *Société de Chirurgie*. Unless in a marked lymphatic temperament, or where there is strumous complication, it is rarely that the swelling exceeds the size of a small nut. Apart from accessory causes of inflammation, wholly foreign to the nature of indurated chancre, the ganglia remain indolent, hard, and resisting, and give to the touch a sensation almost identical with that resulting from contact with the specific induration of chancre. They do not unite among themselves to form one mass, as is the case with strumous adenopathies, for the peripheric cellular tissue does not ordinarily become engorged; they are then habitually movable upon their base, as well as under the skin, which does not adhere to them, and which changes neither its color nor temperature. In fat persons, especially in women, they are in a manner buried in the fatty cellular tissue, and it is necessary to seek them carefully in order to recognize them. These buboes almost always terminate by a slow but complete resolution; and this resolution not unfrequently takes place a long time after the disappearance of the chancre from which the buboes originated. Sometimes the ganglia, as well as the lymphatic vessels, remain indefinitely hypertrophied. They are very rarely the seat of a frank inflammation, and when such inflammation does occur, it is always the consequence of common causes, apart from anything specific. If the buboes which succeed indurated chancre suppurate, a circumstance which still more rarely occurs, they never furnish *specific pus*—a fact which our learned *confrère* of Brussels, M. Thiry, has clearly established, and which I have myself confirmed. They merely furnish *simple pus*, unless there exists a secondary accident; but in any case, this pus *does not inoculate.* It is well understood that the inquirer must not allow himself to be deceived by new chancres which the patient may have contracted on the seat of old indurations, and which, following the law of non-indurated chancres, may give rise to virulent adenites with inoculable pus. These chancres, with a *borrowed* indurated base, are not rare.

The indolent adenitis which I am here describing as the basis of the *specific induration* of chancre, is an accident of secondary transition; the more complete development of this accident we shall find in the constitutional buboes properly so called, or the posterior cervical adenopathies, which constitute the second species of syphilitic adenopathy of which I shall speak hereafter.

In view of the statement I have made, permit me, my dear friend, to present the two following propositions, of which you will comprehend the whole bearing, in their relation to the prognosis of the affection. An experience of more than twenty years enables me to advance these propositions with entire confidence in their correctness:—

1. A bubo which suppurates specifically, that is to say, which furnishes inoculable pus, is never followed by any symptom of constitutional infection. This is a more valuable sign than the absence of the induration of the chancre which has preceded it, and which may deceive us.

2. The multiple indolent adenitis, following an indurated chancre, is an additional proof—and sometimes the only proof we possess when the induration of the chancre has not been verified—that the constitutional infection has certainly taken place.

Now, are you willing to allow me the privilege of presenting some therapeutic reflections, arising from the principles we have laid down and admitted?

And first, we cannot at present admit that we possess but one method of treating venereal bubo; for, as we have just seen, venereal bubo does not constitute a pathological individuality. This bubo does not, by any means, always present the same characters, and the differences which it exhibits do not always consist in its greater or less depth, or in its greater or less acuteness.

We cannot, as in the time of Bell, pretend to present with certainty, or to determine at will, the suppuration of buboes, without taking into account their starting-point and their intimate nature. These ingenuous dreams of syphilographists of bygone times have faded away. At the present day, no one believes that there can be made to pass, exactly by the same lymphatic vessel which gave passage to the virus, a sufficient quantity of mercurial ointment to destroy this virus in the ganglion where it was arrested. We know too well that mercurial preparations, placed in direct contact with the virulent pus, on primitive venereal ulcers, or on buboes chancrously ulcerated, not only do not always neutralize the specific morbid secretion, but, on the contrary, very often augment its activity.

In the great majority of cases, the suppuration of sympathetic buboes may be prevented by the methodical use of antiphlogistics and resolutives; but these means fail when applied to the bubo of absorption which follows non-indurated chancre; and we never succeed, whatever be the means we employ, in determining a *specific*, virulent suppuration in the adenopathy symptomatic of indurated chancre. It is because the inquirer has failed accurately to determine the character of the affection that he has been so often deceived in this matter.

It is well understood, you know, that I shall not embarrass myself with the details of treatment; but you will allow me to refer to the use of leeches. Very well! When acute adenites succeed non-virulent venereal accidents—blennorrhagia, for example—we may apply leeches at a tolerably advanced stage of the disease, without being much disquieted as to whether the bites are more or less distant from the centre of the seat of inflammation. In cases, on the contrary, where the bubo is virulent at its starting-point; when it is a non-indurated chancre; and when the rational diagnosis indicates the existence of a virulent adenitis, it is necessary, if the inflammation can still be combated by leeches, to concentrate them upon the very point which is inflamed; for, if suppuration supervenes, and the abscess opens or is opened, each leech-bite, which is not cicatrized, will be inoculated by contact with the pus which this abscess will furnish.

I have seen very grave accidents occur in such cases, through ignorance of the laws of inoculation; I have seen numerous leech-bites successively become affected with the contagion, and give place to as many chancres; and the intensity of the disease did not by any means diminish, as these became successively developed. The most remarkable example of this kind I have observed was furnished me, several years since, by a receiver of the public revenues, in whose case thirty leech-bites became as many chancres, which subsequently assumed the serpiginous form. The primitive accident cost him ten thousand francs; the cure was less costly, although the treatment lasted more than six months. A young girl, who had witnessed a similar accident in her lover, one day came to consult me relative to an acute sympathetic adenitis. I advised leeches, and she immediately commenced weeping. I inquired if her tears were caused by fear of the pain arising from the bites? She replied no; but that it was on account of her profession, which was to sit as a model for painters. All at once, however, she consoled herself by saying: "After all, it can be done as you wish, since I am sitting, just now, for a saint in drapery." In fact, at the following *salon*,* I recognized my patient in a repentant Madeleine! This, my dear friend, is an historical fact; and you have given me the privilege of relating it.

As regards the opening of suppurated buboes, when they are not virulent, you will, whether you make one or several punctures, most usually succeed in obtaining a prompt cure, the result of which depends much more on the nature of the disease than on the operative process. But in regard to buboes with a specific *dépôt*, whether you make one or several openings, the pus which traverses the latter inoculates their borders, and soon transforms them into chancres which, most usually, by their augmentation, unite and cause the destruction of the whole of the skin covering the abscess, whatever measure to prevent this result be adopted. Those who believe in the constant efficacy of multiple punctures have either failed to notice, or failed to state, the true facts in the case. When the *dépôt* is small, it is necessary to make but one puncture or one incision; when the skin is yet thick, and the *dépôt* too large, multiple punctures may be resorted to; but, if the *décollement* is considerable, and the skin thinned or altered, the Vienna paste, wisely employed, cures with more rapidity, as it soon destroys, within well-defined and appropriate bounds, that which diseased Nature takes a long time to *eat* irregularly. When we are guided by adequate knowledge in these operations, the artistic cicatrices become much less visible, and exhibit much less deformity, than those which are otherwise manifested.

In every case in which we believe we have to deal with a virulent bubo, it is essential to open it too soon rather than too late.

Do not be impatient, my dear friend; I have scarcely any-

* There is, each year, a public exhibition, in Paris, of the works of French artists; and this is often called the *salon*.—TRANS.

thing more to say, for I am coming to the buboes symptomatic of indurated chancre, with respect to which many persons give themselves a great deal of useless trouble. These buboes, aside from complications which may demand a particular treatment—antiphlogistic, for example, if inflammation intervenes, or antistrumous, if scrofula accompanies them—require scarcely any local application, the general mercurial antisyphilitic treatment being the essential, it might almost be said the only, condition of their cure. Whether the mercury penetrates the system through the digestive organs or through the skin, it acts efficaciously against this species of bubo, without the necessity of traversing this or that vessel, or following precisely this or that path. But we must not thence conclude that direct friction and resolutive plasters are useless, or fail to appreciate the benefits of compression.

Yours, RICORD.

LETTER XXVIII.

MY DEAR FRIEND: I have now arrived at a question, so to speak, palpitating with life; the question concerns the constitution! But have no fear of the courts;* for, be it understood, it is the syphilitic constitution to which I refer. Alas! there is no more unanimity with respect to this than the state constitution, and all the efforts which I have made, up to the present time, to reconcile discordant elements, have only induced opponents to deny even the principles which they have always possessed. Yes, my dear friend, the pretended *conservatives*, those who do not wish to believe anything contrary to the dogmas laid down by the *Fathers* of the pox, have become heretics. They deny to-day what they wrote but yesterday; and they will again deny to-morrow what they write to-day. True retrograde revolutionists, blotting out the immortal works of Fer-

* This was written in April, 1851, at a time when so much was said in France relative to a revision of the constitution.—TRANS.

nel and Hunter, plunging us again into the darkness and confusion of the fifteenth century, they seek to carry us back to the period when the pox, rendered active by an epidemic genius till then unknown, struck patients, physicians, and the whole world, with a profound stupor, and gave rise to a belief in the most marvellous stories. Proteus, in forms indefinite and intangible—Chameleon, in colors changing without cessation, and unceasingly deceptive—a last plague from Pandora's box—a prodigy fallen from the stars, according to the politic and poetical Frascator—syphilis at that time pursued its destructive career, governed by no laws, and restricted to no limits, whether of time or space, drawing in its train the desolating cortege of all human infirmities, with all their innumerable theories. But, my dear friend, are we living to-day in the year 1851? Allow me to remain in my own time and in my own century, and to study the pox with the aid of other methods and processes than those which served the purpose of the historians of the epidemic at the close of the fifteenth century. Now, what does present experience teach us? If the *tincture* of Alexander Benedictus is not destroyed, it has, thanks to the progress of hygiene and therapeutics, at least lost its vivacity; and the unclouded eye can now seize all its shades.

Were I one of those least influenced by the doctrines of the physiological school, I should, in order to protect the syphilitic virus from the tempest of *inflammation* which threatens to carry away all before it, struggle with no less energy than now against these retrograde revolutionists, who no longer wish to recognize any laws in pathology; who, seeking to subject everything to the caprices of chance, bring into this department of medicine a love of that anarchy which is borrowed from other very singular dogmatic systems.

Although I am often obliged to suffer a long interval to elapse between these letters, notwithstanding the pleasure I have in addressing you, you have not lost sight of the logical or clinical order, in which the first venereal accidents we have examined are produced; I have especially insisted upon the different nature of the *virulent* and the *non-virulent* affections, and have shown the varieties of the first to pertain only to syphilis.

I have already told you, and it is especially necessary here to reiterate my assertion, that the general syphilitic poisoning, the constitutional syphilis, or the syphilitic diathesis, if you please, is either established only after the development of chancre, or is acquired from hereditary predisposition. Do not fear that I am about to bring forward again all the arguments on which I have based the proof of this important proposition, and by which I have deduced the distinctive characters of blennorrhagia properly so called, and the ulcer which constitutes the first and inevitable accident of the syphilitic contagion, and which is only wanting in the syphilis arising from hereditary taint.

No constitutional pox exists which has not been preceded by chancre. This is a truth which is more consolatory than the doctrine I combat—a doctrine which assumes the pox to be an unconquerable enemy of the human race, everywhere present and everywhere invisible, and which, like the lion of the Scriptures, is unceasingly watchful, *Quaerens quem devoret.* Yes, it is my hope that, at a period which is not distant, this fantastic doctrine will be appreciated at its just value; and that it will no longer frighten those who seem unwilling to examine it closely. My hopes are encouraged by the recent efforts to bring it into notice; and, did you not give such frequent examples of polemic courtesy, I would add that these were the last convulsions of an expiring doctrine.

But does chancre always produce the constitutional infection? If it does not, what circumstances determine its production, and what takes place subsequent to the infection? These are questions to which I should be very glad to reply *in extenso*, but this I am unable to do by reason of the epistolary style I have adopted. And first, you have seen that chancre is the only accident which can be produced with inoculable pus—the one which all inoculators have produced, among others even M. Vidal himself when he inoculated M. Boudeville. You have also seen that nature does not operate differently from art, when we know how to watch her course. Chancre, then, is the first accident which follows contagion, and consequently the primitive act, despite the assumption of *those who inoculate secondary acci-*

dents of every variety, and who, consequently, no longer admit chancre to be a primitive accident. According to these inoculators, primitive syphilides and buboes *d'emblée* are manifested; but primitive ulcers no longer exist. Read their books and their journals; I do not know whether the infecting coition will not, one day, even become in their opinion a consecutive accident. That would be a slightly primitive conception.

But, while admitting the *autocracy* of chancre, I told you that daily observation proved that all chancres did not inevitably give rise to buboes any more than to constitutional syphilis. I told you that the *indurated chancre* alone infallibly produced the adenopathy, and particularly the constitutional infection; that the induration was the proof of the general poisoning, and in a measure the first secondary manifestation. I have been made to say that there is no constitutional syphilis without indurated chancre, when I only said that every indurated chancre is followed by constitutional accidents; these two statements are not exactly alike. In fact, constitutional symptoms sometimes, though rarely, supervene in cases which seem exceptional, but which are not so in reality. I have told you of all the deceptions to which we are liable in our search after the specific induration of the chancre, and how the diagnosis might be completed by a knowledge of the symptomatic adenopathy. The true non-indurated chancre, without ganglionary affection, or with adenites *specifically suppurated*, never infects the economy. These propositions are absolute; but, in order to establish them, our diagnosis must be of the most rigorous nature. We must not do as my learned *confrère* and former disciple, M. Diday, of Lyons, did when he wished to find non-indurated chancres capable of producing constitutional syphilis. We must not content ourselves with a diagnosis made piecemeal, like that with which very honorable *confrères* furnished M. Diday from memory, without *direct* or *accessory* symptomatology, and which necessity alone caused him to accept. Something better, far better than that is necessary to obtain a true diagnosis.

Therefore, there are chancres, and perhaps these constitute the most numerous variety, which do not infect the economy, and which can most frequently be recognized. I will not return to the details of this question, which I have already partly treated in my preceding letters. At present, I only wish to refute what has been regarded as a conclusive objection to the consoling doctrine which teaches that chancre may be only a local accident. It has been asked: How can a virus be placed in contact with the circulation without affecting it? Is not this poisoning effected, on the contrary, from the time when one point of the economy is contaminated? But do those who use this language forget the numerous cases in which inoculations of variola have failed, and in which it is impossible to vaccinate, as well as the numerous cases of malignant pustules and malignant anthrax, which are localized, or destroyed *in situ?* Why should not the syphilitic virus, which is less active, have the same privilege? But let us not insist on this fact, since my opponents will not be convinced; let us rather enter upon the consideration of other questions.

You already know that the constitutional infection does not depend on the number of chancres which exist, nor on the seat, nor on the extent, nor on the absolute duration of the affection, and that it only supervenes in certain circumstances which I have endeavored to specify. Consequently, it is not of this that I wish to speak, but of the time which elapses between the constitutional manifestations and the *implantation* of the virus, or the production of the primitive accident. What interval elapses between the appearance of the chancre and that of the first secondary accidents?

Whatever be the process by which the infection is accomplished, in traversing at first the lymphatics, or in acting immediately on the blood—whether the virus be a ferment which finds in our humors a fermentable matter whence results a new poison which has lost the property of being inoculable; or whether the poisoning be accomplished otherwise—is it impossible to determine the time of the incubation as understood by Cataneus? Here comes up again, my dear friend, the famous India-rubber doctrine, according to which secondary accidents are assumed to show themselves some weeks subsequent to contagion, or after an indeterminate number of years—from fifteen days to thirty years, or more! Is this clinical truth? Is this what observation teaches us, when we really know the starting-point of the disease, and seriously desire to ascertain the point to which it will extend? If we do not know how to recognize the accidents reputed primitive; if we are unable to discern that which infection alone can produce; and if constitutional syphilis is considered as the sum or result of all the blennorrhagias, the ulcerations, and the ganglionary engorgements, which have previously existed, no matter what period has elapsed between the appearance of the one and that of the other, it is very certain that we shall arrive at the same conclusions as the author of the *Treatise on Syphilides*, who, rejecting all primitive accidents, finally admits too great a number of them. The starting-point of a constitutional syphilis will be, in some patients, five or six blennorrhagias, and often as many chancres and buboes; and such is the nature of the infection, that, though it may have commenced thirty years before, it only manifests itself when successive additions of virus have produced the quantity necessary to act! If you think I exaggerate, read the titles of most of the observations contained in the book to which I allude, and you will be astonished. As I have already stated, we might as well say that variola is due to successive infections, which, passing through different epidemics in a certain period of years, only manifests itself at last after it has accumulated in sufficient quantity. You might as well be told that the vaccination which finally succeeds, in a person who has many times been unsuccessfully vaccinated, is not the result of the last attempt, but the result of all previous attempts to vaccinate. You would reply that those who inculcate such errors are unacquainted with the laws which govern virulent affections, and that, by reason of this ignorance, they cast these laws aside. And, I must say, I would be entirely of your opinion.

But let us return to the facts which clinical observation so uniformly teaches; to the facts which I am willing to verify, at any time, to the satisfaction of unbelievers. Let us see what takes place after the chancre is *duly diagnosed*, and flanked (permit me to use the expression) by its ganglionary pleiades. Well! when no specific treatment has been adopted, when the disease has been left to itself, *six months never* ELAPSE WITHOUT

THE MANIFESTATION OF SYMPTOMS OF THE SYPHILITIC INTOXICATION.

This is another law which can only be counteracted by means of treatment. Inquire of my conscientious and persevering colleague, M. Puche, who has verified the law in hundreds of observations made by himself, without ever meeting an exception to it. Six months! Ay, six months; and even that is a long time, for most usually it is from the fourth to the sixth week, frequently from the second to the third month, and much more rarely from the fifth to the sixth month that secondary accidents supervene. This truth, my dear friend, is one which cannot be too often repeated—a truth of immense importance, and of which I am as firmly convinced as of that maintained by Galileo.

This truth laid down, permit me to say a word, before proceeding farther, concerning the syphilitic *disposition*, as it was called by Hunter—that state which is produced by the primitive accident, and from which other accidents are about to spring. The syphilis is certainly an intoxication, or poisoning, which, like that produced by variola, vaccinia, and typhoid fever, cannot take place, excepting by virtue of a predisposition. This predisposition does not always exist, and its production a second time is prevented by the first infection; but the infection is, on that very account, a persistent poisoning, which impresses on the economy a profound modification, whence results a morbid temperament, that is to say, a diathesis. Nevertheless, you know that, in certain treatises on general pathology, constitutional syphilis is not considered as a diathesis; and yet, is there any diathesis which is more marked? Are we cognizant of any general state in which symptoms of a more highly specific nature are produced, repeated, and transmitted with more regularity by means of hereditary predisposition? But what fact has not been contested?

The order of evolution, in the different constitutional manifestations, has been especially contested. Some writers, whose knowledge of the disease is not equal to that of Thierry de Hery, who forget the judicious precepts of Fernel, and are deaf to the persuasive voice of Hunter, assert, as I stated at the beginning of this letter, that syphilis pursues an erratic course; while, in reality, its course is so systematic and orderly that an illustrious professor of general pathology, M. Andral, said to me, one day, that syphilis ought, so to speak, to serve as the key to all pathology.

Here, it must be understood that, to comprehend and fully appreciate this order, the disease must be observed in its *natural* state ; unaffected by artificial influence, or therapeutic modifications. When appropriate cases are selected—an immense harvest of which was recently furnished us by the physiological school —we observe accidents succeed one another, and differ from one another, according to the time of their appearance, to the greater or less duration of the infection, to their seat, their number, their arrangement, their form, their termination, their influence on generation and hereditary predisposition, and finally to their greater or less amenability to this or that medicinal agent, or, if you please, to this or that specific.

Syphilis may be compared to a ribbon which is more or less quickly unrolled, but whose shades change after a certain number of turns, and whose *free end*, corresponding to the person who communicated the disease, no longer resembles the extremity adherent to the bobbin, which corresponds to the skeleton of the affected individual.

These shades, which are so well defined, so often exactly measurable, can never indicate with certainty the acute and chronic states; for each may assume the acute or chronic form, without changing in any respect the other characters on which my classification is based. No; between the primitive, secondary, and tertiary accidents, the difference presented by the acute and chronic states is not the only difference which exists. Syphilis, in its totality, is so much the more chronic, as it has lasted the longer. It does not follow that this is one of those great truths which need no demonstration. What I wish to say is, that the absolute duration of the disease is not the only cause of the differences observable in the seat and form of the accidents which it produces. Thus, roseola, which, according to some writers, is an acute accident, may be reproduced several times in the course of the first and second years of the infection, and, perhaps, sometimes at a later period; whilst the osseous affections, which the same persons would range among the chronic accidents, appear, in some cases, during the first five or six months of the constitutional infection.

You will allow me, in my next letter, to return to this subject, and to present the distinctive characters of these accidents.

Have a little patience, and should no accidents other than those treated of in these letters happen to us, we shall finish our communications, although the pox seems to be an inexhaustible theme.

Yours, RICORD.

LETTER XXIX.

MY DEAR FRIEND: I must now be slightly unfaithful to my programme. You will pardon my digression because of the reason which has induced it. You know that, at the present time, a discussion is going on relative to the inoculation of secondary syphilitic accidents. A great German memoir on this subject has just appeared. I never comprehended better, than on this occasion, the remarks made to me by one of our most *spirituel* Prussian *confrères*, who resides in Paris: to wit, that he thanked God daily for causing him to be born a German. While rendering justice to the learned German, as far as possible, I observed to him that one might be almost as well contented with being born a Frenchman, an Englishman, &c.; and that I did not understand the reason for his thankfulness! "If I am thankful to the Supreme Being," said he, "it is because I am familiar with German, and have no need to learn it." This reason appeared sufficient to me, for I was unacquainted with this remarkable language, and still understand all its difficulties.

By reason of my ignorance, then, of the Teutonic language, I have been obliged to wait until the extraordinary work of M. Waller, of Prague, upon the contagion and inoculation of secondary accidents, should be translated, in order to speak to you of it. The translation of the part which treats of certain diseases of the skin and of a particular syphilis, has been given by two friendly journals, the *Gazette des Hôpitaux* and the *Annales*. These journals have given proof of much *abandon* and courtesy towards me, for which I thank them. The *Gazette des Hôpitaux* blames M. Waller severely for having, in imitation of M. Vidal, communicated syphilis to healthy individuals. The *Annales*, but half contented with, and somewhat punished by, M. Waller, publishes the work *with much reserve*—a highly judicious course of conduct.

Be this as it may, I have been enabled, thanks to these translations, to read the work of M. Waller, which is divided into two parts, the one clinical, the other experimental, with a preamble of generalities.

Must I tell you, my dear friend, that, in perusing the book from the beginning to the end, I constantly thought I was reading German; that is to say, a language which I did not understand.

I was unable, in fact, to conceive why M. Waller, who endeavors to prove the contagion of secondary accidents, and the possibility of their transmission by inoculation, and even of the transfusion of secondary syphilis, through the inoculation of the *syphilitic* blood, reproaches M. Cazenave with admitting, without proof, the existence of primitive syphilides, and yet dares to tell him that such assertions are, in a measure, only opinions; and since they are in no way demonstrated with exactness by experiment, they can prove nothing against the arguments of the adverse party. In fact, M. Waller proves, what I have already established, that the pretended primitive syphilides of M. Cazenave are all consecutive to *chancres*, which are duly and clearly determined.

But the physician of Prague, who wishes to arrive at the demonstration of the possible transmission of secondary accidents by the contagion called physiological, and by artificial inoculation, believes that, if I have not succeeded in my experiments, it is because I have sought, first, to produce primitive ulcerations by the inoculation of secondary varieties; and because, secondly, I have inoculated, with one exception, only venereal patients; that is to say, the same patient already affected with secondary syphilis.

My dear friend, I am convinced that M. Waller has failed to comprehend my experiments, unless he understands French better than I do German. When I stated, once and again, with all those who have repeated my examinations, that secondary accidents, *rigorously diagnosed*, are not inoculable, I did not merely establish the fact that they do not produce chancre, but that they give rise to no other result. As to the inoculation practised upon the patients themselves, I am still at a loss to understand how people who admit that mucous papules of the scrotum or of the labia majora may be transmitted by contagion to the skin of the neighboring thigh, should not admit—if the secretion from these patients were really contagious—that this contagion may be artificially produced under the same conditions, and that it is only possible when transmitted from a diseased to a healthy individual. I had thought, up to the present time, that logic at Prague was the same as at Paris, and that the dif ference of languages amounted to nothing on this question. M. Waller says that, in the numerous experiments which I have made, one healthy subject alone was inoculated with the pus of secondary ecthyma, and that after having established the fact that, up to the third day, no result had supervened, the patient was *discharged*. The inoculated person was not a patient, nor was he discharged, for the inoculation completely failed. This person was M. Ratier, who arranged all the observations of my *Treatise on the Inoculation of Syphilis*, and who remained ten years with me—a period of incubation, perhaps, more than sufficient to cause something to be hatched, had there really been anything to hatch.

But let us come to the clinical facts to which M. Waller accords so great a value that he demands on their behalf a credence which critical science is happily not bound to yield. To *believe* and to *know* have never been synonymous with me; and in so far as a proposition is not demonstrated, I remain among the *doubters*.

It is certainly not rare to see individuals with mucous patches (whatever be the synonyme) claiming the assistance of physicians, and affirming that they have never had either primitive ulceration or clap; in these persons, no cicatrix of chancre can be discovered. But he who knows how to seek and to recognize the primitive accident; who knows that the patient may have an interest in dissembling, or may be really unacquainted with, the truth; who knows that the accident may be everywhere, and not unfrequently concealed; who knows, by experience, that the infecting chancre is especially the one which, *in the great majority of cases*, leaves *no cicatrix*, is satisfied that the word of the patient or the absence of any trace of the primitive accident is not sufficient ground for a proper conclusion, though M. Waller seems to think it is. If, in ninety-nine cases in one hundred—and I here take a small proportion, so as to treat my opponents with fairness—you find that chancre or hereditary predisposition accounts for a constitutional syphilis, and meet with but one instance in which this connection cannot be traced, will you, instead of holding your opinions in abeyance, take this apparent exception for a general rule? As for myself, the profession of faith which I have always made, and which I still make, is this: The clinical facts which I have collected, perhaps in greater number than my adversaries, have not furnished me absolute and incontestable proof of the contagious property of secondary accidents; my experiments, up to the present time, have shown these accidents not to be inoculable.

In the clinical observations cited, has the state of the patient supposed to transmit the disease been verified at the moment of contagion, which may easily be done in cases of chancrous infection; and has the history of the patient, after the physician has been rigorously assured of his previous sanitary state, been traced subsequently to the period immediately following the suspicious contact? No; in no instance! In all these observations, in every one of these thousand and one nights of syphilis, what do we see? That patients come to you several weeks or several months after the contagion, and exactly at the time when they and those whom they had infected had arrived at the proper period for the development of secondary symptoms. Look, my dear friend, at the observations of M. Waller himself, whom I think honest in his belief, and tell me whether they differ in any respect whatever from those on which I have so frequently commented in my preceding letters.

The first case occurred in a "respectable family of Prague;" and of such families—without *amour propre*—there are many in Paris. In this family, a daughter, two years of age, presents mucous patches on the labia majora, on the perineum, and around the anus. The father and mother assert that they never had the venereal disease. The other children, *to the number of eight, are well, and have always enjoyed good health.* In seeking the cause of this accident, it is discovered that the servant, admitted to the house only *three months before*, has mucous patches at the corner of the mouth and on the internal surface of the lips, as well as on the tongue, the tonsils, and the velum palati. She also presents isolated points, covered with a solid exudation (what is that?). Mucous patches are found upon the labia majora and (here we are) upon the fourchette is the *distinct cicatrix of a chancre!* Ah! Monsieur Waller, never has France accused learned and conscientious Germany of levity; and yet what can we think of your distraction, in citing such an observation, when you were not necessitated so to do.

Three cases which follow are perfectly analogous. I shall not cite them; for, like myself, you will always be convinced that you are reading a foreign language, and that you do not understand German.

Finally, not to fatigue the reader, and as a moral to the preceding fables, M. Waller cites the case of three pederasts who had ulcerated mucous papules of the anus, and who affirmed to him that the disease commenced there without known cause. One of them had communicated it to his brother by lying with him! Happily, the recital terminates at this point.

After these admirable proofs of the contagious character of the mucous tubercle, M. Waller, always appearing to understand French about as well as I understand German, assumes the opinions to be my own, which I comment on and combat, relative to the mucous tubercle, in the work I published in 1838. In this case, error is difficult, unless, for the same reason as before, he has failed to understand my propositions, which he cites, and which I ask your permission to re-quote, because, since the year 1838, I have only more and more confirmed them:—

1. Mucous tubercle is never inoculable (this is also the opinion of M. Vidal).

2. It must be referred to the secondary accidents; it is a proof of constitutional syphilis.

3. Its secretion, by acting as an irritating matter, may determine an inflammation of the tissues with which it is placed in contact.

4. When mucous tubercles or mucous pustules have transmitted the pox to another individual, the circumstance is due to the fact that, at the moment of contagion, there existed other accidents *specifically contagious, as in the cases of M. Waller.*

5. Like other secondary accidents, the *true* mucous tubercle cannot be hereditarily transmitted.

The efforts I have made to arrive at these conclusions have, by no means, been so great as M. Waller seems to believe, and have not fatigued me in the least. I have only taken the trouble to study chanres as you are familiar with it; to follow it in all its phases: and I have thus learned not to confound it with mucous tubercle, which it resembles at a certain period, and at last not only assumes the aspect, but even the very nature of the tubercle; that is to say, it passes from the state of a primitive inoculable accident to the state of a secondary accident which is no longer inoculable. It is not my fault, my dear friend, if nature effects this result, and if chancre is not the same at its beginning and at its end. I bow to nature; nothing more. Moreover, the fact gives me no trouble; for I do not believe, with M. Waller, that it would be a *very fortunate* circumstance, if primitive and secondary accidents alone existed; and that it would be a great misfortune were science to succeed in discovering a process by which a fusion would take place between the elder and younger branches of syphilis.

We are now once more among the nurses! Watzka is about to furnish an overwhelming proof in favor of the transmission of secondary syphilis from nursling to nurse, and *vice versa.*

This woman, at the moment of her admission, presents at the base of each nipple an oblong mucous patch; on the right breast the size of a bean, on the left that of a pea. This patch *rests upon a broad base*, and is covered with a plastic exudation. There exists a deep ulceration upon each of the tonsils, accompanied by catarrhal inflammation of the throat. On the 9th of March, there appeared, in addition to these phenomena, a maculated and papulous exanthema, extremely abundant over the whole cutaneous surface. The genital organs, apart from several cicatrices, the results of accouchement, present no abnormal indications. The husband of the patient is healthy.* She pretends that she has been infected by her foster-child, which was intrusted to her care by the Foundling Hospital, three months previously (December, 1847). At the end of the third month, towards the middle of February, she first noticed upon the left breast—and, seven days later, upon the right breast—a red and slightly excoriated point, which gradually became elevated, and subsequently acquired the tuberculous form already described. By reason of the absence of subjective symptoms, the patient failed to detect the commencement of the affection of the throat. Besides, at the end of four weeks, she was cured by the proto-iodide of mercury and the use of warm baths. The foundling confided to her care was a girl (Catharine Holub), which, at the period referred to, was perfectly healthy, and consequently had neither primitive nor secondary accidents; but, soon afterwards, it exhibited on the face, and particularly on the lips, a pustular eruption, to judge from the nurse's description. It was only at the end of three months that she returned the child to the Foundling Hospital, where it shortly afterwards died at the age of four months. "*I was not able, it is true, to procure information with respect to the manner in which the syphilis had acted on the living child;* but, by consulting the register of the hospital, I find that it was treated in the sick children's department for a syphilitic pemphigus." In the account of the autopsy, scales, eschars, cicatrized points of a bluish and dark color, especially in the mouth and on the neck, are mentioned among the external symptoms. *As the cause of death*,

* It seems that the secondary accidents were not contagious with respect to him!

there was noted a general anemia, with catarrh of the bronchi and of the colon.

At the time that she suckled this foundling, Watzka also nursed her own child, a strong and robust little girl. "This child, aged nine months, had, according to the mother, several days before entering the establishment, an eruption upon the right thigh, which we considered to be formed by syphilitic tubercles of the skin. They were rare on the external parts of the thigh; their size was that of a pea; they were nearly circular, and had a dirty red tint. Some were dry; others were covered with scales; others, finally, had commenced to ulcerate. On the rest of the body there existed a maculated and papulous exanthema similar to that which the mother exhibited. Some doses of calomel, and at a later period lotions with sublimate and warm baths, cured this child in the space of three weeks.

"The course of the disease, in the mother and in the child, had already struck me on account of its singularity, and had caused me to suspect a contagion derived from the foundling; but what still further confirmed me in this supposition was the fact that, on the 1st of April, the mother of Watzka, an old woman seventy years old, lean and dry, entered into my service; and, with the exception of the mucous papules of the nipples, she presented the same syphilitic manifestations as her daughter: namely, profound ulcerations of both tonsils, and maculated and papulous exanthema of the whole body. The syphilides were excessively numerous, and were first developed on the left cheek and on the left side of the neck, where this woman, who attended the children nursed by her daughter, was accustomed to carry the foster-child when she wished to quiet it, or put it asleep. The genital organs presented no trace of antecedent syphilitic disease. She was cured by the internal use of the sublimate."

Ah! M. Waller, do you, who find others superficial and sometimes obscure, believe yourself clear and serious—have you placed your clinical knowledge and experience under contribution—in the statement of this case? Why do you, without hesitation, taking no account of the time for which Watzka was diseased, call the ulcerations of the breast, which you so well describe as having a *broad base*, mucous tubercles? I know not what constitutes mucous tubercles at Prague; but at Paris, your mucous tubercles would be very fine *indurated chancres, with a broad base*, and at the period of prominent reparation (*ulcus elevatum*). You say nothing of the neighboring ganglia. You are evidently unaccustomed to carefully analyze your patients, and are always contented with a superficial examination. Be this as it may, I can assure you that, had you inoculated the pus of these pretended mucous tubercles, which evidently proceeded from a chancre, you would have obtained no result.

To proceed. In view of the two indurated chancres of the breasts, it is very evident that Watzka had a fine pox. But who gave her these chancres of the nipples? Was it the *foundling?* The child was healthy when given to the nurse; its parents had never been seen, and nothing was known of their history; nor was the commencement of the disease in the child noticed. It became diseased in consequence of its relations with the woman who nursed it; that it subsequently died of the pox is possible, and even probable; but what proof is there that the woman did not infect it as she infected her own child? How can it be affirmed that the chancres on her breasts were not communicated to her by one of those processes which I have already indicated, or by a still more ingenious process? Prove to me the contrary otherwise than by the assertion of the patient. Are you about to invoke, in support of your hypothesis, what happened to the mother of Watzka, a woman seventy years old (not exempt on that account from liability to a primitive accident, as may have been seen in my wards), who, being accustomed to place the children, nursed by her daughter, upon her left cheek, contracted on this spot a syphilide as a first manifestation; consequently, a primitive syphilide? But you do not wish this proof; you cannot invoke it; for, with reason, you do not admit the primitive syphilides of M. Cazenave.

Be light, M. Waller. I do not object to this, for my taste does not incline me towards dull people; but be logical. On the other hand, you found no traces of syphilitic disease upon the genital organs. Did you examine with the speculum? and even if you had, you know as well as myself that ninety-nine times in a hundred chancre leaves no trace in the vagina or upon the neck of the uterus. But, let us say no more of this case.

We pass on to the second case, to Nowak. Who established the diagnosis of the disease of the child, and of the first accidents of the nurse? The patient herself! And you accept this diagnosis, unconditionally, having seen the patient for the first time not until three months after the beginning of the disease. While I contest your own diagnosis, though you are a physician to a venereal hospital; while I call *characteristic indurated* chancre what you, on hypothetic grounds, call mucous tubercle, you do not even doubt the science and the just appreciation of Nowak. This woman, you say, who might have syphilis, despite her nodular erythema, of the size of a hen's egg—which syphilis does not prevent, but which it does not produce, in France—only had cicatrices, resulting from accouchement, upon the genital organs! I would be very grateful to you, if, in your next work, you would show me how, in all cases, you distinguish the cicatrices which result from chancre from those which follow accouchement, especially when they exist together upon the same regions. For my part, I confess my profound ignorance in the matter, and frequently confound the two. What shall I say also of the youngest child of this woman, which you received at the same time with the mother—that is to say, three months after the commencement of the disease—and in which the mother had first *diagnosed* mucous papules of the vulva, which no longer existed when the child was submitted to your observation? I will tell you that I no more accept this diagnosis than that of which you furnished the elements in your first observation.

What shall I say, further, of the son of this woman's husband, a boy *aged fourteen*, who had *a syphilis of the bones and periosteum*, affecting both tibias, with superficial ulcerations of the tonsils, and mucous papules of the anus? Where and how did the disease commence? At the anus? By the suckling? The two daughters of Rosalie Nowak, who both reside with the son of the husband, in the paternal mansion, have also complained for a long time of pains in the bones! Oh! Voltaire, thou art robbed; for this is the history thou didst give of our unfortunate *confrère* Sidrac, who caught the pox from his wife on the first night of his marriage, and to whom his chaste spouse gave as her excuse that it was a family disease. With the good-natured Sidrac, we can understand why the fables of Portal and of Vercelloni should have been successful; but with the knowledge and close reasoning of our *confrère* and friend, M. Bouchut, facts are presented for what they are worth; and wherever doubt remains, it is necessary to do what I have thought proper to do—to remain among the *doubters*.

But, my dear friend, for a few moments past I have been writing from Prague instead of from Paris. Excuse me; I shall return to you. We have a question concerning the blood to treat. M. Waller does not attack the syphilitic chloro-anæmia too severely. We shall return to the subject hereafter. Moreover, the question only concerns a difference of opinion relative to several globules in the blood of a syphilitic patient. The important point to which our attention is called is this: namely, *clinical* contagion of syphilis by the blood, as a prelude to the inoculation; or the experimental transfusion of syphilis by the blood! This proposition, my dear friend, has strongly moved me. I know, indeed, that we live in a world in which the range of action is from the possible up to the exclusively impossible. I have consequently read the two cases in support of this assertion—distrusting, however, the idiom which I did not understand—and I find that a young man, who had never had connection with a woman, who had never had either chancre or blennorrhagia, lived with a girl* for a long time. Sometimes it happened, after frequently repeated coition, that this act was accompanied, in both parties, by the discharge of a few drops of blood. Now, some months after the commencement of this *liaison*, the young man perceived, upon the corona glandis, *acuminated*† condylomata, which, in spite of repeated ablations and cauterizations, returned several times during two

* This girl is not called a public woman; she must then have been a *private* woman!

† Condylomata, vegetations, and mucous papules are identical in the eyes of those who do not observe closely.

months. To these was finally superadded a syphilitic psoriasis over the whole body.

Here the translation, in the *Annales*, of the particular syphilis of M. Cazenave, comes to an end. However, I think that the young and learned translator, M. Axenfeld, did not derive so little profit as myself from the work, and that he did not understand the German of the last phrase which was given by the intelligent translator of the *Gazette des Hôpitaux*, M. Marc Sée. Here is this remarkable ending: "*The patient had never been able to find the least syphilitic disease in his mistress; and, on minute inspection, I could not find the least trace of disease!*" Thanks to you, M. Sée; for this is really prodigious. Here are two individuals, who at first are absolutely without disease, who become excoriated, who bleed, and one of whom contracts constitutional syphilis by reason of *the contagious property of the syphilitic blood* of the other who is without disease! Here, again, I am perplexed by the German; I confess I do not understand this case at all.*

I have seen, in a French work by M. Richond, what seemed to me to be the same case; and if this author had been at Prague, I should have suspected him of having presented us with an importation from Bohemia. But M. Richond only gave us his case, with an equal degree of candor, for the purpose of proving that syphilis could, physiologically, be spontaneously produced between two healthy individuals. The idea of citing it in support of a contagious transmission never entered his mind.

Now, my dear friend, I scarcely dare to speak of the second observation, which is vouched for by Dr. Cejka. I am like Confucius; I respect the faith of others, when it springs from a kind heart, and can injure no one. If the question concerned a fact observed in private practice, and in consultation, I should never have alluded to it, but would be contented with giving my advice in regard to the treatment of the case; but since the fact is one

* M. Waller does not understand mediate contagions. I advise him to read the old authors; to read what I have written on this subject; and to inform himself in relation to the experiments of M. Cullerier

connected with science, I ask pardon of my honorable *confrère* of Bohemia. There are fathers, mothers, and husbands, who are as confident of the purity of their children and of their wives as he was of his patient, and who still, like him, have been deceived. The following observation needs no comment, and bespeaks the loyalty of M. Cejka:—

A man, about thirty years of age, healthy and vigorous, had a chancre in the month of December, 1848, which was treated by Dzondi's pills, and cicatrized towards the middle of February, 1849. In April, he had a slight sore throat, which disappeared of itself. Towards the end of June, a syphilitic iritis supervened, which was subjected to treatment for three weeks, and at the end of that time was cured. Fifteen days afterwards, the other eye was also seized; at the end of seven weeks, the disease was cured in both eyes, and disappeared without leaving any trace of its existence. Some weeks subsequently, this man married a young girl, whom Dr. Cejka saw almost every day. With the relations of this girl in the house of her parents he was well acquainted, and she had never had sexual intercourse. At the commencement of the marriage, coition was easy; but in December, 1849, the couple perceived a slight discharge of blood during intercourse. In January, 1850, the woman had a syphilitic psoriasis upon the scalp and face, and a maculated eruption over the whole body. In March, two small ulcerations appeared upon the lips, and at a later period condylomata became manifest upon the labia majora. The husband had no manifestation, either primitive or secondary; to-day he is still in perfect health. Thus, in him the same coition developed no morbid symptom; while his wife, who had never had prior connection with a man, *was not excoriated on the first night of her marriage*, but only some months subsequently! Can such things happen in Prague?

And that is the way that——the blood of syphilitic patients may transmit syphilis by inoculation?

But with some, at Paris, all these Bohemian cases have obtained great credit. Would you believe it, my dear friend? Would you believe that men, whose lips are constantly repeating the words *observation*, *scientific exactness*, *critical analysis*, and the like, hail with eagerness statements of this character, which, according to every rule of criticism, are imperfect, and will not, for an instant, bear examination and analysis! Oh! had I the impudence or ignorance to attempt the support of my doctrines by facts of this kind, would criminations against me ever cease? These criminations would be just; and I would not complain of them. But these facts come from abroad; they apparently come to the support of an opposition which is so indigent as to be forced to make any shift. Were they directed against any other pathological doctrine, they would be left in their *gangue*, obscure and unknown; but, inasmuch as they are brought against the syphilopathic doctrine which I defend, my opponents endeavor to polish them, to cut them, and to give them the appearance of real diamonds. But, let these opponents do and say what they will, the observations referred to are only paste, and are worthless. The enlightened taste and unerring tact of your numerous readers are such as to prevent the liability to deception by means of them.

Ask me nothing, at present, in regard to vaccination, as a means of propagating syphilis. Vaccination, like everything else in the world, has its enemies. It is already accused, wrongfully or rightfully, of being the cause of typhoid fever; and it is said to prevent children who have died of the latter disease, from dying, at an earlier period, of the smallpox. It may also be accused of propagating syphilis. But the accusation of MM. Viari and Wegelar has not yet led to condemnation.

I shall close, my dear friend; for nothing more of the remarkable, I may say even extraordinary, work of M. Waller remains to be noticed than the portion on hereditary syphilis, upon which nearly the whole world is agreed, and that concerning the transmission of syphilis by the milk, against which doctrine I protest, and which M. Waller erroneously believes to have been taught by the elder inoculators.

Yours, RICORD.

LETTER XXX.

My dear Friend: I have not yet done with M. Waller, of Prague, and I cannot part from this good Bohemian *confrère*, without saying a few words relative to the second part of his work, that is to say, that which treats of the artificial inoculation of secondary accidents.

I have said, notwithstanding "*the probability* of the contagious nature of secondary syphilis," that M. Waller has neither *been able* nor *willing* to adhere to that point. Therefore, he directed his attention to the secretions, to the morbid products of the secondary accidents, with the object of inoculating them. Up to the present time, M. Waller has failed, like myself, and every one who has experimented with the products of the various secondary accidents. His experiments, like those of others, were made upon the patients themselves; and although these patients must have been subjected during several months to his observation, he has never, so far as the results indicate, been more successful than other experimenters in witnessing, at any period, the supervention either of primitive or secondary accidents at the points inoculated. Is this result due to the fact that the patients, already under the influence of secondary syphilis, were no longer susceptible of a new secondary contagion? But the successive manifestations, the frequent relapses, should induce us, on the contrary, according to the views of my adversaries, to consider the individual already under the influence of the diathesis which is assumed to constitute a soil perfectly prepared to receive the seed of constitutional syphilis, and in all cases to produce the secondary accident. You are aware that, in regard to this point, a celebrated Napoleonic expression has been paraphrased. When it was attempted to be proved that the inoculation of chancre in individuals already infected was only the result of their syphilitic constitution, it was said that *to scratch a syphilitic person was sufficient to cause the pox to manifest itself.* But when the same subjects were inoculated, it was asked why, when they were scratched with the secretion of secondary accidents, no result was obtained. To this question there was either no reply, or the reply was that the inoculation was uncertain, and that the accidents, which were not inoculable, *were for that very reason contagious.* A strange and convenient reply, which calls to mind that which Pascal so thoroughly criticized in his *Provincial Letters.*

Permit me here, my dear friend, to call your attention to an argument which has often been brought forward. It has been said to me: If the pus of chancre alone is inoculable, it is because it possesses all its freshness, all its vigor, all its virulence; whilst the morbid secretions of the secondary accidents are perhaps modified, and weakened in such a manner as to be no longer inoculable, but only physiologically contagious. Let us suppose, my dear friend, that there are two assassins—and the syphilitic virus well deserves the name of assassin—the one very strong, the other very weak, who seek to introduce themselves into a house. The stronger, corresponding to the chancrous pus introduced by the lancet, waits until a passage is opened for him: the weaker, on the contrary, corresponding to the mucoso-purulent secretion of the mucous tubercles, traverses the entire house, although the way is not prepared for him! The product of the secondary accidents has its physiological *master-key;* and by this means it penetrates the system without your notice. When the school of Broussais formerly invoked the special orgasm and the functions of the genital organs, in order to explain the production of venereal accidents, it spoke of a physiological act; but in the physiological act of drinking a glass of water, or of swallowing a soup, where is the orgasm, or the part of the glass or the spoon which had been employed by a person affected with *secondary* syphilis, to infect a healthy individual who subsequently makes use of them? What, besides, are the particular physiological conditions exhibited by the lips and the tongue, and which are no longer met with when sought by the aid of inoculation? We have seen a great number of these physiological contagions; we have already spoken of them; and, when we have known how to seek it, the inoculable chancre has been found upon the edge, or at the bottom of the poisoned cup "Seek, and ye shall find."

But to return to our *confrère* of Prague. He has sought to use all possible rigor and precision in his experiments, and to place the facts he has presented in such a light as not to be questioned. Let us see whether he has succeeded.

In the first place, why did not M. Waller inoculate the patients who furnished the supposed inoculable matter, at the same time that he inoculated individuals reputed healthy? He has not told us that he believed them secure at the period when the secondary inoculations were made, although he had never succeeded in producing anything in them; but he was merely unwilling to make the attempt from the fear, he says, that, in case of success, the results he might obtain would be contested. This reason is not a good one. When a proposition which is much contested, is to be proved, one additional proof can never be of injury.

I would therefore urge our *confrère*, in his next experiments, not to neglect this advice, were it for the mere purpose of proving that the pus which does not inoculate when applied to the patient himself, does not prevent the healthy individual, who is inoculated with it, from subsequently presenting accidents, the true source of which must then be found.

Nevertheless, the experimenter of Bohemia, in his first trial, inoculated a child of twelve years, which was *healthy*, but affected with a porrigo favosa. This child was placed in a hospital where syphilis is treated, and where consequently it is endemic, easily communicated from one ward to another, and from one person to another in the same ward, and is thus adapted for all inoculations, and all accidental contagions.

A scarificator was applied upon the anterior part of the right thigh of the child; and into the still bleeding wounds made by this instrument, the *pus* of mucous papules was insinuated, and then retained by charpie likewise impregnated with the pus. Whence came the inoculated matter? A woman named Némec furnished it. This woman plainly presented, at the moment of the experiment, "*the cicatrix of a chancre.* She had, upon the labia majora and minora, mucous papules covered with a partly *croupy* and partly *purulent* exudation. Moreover, the croupy exudation existed in the throat, and was accompanied by an ulceration, in its forming stage, upon the tonsils. There was an eruption in spots over the whole body. This woman had, at the same time, a *vaginal blennorrhagia*.

"The next morning (7th August), and during the following days, the scarifications and the skin between them appeared very slightly inflamed, but at the end of four days all the wounds were closed; there was no trace of inflammation, and the aspect of the whole surface was the same as that of an old scarification.

"On the 15th of August, I observed some red spots at the point where the inoculation was made; and on the 30th of the month, twenty-five days after the inoculation, I discovered at the same part fourteen cutaneous tubercles, the greater portion of which had their origin in the wounds made by the scarificator. Nearly all of these tubercles were confluent. Only four, situated upon the outside, were isolated. Their base was broad; their size that of a lentil, and many of them that of a pea; they were hard to the touch; most of them were of a dirty red, and some of a dirty yellow color; their form was almost exactly round; upon some a slight desquamation was perceptible. Nothing morbid in the other regions of the body (no treatment).

"On the following day, there was a further augmentation of tubercles; these were all blended together; then represented a patch of the size of a thaler, knotty, projecting half a line above the level of the skin, and covered with grayish scales, which thickened and finally formed a large crust, common to all the tubercles. Upon deterging this surface with warm water, the crust was detached, and the tubercles then appeared under the form of slightly excoriated flattened elevations; but they were soon covered with new, thin, dry, and grayish scales.

"On the 27th of September, twenty-seven days after the appearance of the tubercles, and fifty-two days after the inoculation, a maculated syphilide appeared on the skin of the belly, of the breast, and of the back; the spots were, for the most part, united; some of them a little prominent and isolated, of the size of a grain of millet or of a lentil, oval and elongated; some were of a pale yellow, and others of a grayish-red color; they were unaccompanied by areola, itching or pain, were completely dry, and without crusts or scales. The next and the following days, the number of these spots increased prodigiously, and the whole body became covered with them; no febrile movement or symptom of catarrh was manifested. Early in October, some of these spots were changed into papules, others into tubercles, and the whole took on so characteristic a physiognomy that, without inquiring after the antecedents, any physician would, in a moment, recognize syphilis. As yet there was no sore-throat; but since this maculated, papular, and tubercular syphilide sufficiently proves the success of the inoculation, I can, even now, make the case public."

Let us first analyze the patient from whom the inoculated pus was taken. She had the CICATRIX OF A CHANCRE. But because one chancre was already cicatrized, does it follow that other chancres were prevented from pursuing their course, and from being inoculable? Were not the mucous patches of the labia majora and minora, as they are called, with their *croupy* exudation, still primitive ulcers, with their *diphtheritic, their specific layer?* Where is the differential diagnosis made by M. Waller? Is it sufficient that he should authoritatively tell us that they were mucous patches, when we know that he did not recognize the different forms which the primitive accident may assume, according to its seat, its duration, and the transformations it may undergo? With M. Waller, you know, chancre is always one and the same thing—perhaps even before, during, and after its existence. That which is not circumscribed by the descriptive formula which the paroquets of all ages and of all climates have repeated, and still repeat, is not chancre, and must be something else—even mucous patches on occasions of necessity! Am I exacting? Do you wish me, then, to accept the diagnosis of those who, as I stated in the preceding letter, invariably confound mucous papules themselves with the raspberry-vegetations erroneously designated condylomata? In view of so great an error, it is not difficult to suppose that these persons sometimes confound chancre with mucous papules; but, aside from possible error in the diagnosis of mucous tubercles, produced we know not how long a time subsequent to the chancre whose trace still remained, it may be asked what was the vaginal blennorrhagia of Némec? What was the state of the vagina, and of the neck of the uterus, at the time of the experiment? and, consequently, what was the nature of the vaginal secretion which polluted the ulcerated surfaces of the vulva, from which perhaps a foreign matter was about to be taken? About this, M. Waller, you, who are so precise, say nothing. In experiments of so important a character, and from which you are about hastily to infer the establishment of a truth which, up to the time of your experiments, you have believed to be misunderstood, you neglect to fulfil the commonest conditions; you do not tell us that you have examined this woman in the most careful manner, and that the speculum had left nothing doubtful at the bottom of the well! Believe me, these experiments must be repeated, for they lack the most elementary conditions. In spite of your good faith, which I in nowise call in question, I have no idea of the nature of the matter which you took from the genital organs of Némec.

There is one way by which you could have escaped this untoward result: namely, to take from the *croupy* exudation of the tonsils the matter to be inoculated. If it had been of the same nature as that of the genital organs, you would necessarily have succeeded. I advise you to make this experiment another time, and to give me the results at which you shall arrive. You know, like myself, that difference of seat is of no material consequence, and that, if secondary accidents on the genital organs are inoculable, those of the throat must likewise be so; for chancre of the buccal cavity is inoculable, like that of any other region of the body.

We come now to the child. You inoculate it by means of deep scarifications. At the end of four days, the wounds are healed; there is even no trace of inflammation. But what becomes of the wounded parts? how are they guarded from ulterior contaminations, which so easily and so commonly take place in a venereal hospital? Did you place them under watch-crystals, under your fine Bohemian glasses, as I do in Paris? Did you isolate them, or protect them in any way whatever? It seems that you did not; and you wish me to feel no doubt concerning the matter! Be it so; for eight days subsequently the evolution of primitive accidents commences, which, by their slowness, their course, and by their form, modified by the artificial conditions impressed upon the tissues in which they were seated, perfectly correspond to the *indurated, crustaceous, ecthymatous chancres* which occur upon the skin, and, like these, are regularly followed at the prescribed time (*forty-seven days* after the *first* manifestation of primitive accidents) by characteristic secondary accidents.

What do you say, my dear friend, of this observation, translated into syphilographic French? Does it not appear to you, aside from the slight errors of observation which I have pointed out in the primitive text, that the question concerns a very ordinary case of inoculation of primitive accidents, giving rise to the whole sequel of constitutional accidents, as occurred in the famous case of M. Boudeville? Does it lack any essential element? If so, tell me; and I will furnish the complement. I will tell you how the virulent pus acts when it is placed in the cellular tissue, the wounds above which, whose edges were not inoculated, may momentarily be closed; I will remind you how certain leech-bites, contaminated by chancres in their vicinity, comport themselves; I will explain to you further, as I have done in my annotations on the works of Hunter, in what way M. Babington deceived himself, and why he believed that chancre sometimes commences by induration, or, if you prefer, in the language of M. Waller, by *tubercles.*

I farther believe that the experimenter of Prague would have done well had he not cited this observation, which compromises his doctrine.

Second experiment—*with the blood of an individual affected with constitutional syphilis:—*

"Friedrich, a young boy aged fifteen years, No. 15,676, had been rachitic in his infancy, and had a lupus exfoliatus upon the right cheek and one under the chin;* this lupus, of the size of a thaler, was cured, excepting a small point upon the cheek, after long treatment by cauterizations and the use of the iodide

* The experimenter ought to have been firmly convinced of the failure of the inoculation in such a subject, in whom there was everything to be feared from a constitutional syphilis, in case of success.

of potassium. This child had never had syphilis, and in this respect was a fit subject for the inoculation, which was made, the 27th July, upon the right thigh. For this experiment, I took blood from a woman (Preund), in whom the secondary syphilis was developed under our own observation. This young girl, previously of fine appearance, had, of late, contracted primitive ulcerations five or six times, without, however, having had secondary syphilis. But, during the treatment of the last two chancres, which succeeded each other at an interval of fourteen days, she began to grow thin and pale, and when the last chancre was cured, and there only remained a catarrh of the uterus, tubercles formed upon the skin of the face, and spots appeared over the whole body.

"The inoculation was made in the following manner: The skin of the patient was scarified with a new scalpel, and by the aid of a cupping-glass three or four drachms of blood were drawn. In spite of the rapidity of this operation, the blood had, for the most part, coagulated before it could be carried from the chamber of the patient to the one where the inoculation was about to be made. The scarifications (made upon the child, as in the preceding experiment)* were perfectly cleansed from the clots of blood by washing them with a tampon soaked with warm water; afterwards, the blood to be inoculated was introduced into the wounds, partly by a small stick, and partly by means of charpie soaked with this liquid, and then fixed upon the scarified part. Neither inflammation nor suppuration supervened. At the end of three days, the wounds were completely closed. The patient continued well.

"On the 31st of August, thirty-four days subsequent to the inoculation, I perceived on the left thigh, where the inoculation had been made, two distinct tubercles, of the size of a pea, and of a pale reddish tint upon their surface, unaccompanied by itching or pain. On the day following, they began to increase, to unite at their base, and to become covered with scales; and a dark red areola surrounded both tubercles. The base of the

* We are not astonished that so inattentive an experimenter should fail to tell us how many scarifications were made, in order that we might know how many escaped contagion.

tubercles, that is to say, the subjacent skin and the subcutaneous cellular tissue, became firm, resisting (indurated), and upon the surface of the tubercles an ulceration was formed, which gave origin to a thin and brown crust. In this way there was formed, towards the 15th of September, an ulcer. The base of this ulcer was as large as a pigeon's egg, and its borders were surrounded by a coppery-red areola; and the ulcer was covered by the crust in question. Upon the removal of this crust, the bottom of the ulceration became visible; it was infundibuliform, and lardaceous, and bled easily at the edges. A few days since, there was thus formed upon the right shoulder *an isolated tubercle*, of the size of a pea, reddish, and sparsely covered with scales, *while the patient was ignorant of the precise day when this accident first appeared.* The general health is unchanged.

"On the 26th of September and the following days, Friedrich complained of want of appetite and restlessness; on the 1st of October, sixty-five days after the inoculation, and thirty-two days after the appearance of the first tubercles, an exanthema of the skin, of the forearm, of the back, of the breast, and of the thighs, supervened; an exanthema which we recognized to be one of the best characterized of syphilitic roseolas. The spots were exactly similar to those described above (in the first experiment), with the only difference that, in certain points, they were a little more elevated. The ulceration of the thigh had acquired the size of a thaler, while it preserved its infundibuliform aspect, its lardaceous bottom, and its coppery border.

"During the subsequent days, the eruption of the spots became so abundant that the whole body, not excepting the face, was covered with it, and appeared spotted like a tiger. There was no itching, no pain, no symptom of catarrh or fever.

"On the 6th of October, several spots, especially upon the internal part of the thighs and upon the abdomen, were elevated into papules and tubercles; and from that time the diagnosis of the syphilide, even without a knowledge of the antecedents, became as easy as in the preceding case."

In this experiment, the blood which was used appears to have been taken from a woman affected with constitutional syphilis. But was it really the blood of this woman which infected with syphilis the unfortunate child subjected to the experiment? A scrofulous child, affected with a lupus, with such a skin as you are aware these patients possess; living, according to the above statement, among venereal patients, with no precautions taken in relation to it; with no guarantee whatever that the cicatrices, which are so apt to be irritated and excoriated in such patients, and afterwards to furnish an easy entrance to the contagious matter, in almost constant *circulation* in venereal hospitals, were duly protected! Thus, inasmuch as all the accidents subsequently developed cannot be attributed to the patient who furnished the blood, the *two tubercles* developed only thirty-four days after the experiment, are, in my opinion, *due to another method of contagion*, from which this little patient was not protected! For, whilst the evolution of the chancres *with an indurated base* occurred upon the thigh in the most regular manner (only in slightly gigantic proportions, since the base of the chancre attained the size of a *pigeon's egg*, which probably depended upon the concomitant pathological state of the little patient), another tubercle, of the same form, of a more common size, is seen upon the right shoulder, of which *we neither know the cause, nor the time of its first appearance*, and which is probably not the direct result of the inoculation, unless a blade of the scarificator may have gone astray. But what produced this tubercle of the shoulder? Whence comes it? No matter; its explanation is not attempted; it is sufficient to explain the development of the tubercles of the thigh, by the fact of the inoculation of the blood, without going farther. Yet this tubercle of the shoulder is not an accident consecutive to the first *secondary accidents of the inoculation*, for it appeared simultaneously with them; whilst the true secondary manifestations, which were perfectly regular, only appeared *thirty-two days subsequent to the appearance of the primitive accidents.*

These last accidents were verified by numerous and honorable *confrères*, whose knowledge I in nowise call in question. These gentlemen stated what they saw and recognized perfectly well. But, notwithstanding their number and the authority of their names, before which I am ready to bow, I should, had they offered their united testimony to certify that the infection could have taken place only according to the theory of M. Waller, be convinced that M. Waller was not alone deceived.

But M. Waller is unfortunate. I believed that Wallace was dead; I had even the boldness to add a few words to his funeral oration. It seems that I have been deceived.

Be this as it may, had I commenced to read the Bohemian work at the end, instead of at the beginning, I might perhaps have dispensed with my remarks on this last and astonishing observation; for the violent attack of its author upon my friend Diday, of Lyons, would have led me to think that he did not believe in the possibility of inoculating constitutional syphilis, unless he restricted his belief in inoculation to secondary accidents, and held that tertiary blood is no longer virulent, notwithstanding the influence of this period upon the hereditary diathesis; the relations of which influence are brought forward by M. Waller when necessary. M. Waller is right in affirming that my friend Diday produced no result from tertiary inoculations; but M. Diday can, in turn, say to M. Waller that the latter has done no more than himself, in this connection, with secondary blood; for, if he cannot be exonerated from the charge of communicating syphilis to the patient of his first experiment, he ought to receive the most complete absolution for that of the second.

I will make a proposition to those who propagate among us the opinions of M. Waller: Let them present the facts which I have just cited to the *Société Anatomique* and to the *Société Médicale d'Observation.*

But they dare not do it!

In view of what I have stated, my dear friend, you will permit me to say to you that I have not made a single step towards acquiring a knowledge of the German language; and that I shall understand the new propositions of M. Waller, and his conclusions in regard to *sanitary police* and *legal medicine*, only when he shall give us observations which I cannot translate by simple common sense without the help of German, as I have been enabled to translate those he has just given us with so much pretension.

It is for you, and particularly for your numerous and impartial readers, to decide whether I have gained my battle of Prague.

Yours, RICORD.

LETTER XXXI.

MY DEAR FRIEND: Previous to our excursion to Prague, we were studying the manifestations of constitutional syphilis.

I told you that, when no treatment had been directed against the chancre, these manifestations occurred within a given time, and pursued a certain order which allowed of their classification.

In fact—notwithstanding the efforts of those who wish to envelop everything in obscurity—from the moment that the constitutional infection takes place as a result of the primitive accident, the patient has acquired what Hunter, with reason, designated the syphilitic disposition—that is to say, the syphilitic *diathesis;* and from that moment the accidents show themselves more or less quickly, and progress with more or less rapidity, in different situations and upon different tissues.

In the first place, in what may be considered as a period of incubation, up to a certain point, the first effects we frequently observe, are more or less marked disturbance of hematosis and of innervation.

Prior to any other manifestation, I have been able, in a great number of analyses of the blood, made with the utmost care by M. Grassi, and mentioned in the inaugural thesis of my pupil and friend M. McCarthy, to verify the diminution of the blood-globules, the chloro-anæmia, which is about to accompany the secondary accidents properly so called, and which is often very marked.

At this period, too, and frequently before the appearance of any other symptom, and as a first consequence of the disease, vision becomes affected, and the muscular strength weakened; neuralgic pains in the head, and rheumatic pains of the members, also supervene. These *precocious secondary pains,* which may also show themselves at the same time that secondary accidents become manifest, and which may return alone, or accompanied by the latter, are not found at any other period, unless they be systematically confounded with another order of pains.

It is no part of my plan to write a detailed history of these varieties of prodromic neuroses or of the secondary period of syphilis—neuroses which are not invariably observed, which are often even wanting, but which possess the common characters which are of sufficient importance to deserve mention.

These neuroses consist of intermittent *nocturnal* pains, which are especially manifested under the influence of heat, especially that of the bed. Consequently, in patients who turn night into day, and *vice versa*, this species of attack is inverted. The pains of this period do not regularly return every time in the same seat, and, during the intermission, pressure does not occasion them. Indeed, some patients frequently experience relief at the moment of the greatest suffering, not only by exposing the painful parts to the action of cold, but also by compressing them. Motion of the members in which the rheumatoidal pains are seated relieves rather than increases these pains, which the patients only complain of in the neighborhood of the articulations, and at times in the dorso-lumbar region. In these cases, there exists no change in the color of the skin, no change of temperature, no tumefaction. In some circumstances, the feelings are simply those of lassitude, which ordinarily cease when other symptoms, such as the cutaneous eruptions, manifest themselves.

At this period especially of the precocious accidents, one of the most constant manifestations is adenopathies—adenopathies which may be designated, with the utmost precision, secondary buboes.

The affection of the lymphatic ganglia, at the secondary period, merits particular attention. It is, in some measure, characteristic of this period.

This variety of adenopathy is seldom wanting, when we know how to seek it, and it often constitutes one of the first proofs of the infection. Sometimes it is present from the third week, but oftener after the sixth, and thus succeeds to the indolent multiple adenopathy which is *invariably symptomatic* of indurated chancre.

The seat which it prefers is the posterior cervical or cervico-cephalic region. It is much more rarely found elsewhere. Nevertheless, I have seen other ganglia tumefied in a small number of subjects; but then it is necessary to be careful not to be deceived by other causes of ganglionary engorgement, and especially by primitive accidents at unusual seats, or by the strumous dispositions which everywhere favor engorgements of the lymphatic glands, but certainly less in the posterior cervical region than anywhere else.

The true secondary adenopathies never attain a very large size; they are indolent, and usually multiple; they never suppurate, or at least they *never suppurate specifically;* they never furnish inoculable pus.

Undoubtedly, as most observers have verified, this variety of adenopathy is only observed when the skin is already the seat of an eruption, which is usually superficial; but I can affirm that I have found the engorgement of the posterior cervical, of the occipital, of the mastoidean ganglia, in patients who did not present the least trace of an eruption on the scalp. My colleague at the *Hôpital du Midi*, M. Puche, says he has observed the same circumstance. It is certain that, if this variety of adenopathy is connected with certain forms of secondary accidents with which alone it is found, it is not always produced by these secondary accidents in the same regions; inasmuch as the indurated chancre produces its own ganglionary satellites, which, aside from this *inevitable solidarity*, are very analogous, and even identical, in other respects. At all events, if these two varieties of syphilitic adenopathy may sometimes be confounded, they can always be distinguished from that variety which is produced by the non-indurated and *non-infecting* chancre, the variety which suppurates, and *which furnishes inoculable pus.*

These secondary adenopathies are not to be found after a certain period. You will not see them produced, *for the first time*, at the late secondary period, and far less at the tertiary period of syphilis. If the late accidents are accompanied by diseased ganglia, seek and you will find sufficient reasons for their existence; otherwise the patients will tell you that these engorgements are the result of the first accidents.

At the beginning of constitutional syphilis, at the moment of its first explosion, an accident is sufficiently often met with which observers, who simply collect their observations from books, have considered as a proof of an old, grave, and inveterate affection: I speak of alopecia, one of the most precocious symptoms of constitutional syphilis. This is the first symptom developed in some patients, and, at an advanced period of the disease, it no longer presents the *same characters as at first*, unless calvities, and other causes of the fall of the hair, be confounded with it.

If we now pass on to the affection of the skin, and the mucous membranes and their dependencies, we find (even according to the avowal of those who deny the existence of marked phases in the pox), that the nearer we approach the moment of contagion, the more do we observe the forms of the affection to be superficial and disseminated, or more or less confluent. You know, my dear friend, that some physicians have made of these forms secondary accidents *d'emblée*, or *secondary primitive*, or primitive secondary accidents; but the idea has never suggested itself to their minds to regard profound tubercles, gummy tumors, affections of the periosteum and of the bones, in the same light. Had they regarded these affections in this light, I should not have been much astonished, since they have advanced so far in the path which leads to this assumption.

Follow, my dear friend, the stages of the syphilitic evolution —which, unfortunately, in our day, may so easily be done—and you will see with what regularity and constancy are exhibited, within the prescribed time I have already mentioned, the exanthematous eruptions, of a rubeolic or erythematous form. This constancy is such that some observers, and I will still cite my friends, MM. Puche and Cullerier the son, think that these eruptions are never wanting. It is certain that they almost always become manifest, provided we know how to seek them in time, and do not allow them to escape our notice, for nothing else than the sight reveals their existence.

But these first eruptions, which are succeeded more or less quickly by papules, by more or less prominent patches, by the dry forms of squamæ, by vesicles, by vesico-pustules, and by more or less superficial pustules of the suppurative forms, do not present the same characters, in every stage of syphilis, as may be seen when we know how to refer them to their true source, to their real starting-point—to the infecting chancre, or to hereditary diathesis.

With respect to the mucous membranes and the regions of the skin contiguous to them, and easily susceptible of being transformed, we observe the same phenomena. We first perceive simple alterations of color; but here, on account of the structure of the parts involved, their particular seat and functions, and the papular state, the patches are sooner defined, and more quickly produce papules or mucous patches, on which so many debatable hypotheses have been built! But these accidents, so little understood, and the particular physiognomy of which is due to accessory circumstances, as I have just stated—such as texture, seat, and functions—do not show themselves at all stages of syphilis any more than roseola.

If you will take the trouble to make the differential diagnosis, and if, through a deplorable confusion of language, you do not confound *tubercular syphilides* with more or less prominent, and more or less *tuberculiform* papules or mucous patches, you will not find these accidents as the first manifestations of a syphilis which had been contracted ten or twenty years before, and had not been treated.

But, in proportion as the syphilis grows old; in proportion as it traverses its orbit, the accidents which it produces, and which tend to become more and more grave, and more and more profound, seem, by a sort of compensation, also to become less numerous, more *discreet*, if we may be allowed to employ this word in this connection. It is the thickness of the skin which is affected, as well as the cellular tissue which lines the skin; and certain portions of the latter are, as it were, selected by preference. Other things being equal, the affection is observed where the cellular tissue is most dense. Thus, in the mouth, it is the thickness of the mucous membrane and the submucous cellular tissue which are invaded; and whilst the precocious secondary accidents occupy the internal and superficial surface of the lips, of the cheeks, of the borders of the tongue, or of the tonsils, the tardy accidents burrow more deeply in the tongue itself, in the palatine region, or affect the velum-palati, or develop themselves behind the posterior pillars, in the pharynx, where they produce grave alterations and frightful ulcers.

All the phenomena I have described, my dear friend, aside from some rare cases of galloping poxes, which you will permit me to call poxes of the *Renaissance*, and which, like many inconvenient and worm-eaten pieces of furniture of that epoch, are fortunately gradually disappearing; all these phenomena, I say, show themselves only a long time subsequent to contagion. This fact, you may be sure, is well known to those dermatologists who have done so much to elucidate the nature of syphilides, and to whom no one is more inclined than myself to render justice. Yet this fact is none the less denied when it comes in contact with the *system of confusion*. To recognize the truth of the statements I advance, we need a diagnosis somewhat more precise than that to which a certain opponent restricts himself. At one time, all syphilides were bullæ; at present we are incrusted in a mystic ecthyma, which our *confrère*, M. Baude, thinks he is familiar with!

But if a certain time must elapse before we witness the manifestations of which I have just spoken, according to the opinion of every observer since the epidemic of the fifteenth century, a much longer time must elapse before the disease reaches the testicles, the fibrous system, the osseous tissue, the muscles, and other deep organs—the heart, the brain, the lungs, the liver, &c. Examine the history of the patients minutely; take your start from the earliest symptom of the disease; do not let go of the end of the ribbon which I mentioned in a preceding letter, and you will find that it is very rarely before the first six months, and often not until a much longer period has elapsed, that these accidents, preceded of necessity by the manifestations of which I have already spoken, show themselves.

When the periosteum and the bones become affected, pains precede or accompany the disease. These true osteoscopic pains, so easily confounded by inattentive observers with those of the second period, and misleading those who are so much inclined to fall into error, are as distinct from the latter as can possibly be imagined. Their seat is upon the superficial bones, and in the compact regions; they are fixed, and have not the *rheumatoidal* character; they are nocturnal, and are aggravated by heat, particularly that occasioned by the bed; they are always increased by pressure, either during the paroxysm, or during the intermission, or during the diurnal remission. Finally, where the pain is seated, a swelling of the periosteum or of the bone may, and commonly does, supervene.

These facts, my dear friend, are based on observation; they are not copied from books, nor are they the fruit of the imagination; for, thank God, if I have known how to study the pox, I did not originate the disease—a circumstance which, in a social point of view, I should have had much reason to regret.

Therefore, from twenty years' observation, based on an examination of hundreds of patients, whom a large number of physicians attending my cliniques have seen with me, I have deduced the conclusion that, if syphilis, left to itself, tends to produce more or less frequent manifestations, which are observed for a period of greater or less duration, these manifestations are made at a certain period, and in certain determined seats, whence result certain forms and lesions, which constitute, in some measure, so many distinct diseases, united among themselves by virtue of their common source, and often succeeding one another by gradual transitions, though sometimes by clearly marked leaps.

We can then admit, with Thiery de Hery, Hunter, and others, three well-characterized periods:—

1. The primitive accident, the chancre. This is the immediate result of the contagion, and the invariable source of the reproductive virus. It remains as a local accident, upon the skin or mucous membranes, within certain limits; and is able to extend only to the neighboring ganglia, and to give origin to buboes. Finally, it infects the economy.

2. The secondary accidents, or the constitutional poisoning which results from this infection, and is first exhibited during the first six months. The seat of this poisoning is the skin, the

mucous membranes, and their connections. These accidents are, in default of rigorous demonstration, supposed contagious. They have not yet been reproduced by artificial inoculation; and are transmissible hereditarily, by the father or by the mother singly, or by both at the same time.

3. The tertiary accidents, rarely showing themselves prior to the sixth month. Their seat is the subcutaneous cellular tissue or the submucous, the fibrous, the osseous, and the muscular tissues, and certain organs, such as the testicles, the heart, the brain, the lungs, the liver, &c.

Not only are none of the morbid secretions of these accidents contagious by ordinary contact, not only are none of them inoculable, but their *specific* influence upon hereditary predisposition seems to decrease constantly, so as to become subsequently only one of the hereditary causes of scrofula.

These periods, with due submission to those who have a horror of the precision as well as of the phraseology which medicine borrows from the exact sciences, may easily be verified; and the only derangement in this perfect order is due to that which therapeutics impart to it, so that it may be said here, as I will hereafter prove to you:—

A fine disorder is frequently the effect of art.

Yours, RICORD.

LETTER XXXII.

MY DEAR FRIEND: "Order is the will of God," said one of the finest women of the seventeenth century. It seems to me that Madame de Sévigné would acknowledge that I have conformed to the supreme will, and that she would appreciate the order which I have re-established—those who make greater pretensions might say created—in this disease, which many syphilographists have treated worse than poor pox treats humanity.

I have told you how orderly, how regular, how symmetrical is the pox, in its free and normal development; that its course is regular, and its steps counted and measured; I have shown you with what art, agreeable to time and place, it can remove the hair, grow pale, or cover itself with its copper-colored paint. Finally, I have shown it to you as superficial, light, and diffused at its commencement; and augmented in gravity at its later stages. Well! all these phenomena, like the existence of the affected person, are subject to perturbations which are not always inherent in the nature of the disease; but, on the contrary, are most usually the result of accidental causes, and are more especially the result of treatment.

Syphilis is, undoubtedly, one of those maladies against which art has most power. Many credulous and inexperienced physicians even believe, with the common people, that medicine must always be all-powerful, and that where the disease has proved refractory, or has increased or reappeared, despite treatment, the physician, and not the remedies, must bear the blame. You have seen one of our *confrères* affirm, with wonderful assurance, some time since, in a medical journal, that not a single pox could resist one hundred and ten pills of Dupuytren!

I do not wish to give you a treatise on antisyphilitic therapeutics, and I cannot though I would. I only desire, as I have done in relation to the other questions upon which I have touched in these letters, to speak of the treatment in the most general manner, so far as it relates to the doctrines which I inculcate.

Constitutional syphilis is certainly one of the great calamities to which mankind is exposed. Happily, in spite of its frequency, it is still relatively rare, for it does not affect all who are exposed to it. "He who wills does not have the pox," as was remarked by one of our old masters, the aged Professor Dubois. We have observed this *inaptitude* in certain idiosyncrasies; and does not experience, which has taught me that, as a general rule, a person does not have constitutional syphilis, that he is not *apt* to contract indurated chancres twice, followed each time by the syphilitic evolution which is now so well understood, permit us to believe (since the pox is hereditary) that, in some cases, the disposition acquired by the parents, and by which they are protected, may be transmitted to the children?

It is in accordance with these ideas, which I have taught and still inculcate, and the correctness of which is daily verified, that the economy is supposed to be impressed with a general disposition equivalent to that ordinarily imparted to it by the vaccinia or a variola. This predisposition not only prevents the variolic virus from acting locally, but especially counteracts infection and its consecutive effects.

In researches of this character, and in attempts made to arrive at this desirable result, a certain reserve and great prudence are nevertheless essential. We must guard against eccentricities; and in view of the good we seek to obtain, we must not neglect to take into account the evil which may be produced.

It would certainly not become me to find fault with experimental researches, after having so often invoked them in support of my doctrines, and acknowledged my indebtedness for the brilliant light which they have shed on so many obscure questions which could not have been elucidated without their assistance. No; I leave this to those who have stigmatized and reviled that which is most exact in science; and who, after having calumniated experiment, now demand of it results which we have not only not the right to expect from it, but which duty commands us to declare that it is wholly incompetent to furnish.

Educated in our school, and persuaded like ourselves that he had no right to compromise the health of any subject, by communicating so grave a disease as syphilis to a healthy individual, my learned *confrère* and friend, M. Diday, of Lyons, in searching for a means of prophylaxis against constitutional syphilis in the syphilitic virus itself, has experimented only upon individuals already diseased, but in various conditions.

He started from these principles, which I inculcate, and which I again present to your notice:—

1. Chancre is at first a local accident.

2. The constitutional infection only takes place subsequent to its development.

3. When the syphilitic diathesis already exists, a new chancre remains definitively local.

4. A person may, in given conditions, be under the influence of a syphilitic diathesis, or have acquired immunity against a new syphilis, without necessarily exhibiting syphilitic accidents.

5. Finally, syphilis is transmitted from parents to children, from the mother to the fœtus, by the circulation; but the older it becomes, the more it approaches its last tertiary phase, and the less it tends to be reproduced, by generation, with the traits of its other periods; then, perhaps, it otherwise modifies the constitution of children.

Therefore, to modify the general state, before an existing chancre should have time to infect the economy, and to obtain this result with the syphilitic virus itself, introduced directly into the blood, but enfeebled, and approaching that stage when it could no longer produce merely a general *disposition* without syphilitic manifestations; such, I say, was the laudable object of the learned surgeon of Lyons. In order to obtain this result, M. Diday took the blood of an individual affected with a tertiary syphilis, and presenting an exostosis as the characteristic symptom of this period. This blood was used to inoculate patients who actually had *non-indurated* chancres; and these patients, who were subjected to no antisyphilitic treatment, and in whom no *direct* result of the inoculation was observed, presented no constitutional accident after the requisite time, which I have elsewhere determined, had elapsed. Only one case, in which the chancre was indurated at the time of the inoculation of the *tertiary blood*, presented the classic and regular march of the syphilitic evolution.

You know, my dear friend, that when M. Diday's experiments were made known at Paris, they were subjected to violent criticism. He was especially blamed for stating that tertiary accidents might be allowed to persist, momentarily without doubt, in order to prevent the subsequent development of constitutional accidents in individuals affected with primitive accidents. Some would willingly have brought M. Diday to the bar of the *Conseil des Hôpitaux* of Lyons, although he merely transmitted the disease *from one diseased individual to another*. It was these inoffensive attempts of M. Diday, you know, which became the innocent cause of attacks against myself, and was the origin of my letters, which you have so kindly received. I do not know whether I ought to thank my friend of Lyons for this circumstance; you will hereafter be the judge.

Be this as it may, I must combat the views of M. Diday, for the two following reasons:—

1. The local effect of the inoculation of *tertiary blood* being null, you cannot tell that it has acted.

2. The absence of constitutional accidents in the individual inoculated proves nothing at all; for the chancre, in the conditions in which you experimented, is not followed by general accidents, in cases which I do not treat at all.

Brought up in the seraglio, M. Diday well knew my opinion in this matter; consequently, he sought to render his *non-indurated* chancres as infectious as possible, by basing his conclusions on authorities opposed to me, which furnished him with the statistics with which you are acquainted, and which he is too sincere to receive as true. But he deserves none the less credit for his work. In his memoir "upon a process of vaccination preventive of constitutional syphilis," the ex-surgeon of Antiquaille has given, as he always gives, proof of profound knowledge; and he deserves to be read with attention.

But M. Diday merely had presumptions against constitutional accidents; he remains convinced, up to the present time, that nothing disproves the contagion or the inoculation of the primitive accident.

M. Auzias-Turenne has gone farther; he thinks that individuals may be rendered refractory to the direct and immediate action of the virulent pus, and may resist the contagion of chancre. He has arrived at this belief from his inoculations on animals. He states that he has observed that, in making successive inoculations, the accident gradually became less and less intense, and of shorter duration, and that, finally, it could no longer be inoculated. M. Auzias-Turenne explained this result by some modification impressed upon the economy; by a sort of infiltration of the syphilitic virus, producing what has been called syphilism or syphilization, bearing to the pox the relation which vaccinia bears to variola; that is to say, the physician who seeks to hinder or to prevent the development of new primitive accidents, has not even the chance of determining the syphilitic diathesis such as we understand it, and the possibility of witnessing the development of constitutional accidents is thus denied. What say you to that, my dear friend? You do not dare to respond, even on behalf of the monkeys, which nevertheless seem to assume a certain nosological importance! But the experimenter whom I have just cited, seeking naturally to apply this law to the human species, thinks he verified the fact that certain persons had become refractory to chancre after having been subjected, a given number of times, to infectious contact. How many of these cases did he count? how many of these contagions are necessary to induce immunity? So far as I am aware, he has failed to answer. His cases, I believe, were taken from among public women, for a long time given up to debauchery, and who had chancres less frequently than beginners. You know very well that all those who expose themselves to the syphilitic disease do not contract it; or, so to speak, they are not *caught*. You know that something else than the *physiology*, propounded by one of our *confrères*, is necessary to produce contagion; and this something consists in the conditions of tissue which are seldom met with in proportion as the parts have been long in use; in proportion as they are roomy, well tanned and lined, like the hands of a workman, with a thick and resisting epidermis; and, finally, if my physiologist wishes, which are seldom met with in those who are *blasées*, and incapable of excitation, of orgasm, of emotion, and of that virulent temperature demanded by M. Cazenave.

Too often, alas! have I seen, and others like myself have seen, patients in whom chancres were developed many times, at various periods; in whom the last infection was not less grave than the first; in whom numerous non-indurated chancres did not prevent the last chancre from becoming indurated and infecting the economy; and in whom this infection did not prevent the contraction of a new chancre which failed to indurate, and which frequently became more intense than all the chancres which preceded it.

I have seen chancres, and cases of this kind may always be seen at the *Hôpital du Midi*, unceasingly extend, step by step, by the progress of the phagedænism, by true successive inoculations—particularly in the case of the serpiginous chancre; run through and furrow surfaces to a frightful extent; amputate the penis; hollow out the inguinal fold; cut and plough up the skin of the abdomen from one iliac region to the other; descend to the thighs; and, if I dared so to say, *unpantaloon* the patient. Well! these chancres, in order to make this progress, in order to attain these bounds (which are not even the last they may attain), have often required the lapse of months and years, though, at the close of this period, furnishing inoculable pus with results as grave as at the commencement. And yet in these cases the number of the accidental and successive ulcerations, their surface and duration, are, it seems to me, equivalent to what is observed in the inoculations designated preventive, which are repeated at short intervals, and in *the same region*. It is true that here nature or the disease produces this result without *a preventive intention*, which establishes a difference as to intentional art. Animal magnetism, if you are a believer in the doctrine, may perhaps give you the explanation of this mystery.

But what can now be said in comparison with what has just reached us from Turin? Bohemia is excelled; and the name of M. Waller must pale before that of M. Spérino, the boldest and most fortunate of experimenters. Since I saw the balloons of Paris, and have been familiar with all that MM. Poitevin and Godard transported to the clouds, I have become more credulous, and am no longer astonished at anything, unless it be at the fact that three or four inoculations were made once or twice a week, for two months, on the bellies of fifty public women (which gives us a total of twenty-four, and in some forty-eight and sixty-four inoculations); that there was no question of phagedænism; that no circumstance occurred to render the experiments questionable; that in no instance could a chancre become indurated before another inoculation prevented this result, though it is well known how rapidly chancre infects and becomes indurated, and that it is not assumed to be able to infect prior to this manifestation; and, finally, that M. Spérino tells us it was not until the figures above indicated were attained that he could no longer inoculate! Yes, I am astonished; and I await the report of the commission which, I trust, will give us all the details which are not supplied by the facts of M. Spérino. I await especially the presentation of a *syphilized* and refractory individual, who may come before the *cliniciens* of the *Hôpital du Midi*, or before the National Academy of Medicine, to defy me, in the lists, with the arms of my choice.

In the mean while, the conclusion which results from the analysis which I have made of the published observations made at Paris and Italy, is, that the pus which comes from *non-indurated chancres* has always been inoculated to produce analogous accidents; and that in the only instance in which the pus obtained from a primitive accident which had produced a constitutional syphilis, was used, at Paris, to inoculate a patient who was healthy, the individual was affected with an *indurated chancre* and a *general poisoning*. Were it always thus, as I have already stated, it would be necessary to come to this conclusion: That differences may exist in the disease which do not depend upon the conditions of the affected individual alone, but also upon differences in the causes of the disease.

Be this as it may, what, in view of all the circumstances with which you are familiar, would you think of a method, which, to prevent your contracting a chancre of which you do not necessarily run the risk, as in the case of variola, requires that you should be first inoculated with it from twenty-four to sixty-four times, and that, too, without your knowing how long this dearly-bought immunity is likely to last?

However, with respect to such grave questions, studied by men of respectability, it is necessary to be calm and unprejudiced. Doctrines and systems ought to be presented with wise moderation, so as not to come in contact with new facts; though they should embrace nothing which is not rigorously demonstrated. This incontestable demonstration, then, is what I require; and, as an inducement for M. Spérino to give it to me, let him recollect that Turin was the country of Lagrange, one of the most illustrious representatives of the exact sciences, and that, as his compatriot, he should render me mathematical precision; else I shall say to him, "*Si non è vero*, non *è ben trovato*."

Yours, RICORD.

P. S.—My colleague, M. Puche, has just performed seven successive inoculations; the last as active as the first!

LETTER XXXIII.

My dear Friend: You had the kindness to communicate to me a letter addressed to you by M. Auzias-Turenne, relative to what I said in my last letter upon *syphilization* and *syphilism*. You have expressed the desire, if I had any reply to make to the letter of M. Auzias, that it should appear at the same time as the letter itself. Your motives are proper, and will be understood, without other explanation, by every candid reader. You believe in progress, and receive it, without repugnance, even in its boldest manifestations. But I congratulate you on the fact that you neither surrender your right of examining what is presented to your notice, nor of holding, with a wise and prudent reserve, your opinions in abeyance. When a question so important as that we are about to consider comes before us, it is dangerous not to attack it directly; it is puerile to expect to stifle it by a disdainful silence.

Let us then examine the new doctrines which M. Auzias seeks to propagate; but first let us yield him the floor, so that he may explain his new views:—

"To the Chief Editor of the Union Medicale:

The poison furnished by chancres produced, when inoculated upon the arm by means of a lancet, two venereal ulcers. The experiment was followed by the *cure* of a soldier who was the prey of an old syphilis which proved rebellious to all treatment.—Petit-Radel.

Mr. Editor: There are correct ideas, as there are good men. They improve upon acquaintance. Now, M. Ricord has, in your columns of the 12th of August, thrown a false air around *syphilization;* involuntarily, without doubt. Permit me, then, simply to make the subject understood by your readers.

Syphilization is neither a virus nor a disease—such, for example, as vaccinia and variola. It is a state analogous to that in which we consider one who is affected with the smallpox. In fact, after having had the variola, we have acquired immunity from the disease. In the same way, after having experienced successively a sufficient number of chancres, we are *syphilized;* that is to say, insured against all the forms of syphilis. *Syphilism* is the aptitude to be *syphilized.* Undoubtedly, we possess this in different degrees. Therefore, it is a natural quality; while *syphilization* is a property acquired by virtue of this quality. Finally, we accept without hesitation the qualifying term *syphilizer*, suggested by M. Diday; in the same way as we formerly spoke of *circulators*, inoculators. This analogy is not without force.

But what are we to say about the words *saturation*, *impregnation*, and *infiltration*, when taken literally? We do not wish to be *saturated*, *impregnated*, or *infiltrated* with the syphilitic virus, any more than with that of the smallpox; we do not wish, in a word, to be the focus of infection and corruption itself! What we maintain is that, when we are *syphilized*, we have experienced, in a short time, the syphilitic disease, and are not liable to it any farther than to the smallpox with which we have been affected. We would accept any other rational explanation of *syphilization;* but we energetically reject a theory which would prove to every one a source of prejudice.

In order to make *syphilization* understood, let us suppose that a traveller passes over the two sides of a mountain, first from the base to the summit, afterwards from the summit to the base. He represents the person whom we *syphilize.* The chancres correspond to the different portions of his route; thus, the indurated chancre, the index of constitutional syphilis, corresponds to the crest of the mountain, and syphilization to the end of the journey. By his first chancres this traveller approaches constitutional syphilis. He then goes on until, by means of other chancres, he is brought to *syphilization.* In order, then, to extricate himself from the constitutional syphilis, he must not pause in the middle of his route.

Every one, prior to being *syphilized*, is susceptible to constitutional syphilis; but it is avoided by the majority of those who have chancres, either because they do not reach, or because they go beyond the disease. Constitutional syphilis can undoubtedly be given to any one who has not had the affection, just as every one may be preserved from it.

It is easily understood, from what I have just said, that it is impossible to attain the state of *syphilization* without passing through that of constitutional syphilis. The essential point is so to hasten its development by inoculations that it may not have time to injure our organs. Indurated chancre, then, is nothing but the index of a pause at this period, which, though really inevitable, may be rendered as short as is desirable. We consequently say, with due submission to Dubois and M. Ricord: "*He who wishes to have, can have the pox.*" But we add: *Non bis in idem.* There is perhaps an exception in the cases of those whose parents had the pox, and who, on that account, may, from hereditary predisposition, be refractory to it. A certain degree of *syphilization* in the parents would be a source of immunity to the children.

Thus I am led, by facts and by reasoning, to admit the existence of but one virus, which produces, according to its specific condition, or according to the state of the organism, sometimes a simple, and sometimes an indurated chancre. Should M. Ricord, as he gives us reason to suspect, cease firmly to hold on high the flag which Hunter committed to him, and on which is inscribed, *unity of virus,* I would seize its staff boldly, so much am I convinced that within its folds is the truth to be found. Yes, there is only one syphilitic virus; and this unique virus is not protean. But it reacts differently, according as the organism is influenced by such or such a reagent, or perhaps as this virus itself varies in regard to the degree of its concentration. I fear that some may misunderstand it, as the old chemists misunderstood a simple body, in its various combinations!

Be no longer surprised that M. Ricord has seen simple chancres precede and follow indurated chancres upon the same person; but be surprised that he should suspect, in order to explain these differences, the existence of more than one virulent cause. A single virus with graduated forms, and an organism variously modified by these forms, easily furnish the key to these apparent contradictions.

Moreover, there is no necessity to assume a particular virus in order to account for phagedænism. To account for a notable diminution of *syphilism*, under the influence of which diminution phagedænism manifests itself, it is sufficient to assume the intervention either of the scorbutic, the herpetic, or the cancerous taint, or the abuse of alcoholic drinks or mercury, or, finally, an inflammation, or some equally operative cause. Theory is here in accordance with practice, to indicate the means of combating these *antisyphilizing* tendencies, or to teach us how time may be allowed to dissipate them. Do not, however, misunderstand me; for, in spite of the astonishment of M. Ricord, phagedænism is not to be feared in cases where we intentionally *syphilize*, and know how to manage the virus.

We may now understand that syphilization does not cast aside, but explains those chancres which surpass in virulence those which have preceded them, and which are presented by way of objection to my theory. Does not every one recognize, in these chancres, the influence of the modifications which the organism has experienced in the interval between their development, or the intervention of a virus of less strength than the one whose influence was first manifested?

Is it possible to estimate the number of chancres required to produce *syphilization?* No. Because, in the solution of this problem, it would be necessary to take too many unknown quantities into account. This number must undoubtedly vary according to the seat of the chancres, and according to their duration, their size, and especially their mode of succession; according to the integrity, or the prior syphilitic contamination, of the individual; according to the idiosyncrasy, or, to use more correct language, the absolute syphilism of the individual; according to the intervention of mercury, of alcoholic drinks, of various organic excitants, &c. Thus, for example:—

1. Successive chancres syphilize to a greater degree, provided their number is equal, than those which appear simultaneously. But it would require too long a time to obtain complete *syphilization* exclusively by successive chancres. For this reason, I recommend more frequent and multiplied inoculations towards the close of the affection, for at this stage there is no longer risk of inflammation. Parodying the old adage, it may be said: *Il n'y a que les premiers chancres qui coûtent.*

2. Other things being equal, fewer chancres are requisite to

syphilize an individual who has a constitutional syphilis than to syphilize another individual. But it must not be forgotten that constitutional syphilis tends to impair our organs, or, in other terms, that the *syphilitic diathesis* may engender a *syphilitic cachexia!* Now this cachexia may, in turn, be a cause of phagedænism, that is to say, of extreme diminution of *syphilism*, particularly when the latter occurs after the intervention of a prolonged or recent mercurial treatment.

3. Mercury favors the progress of chancre. It is therefore desirable that persons who are *syphilized* should be withdrawn from its influence. But, since its action is transitory, while *syphilization*, even when incomplete, is persistent, the inoculations may be resumed after an interruption occasioned by the presence of mercury in the economy.

4. Alcoholic drinks, fatigue, excesses of all kinds, internal inflammations, taints, impoverishment of the blood, &c., are so many lashes to phagedænism or to ganglionary engorgement. Is there any necessity of insisting upon the importance of counteracting these influences?

In view of so many causes which may act together or separately, we are less able to determine the number of chancres necessary to produce *syphilization* than to state, for example, how much opium is necessary to produce sleep, or how much wine is required to produce drunkenness.

But we can, without fear of error, diminish, by at least three-fourths, the number mentioned by M. Ricord, in relation to which the question is not explicitly raised in the memoir of M. Spérino. And then why omit such passages in the memoir as the following: "*In the women who had old and large ulcers, the first artificial ulcerations were small, and it was no longer possible to produce new ones after a few inoculations.*" The maximum of M. Spérino might, besides, be wonderfully reduced by making, as I before stated, the inoculations one by one, excepting towards the close of our experiments, where this discretion is no longer necessary.

You must dispense, too, with my estimate of the precise number of years that this immunity will last. How long does the vaccine disease or the smallpox itself preserve us from variola?

We cannot answer in regard to either of these preservatives, notwithstanding we have so long studied them! How can we be better informed concerning syphilis? But I am sure of being within bounds when I assume the time of this preservation to include the entire period of youth. I have arrived at this conviction from various sources, the principal of which are the experiments (already well known) and the observations which have been made. Besides, what should prevent syphilitic revaccinations, in case they should become necessary? These revaccinations would be reduced to a very few inoculations, since the only object of making them would be to prolong an immunity previously acquired, and which would not be wholly destroyed!

I do not propose to *syphilize* those who are ever free from contagion, if such individuals exist. It would be folly, I know, to insure against fire a building which cannot be burned. Let the measure, on the contrary, be applied to those who are very much exposed to syphilis, and to those who are affected by it in different degrees. The disease itself is the commencement of preservation and of cure. Our vaccination has this valuable, and I will say marvellous, property—that it produces its benefits *before*, *during*, and *after* the affection.

Reduce, then, the number of the chancres which M. Spérino has mentioned, and commence by making, each time, only one inoculation at an interval of eight or ten days. But, towards the close of your experiments, when you merely produce chancres without virulence, make several inoculations every two or three days, and even more frequently. The essential point is to proceed quickly. And then be not surprised if you do not perceive induration; there is no time for its production, because you slide, as it were, over the constitutional syphilis, of which the induration is only the index, and, it may be said, the first sign.

With *syphilizers*, the induration is not the cause, but only the effect, of the disease; and should you destroy by the knife or by fire this witness of the general state, you would effect no change whatever in regard to the contamination itself. When a person is syphilized very quickly, no indurated chancre is seen, although he certainly passes through the state of constitutional syphilis.

I will go farther; you have, in some cases, destroyed chancres before induration has taken place, and even when constitutional syphilis already existed; and cases of this kind may be offered by way of objection to your otherwise valuable theory of indurated chancre.

In this way syphilization explains facts which strike your doctrines at their very foundation.

Let us say a few words now relative to the *syphilized* patients of M. Puche. They are not under my care, although I see them nearly every day. Had not M. Ricord first alluded to them, I should not have mentioned them. This initiative step is one for which I am obliged to him, because it gives me occasion to relate two facts wholly confirmatory of my assertions. In one of these *syphilized* individuals, the *syphilization* progressed without obstacle; and in the other the result would have been similar, had he not been put, at the time the inoculations were made, under a mercurial treatment. In proof of this assertion, I will state that the suspension of this treatment put an end to the clogs which the *syphilization* encountered.

Our inoculations are not preventive alone; but they are also in an especial manner curative. This results from the fact that *syphilization* is not attained unless the organism passes, with more or less rapidity, through the state of constitutional syphilis. Now, if the organism has not been subjected to the action of the virus for too long a time, it may yet be made to reap the benefits of *syphilization*.

I should be afraid of abusing your patience, were I to insist upon conditions in regard to the seat of inoculated chancres; but you will understand how much less pain and inconvenience are caused by chancres upon the arms or abdomen than by those upon the penis.

M. Ricord earnestly desires to meet a *syphilized individual in the lists*. His wishes shall be more than gratified; for the *syphilized* person whom I will oppose to him shall also be a *syphilizer;* let M. Ricord, then, take care. He will see whether he has to deal with convictions which are growing feeble.

And let him know well, that the question at issue does not simply concern a revision of the syphilitic constitution, but a radical revolution! AUZIAS-TURENNE."

22d August, 1851.

In the strange letter communicated by M. Auzias-Turenne to the *Union Médicale*, and which is addressed rather to me than to you, he accuses me of having, though involuntarily, thrown a *false* air around *syphilization*. If syphilization has not, at first, an air of truth, the fault certainly is not mine, but M. Turenne's; I submit the question to those who are *au courant* with science. Voltaire one day said to the sister of the King of Prussia: "An air of truth is often mingled with the grossest falsehood." And I will say to M. Turenne, whose good faith I have never doubted, that, if all he advances in his letter is the expression of the truth, the lines of Voltaire must be inverted.

Great discoveries, it has been said, have often been taken for madness. Salomon de Caus was shut up in Bicêtre. Everything that differs from the usual course of events, everything that cannot be referred to established laws, is frequently assumed as extravagance. People are sometimes wrong, undoubtedly; and history furnishes too many instances of marked injustice of this kind. But is that any reason why an extravagant, eccentric, and apparently irrational idea should be accepted without examination and criticism, and accepted the more readily in proportion as it is contrary to experience and to acknowledged facts which it has neither explained nor destroyed? Is it necessary, because the idea appears *very dangerous*, to follow it blindly, without knowing into what abyss it will conduct us? No; and at the risk of being deceived, we should—without wishing to condemn to the stake or to prison those whom we believe to be heretical or crazy—employ our sagacity and reason, not to prevent progress, but at the same time not to applaud all revolutions, which oftentimes tear down more than they build up.

It is a strange circumstance, my dear friend, that, whilst for more than twenty years I have struggled to establish the doctrinal points which are the source, the generative idea of all M.

Turenne's views, the men who have used so much *black* ink in attacking my experimental researches, and have given to their pen so sharp a *point*, seeking to disprove the *unicity* of the syphilitic diathesis—a truth which is now incontestable—should no longer have any remarks to make upon the following proposition of M. Turenne, which comprises all the rest:—

If you suffer from the pox, it is because you have not taken enough of the virus!

In fact, if you consult M. Auzias relative to a chancre, he tells you to go back to the source, and again go back, until you can no longer—contract it. If you have neither the courage nor the strength, he gives you chancres until you have enough of both; how many, he does not know, because there is an infinitude of conditions of which he is ignorant, and in virtue of which the power of *syphilism*, or the aptitude of contracting chancres, may be augmented or diminished. Perhaps ten, thirty, forty, fifty, sixty, or more are required. But let the patient take courage, and the goal is reached without much trouble; for the chancres are to be placed on regions of the body where their presence occasions but little inconvenience; such as the belly, for example, of public women, or the arms of those who do not use them.

But in thus multiplying, for one or two months and more, the sources of infection, do not fear that you will be infected, infiltrated, impregnated with the virus; that is not the business of the syphilizers; they do not wish you to believe that they put the pox in the blood. It is sufficient for you to know that you are *syphilized;* that you have undergone a general modification which has forever destroyed your *syphilism*, without the mixture of the virus with your humors; M. Auzias is sure of this result, for he has followed the virus in its peregrinations; and you are about to judge of the fact.

Suppose that all, without exception, who are endowed, like every animal of the creation, with syphilism—that is to say, with the immense prerogative of being able to contract the pox, and of thus being placed beyond its power—should represent a mountain with two sides; and that a chancre seeks to climb the first side of the mountain of—Venus. If it is alone, it remains at the foot, where it may die without descendants. If, on the contrary, other travellers of the same *species* come to its aid on its route, to shoulder it along, and to cause it to take the short-cut, it may reach the summit; but if, when it has gained this point, it is not assisted, like the monkeys mentioned in the fine fable of M. Viennet, in descending the opposite slope, it is unable to advance, indurates, and sets on fire the syphilitic mountain, which then vomits its lava under the different forms of constitutional accidents with which you are familiar. But should its course be unhindered, or should it be resumed after a short cessation, and even after an *eruption*, the traveller, fatigued and worn out by the last half of its journey, carries away with itself the evil it has wrought, and finally dies in the valley of Jehoshaphat, to await the last judgment of—experiment.

However, my dear friend, in this ascending journey the virus may leave its traces in the soil, fasten itself at first to the neighboring lymphatic ganglia, then hollow out a deeper furrow so as to indurate, and, if it stops, produce general accidents; whatever may be the opinion of M. Turenne, who does not admit that it penetrates the economy, and that it enters by the absorbents, and infects and poisons the system after the manner of toxical agents. Does it follow another course when it does not indurate? No; since, in order to dislodge the first indurated chancre, we cause the syphilizing chancres to follow the same path; and necessarily so, for otherwise there would be no chance of their meeting the first, and destroying it.

Now, how many chancres are necessary to reach the summit of the mountain and to overcome the constitution? How many are afterwards necessary to establish order in the plain? M. Auzias can, as I have said, tell nothing, and cares very little about the matter; he is worse off than the man who, when asked how many rats' tails it would take to reach from the earth to the moon, replied, "Only one, provided it be long enough." Very well! Daily observation will show M. Auzias that a very large number of individuals have but one chancre; that all solitary chancres do not indurate; that the syphilitic diathesis is not inversely proportional to the number of primitive accidents; and that all the individuals who have only one chancre do not, from this circumstance alone, have constitutional syphilis. Not only so; but nothing is more common than to see individuals with the symptoms of a general syphilis, and who have had at different times, separated by a greater or less interval, sometimes one or two months, *successive* chancres to the number of ten, fifteen, twenty, and more; provided that, among these chancres, there has been one which has become indurated, or, if you prefer, one which infects the economy. In that case, you know, this chancre presents specific characters; impresses upon the economy a certain disposition, the analogue of which we find in variola; and prevents the reproduction of a similar accident with the same consequences.

If, with a certain number of chancres, constitutional syphilis must always be produced; if a *determined* number produces immunity from it, the question would be settled; but observation has already shown what are the true facts in the case.

When, with a single *non-indurated* chancre, you have no constitutional accidents, you might say that syphilization is already attained, in the same way that vaccinia is produced by a single puncture, a single vaccinal pustule; but this is not the fact, as we have seen, for, when we inoculate under these circumstances, the ulterior chancres may be followed by the poisoning, by the syphilitic diathesis.

In order to attain syphilization, weeks and months are necessary; while we know, beyond doubt, that chancre infects and indurates within a few days; and that less time is required to bring on the secondary manifestations than to prevent them.

Chancres, says M. Auzias-Turenne, are cured with rapidity in proportion as they are multiplied, and as syphilization has been attained. This proposition is untenable. It must often be reversed, and the inoculators of the present day, who formerly combated inoculations, are firmly convinced of this fact. In some cases, the chancres of inoculation have been far more grave than those from which they originated. It is not rare to see a single chancre cured, without special treatment, in three, four, five, or six weeks. If art intervenes; if, particularly, we have recourse to a mercurial treatment in indurated chancre, the cure is more rapid! Does syphilization proceed more quickly?

The diminution of intensity exhibited in successive inoculations, as in some of those made by my colleague, M. Puche, in which the inoculated pus was constantly taken from the patient himself, may be attributed to a gradual weakening of the virulence, up to the time when the chancre has reached the reparative period, and can no longer, as I have already demonstrated and taught for twenty years, furnish inoculable pus. Here the seed is bad, or it is not present; subsequently, it is the soil which is at fault.

An undoubted fact, which all observers have verified, is that there comes a moment, sooner or later, when all the chancres cicatrize, and generally almost at the same time, whether there has existed but one or a great number; the last cicatrizes as quickly as the first, and this often takes place without our being able to refer the cure to the remedies employed, and sometimes even in spite of the remedies. How, then, does this cure take place? Not, in all cases, by means of syphilization, even according to yourself, since this result is observed where there is one or several chancres, and since it is not true that all individuals are, subsequent to the cure, refractory to new inoculations. These observations, which apply to the primitive accidents, also apply to the secondary accidents, which, after having lasted a certain time, may of themselves simultaneously disappear, *independent of new contagion*. Syphilization does not explain this result. In this case, we observe what takes place in many other diseases; that is to say, an effort made by nature to rid the system of that which is not assimilable; of all that is foreign to it; a work of elimination, of repulsion, of reparation, more or less general, and capable, especially in homogeneous tissues, of preventing, at a given moment, the production of new effects, as it is about to destroy those which already exist.

Art often comes to the aid of this *vis medicatrix*, not by augmenting the morbid principle which it ought to combat, but, on the contrary, by removing and seeking to destroy it. Thus, in cnrt aeiforms of syphilis, recourse is had to powerful auxiliaries, and to medications almost specific, mercury especially, which, like all the great powers of this lower world, has alternately been exalted and debased.

After the restoration—in which the Academy of Sciences has recognized my participation, and which succeeded the physiological revolution in which the existence of the virus and consequently the efficacy of mercury were denied—observe how the power of this remedy is again questioned by the revolutionary *syphilizers*, who, like their predecessors, the physiologists, assert that it produces the evil which it pretends to cure. Is it possible that such language can be employed in the year 1851, in view of the innumerable patients in whom syphilis is developed when a particle of this medicine has not been taken, and disappears as soon as it is properly administered?

It is certainly true that this therapeutic agent is not equally efficacious against all the forms of syphilis; that there are even cases which it aggravates, as I teach in common with many other syphilographists; and that the form of the affection in which it is most frequently injurious is the non-indurated chancre (the only one which M. Auzias appears to me to have thus far inoculated), of which it tends frequently to prevent the cure, not by augmenting the syphilism, but by altering the constitution in such a manner as to favor the progress of any ulceration, whether of a chancre or of a scrofulous or scorbutic affection, and by producing even ulcerations *sui generis*.

According to M. Auzias, we must no longer look to mercury for the cure of the pox; but to the pox itself! This idea is not new, says M. Auzias. He is right. There is nothing new under the sun; not even man, when God created him, since he was only an image of God himself, according to Holy Writ, which recorded the fact prior to M. Alexander Dumas.

In fact, Percy, cited by Petit-Radel, thinks that the doctrines of Bordeu may be applied to the treatment of syphilis, and that chronic and rebellious cases of syphilis may be cured by bringing on the acute stage, and by renewing the affection, as some persons still recommend to those who have chronic discharges. It is thus that Percy inoculated his patient; the inoculation did not cure; but the patient was cured by a methodical and rational mercurial treatment of a disease which, according to M. Auzias, should have become worse, the mercury here neutralizing the benefits of the syphilization.

M. Auzias reproaches me with making a partial statement of M. Spérino's facts. As to my approximation to the number of inoculations required to be made, I maintain that I am correct. As to the phagedænic chancres, the cure of which was not prevented by the new inoculations, there is nothing which ought to surprise us; nothing which does not occur every day.

I have stated, and I still maintain, that "every one does not have the pox who wishes to have it."

Finally, I have been reproached with abandoning the flag of Hunter, on which, among other things, is inscribed the unity of the virus. I have already made my profession of faith, and made known the colors of my banner; I shall not return to this matter. I will only say that, if what I have taught in my lectures for many years is about to be verified—to wit, that syphilis, which is so analogous to the smallpox, a fact made especially apparent since I have demonstrated the unity of the diathesis, must also have its vaccine—and if the assertions of M. Auzias shall be demonstrated, it would seem probable that the virus furnished by the non-indurated chancre is different from, or is *a modification* of that produced by the infecting, indurated chancre, and that the first is to syphilis what the vaccinia is to variola, influencing the economy after a local effect is induced without general manifestations, and preventing the other from afterwards acting, either locally or generally.

This, you perceive, is a grave question, and merits the greatest attention. To encourage young men to multiply the accidents of primitive syphilis, is to encourage them to return to the source whence they have derived them. To say to those who have constitutional syphilis: "Go; have no fear; allow secondary and tertiary manifestations to appear; omit the employment of remedies reputed efficacious; for, whenever you may please to do so, you can be cured by contracting new chancres;" the use of language like this, I say, would be too serious a matter in the eyes of those who are placed at the outposts of society, and on whom a certain degree of responsibility rests, not to induce them to demand facts in place of theories which, up to the present time, rest on no legitimate foundation, but which, on the contrary, all experience seems to condemn.

1. Therefore, I require M. Auzias to show us his syphilized individuals. They are all ready, he says; so much the better; I will then be convinced that a person may be refractory to inoculation.

2. I require the limit of the immunity, to which M. Auzias seems to attach but little importance, but in which the syphilized must be much interested. With this limit, M. Auzias must be acquainted; for, in such a matter as this, cases of yesterday do not form a proper groundwork of opinion. I am therefore justified in demanding the oldest cases.

3. I require that M. Auzias shall produce at will indurated chancres upon the first comers; that he shall arrest some of these chancres at will by syphilization; that he shall allow others to proceed to secondary accidents, which he shall afterwards destroy by his inoculations.

4. Let him present us, before and after these inoculations, with patients affected with constitutional syphilis at different periods, and cured by the syphilizing inoculations; and I will accept the revolution.

Until these conditions are fulfilled, my dear friend, your journal, which is of so discreet and rigid a character, ought to accept such works as those of M. Auzias only with extreme reserve, and without guarantee—I was on the point of saying, without encouragement; for when we call to mind the misfortunes which happened to the physiological school, whose adepts were as firmly convinced and as honest as M. Auzias, we tremble in view of the terrible consequences which reason, clinical observation, and science, give us occasion to fear.

Yours, RICORD.

LETTER XXXIV.

MY DEAR FRIEND: It is a very long time since I wrote you my first letter; it is also a very long time since you received my penultimate; and, however agreeable this correspondence may be to me, it cannot, like everything which is too prolonged, afford you pleasure. But this is not my fault, but that of time and circumstances; for I recollect one of your aphorisms: *Pleasure is only pleasure because it is rare and brief.* If my letters have caused you some satisfaction, it is because they have possessed at least one of the conditions of your programme.

The hope entertained by syphilizers that the pox will one day disappear from the list of diseases, and that it is essential to remove from treatises on therapeutics the useless pages on which antisyphilitic remedies are indicated, for a moment arrested my attention. Why continue the history of a disease which is about no longer to exist, and why speak of treatment which will, in that case, have no farther application? Hence, I was about to bid you farewell, when a visit to the hospital convinced me that, whatever might be the future history of syphilization, its present character is still sufficiently serious to induce us to leave our classical works on this subject intact; and we remain persuaded that the pox, alas! is neither dead nor dying!

In fact, while waiting until the idea of syphilization, the product of my school, which has prophesied a vaccine, shall be demonstrated by syphilizers; while waiting until it shall be proved that the pox, up to the present time, has been calumniated by all syphilographists of past and modern times; while waiting until it shall be recognized that, instead of being one of the greatest scourges ever inflicted upon humanity, syphilis is, on the contrary, a blessing from heaven, let us still direct our attention to those features of the disease which, whether a scourge or a blessing, are still sufficiently prominent to deserve our notice.

In a prophylactic point of view, I told you, in my last letter but one, that it was impossible to believe in a preservative inoculation with the pus or blood of tertiary accidents; and that syphilization, by means of experiment, ought to be carefully studied before being seriously adopted.

On this subject, I will say that a *courageous* student of medicine has presented himself at the clinique of the *Hôpital du Midi*. This student has submitted to experiments for the past three months. During this time, he has himself made more than sixty inoculations, the traces or cicatrices of which are visible; one of these still presenting, on the twenty-first day, the characters of ecthymatous chancre. I would give you an account of the result of the experiments which were to be continued at my clinique, if, since the first publication of this letter in the *Union*, this pretended syphilized person had not ceased to submit himself to my examination.* This case would have

---

* But in lieu of this patient, we have had the unfortunate case of Dr. L——, which was presented to the *Société de Chirurgie*, and which imposes a terrible responsibility upon the heads of those who recommend measures to which they should be condemned themselves to submit to.

The following are the words in which the *Union Médicale* published the transactions of this learned society at its session of the 12th of November, 1851:—

"We have the following communication from M. Musset. We transcribe it word for word:—

"Dr. L—— was presented to the *Société de Chirurgie* by M. Musset, interne of the service of M. Ricord, in order to submit to the observation of this learned society the results of experiments undertaken with the object of verifying the views which were presented relative to syphilization.

"While awaiting, from Dr. L—— himself, a complete history of his own observations relative to the case, we shall present the principal results at which he has arrived:—

"Doctor L—— had never had either chancres or blennorrhagias.

"In the months of December, 1850, and January, 1851, he inoculated himself upon the penis, each time at an interval of one week, with ten chancres, for the purpose of studying a new medication. These chancres disappeared in a short time under the influence of a simple, hygienic treatment.

"On the 2d of July, he inoculated himself anew upon the left arm; and an indurated chancre was the consequence.

"Three months afterwards, that is, on the 1st of October, an exanthematic, and soon a papular, syphilide appeared, accompanied by engorgement of the posterior cervical ganglia.

"Some days afterwards, mucous papules appeared upon the tonsils.

"Dr. L—— underwent no treatment.

"On the 17th of October, an inoculation was made upon the left arm, by M. Auzias, in presence of M. Ricord, with pus taken from a chancre twenty days' old, existing upon a patient who had himself been inoculated with the pus taken from a pretended syphilized individual who had contracted nearly his sixtieth chancre.

"On the 24th of October, M. Ricord made two inoculations, one upon the left arm, the other upon the mucous membrane of the prepuce, with the pus of a non-serpiginous phagedænic chancre, existing on a patient in his wards.

"On the 25th of October, Dr. L—— inoculated himself on the same arm, and on the penis, with the pus of the first chancre.

"On the 28th of October, two inoculations were made upon the left arm; one with the pus of the first chancre, the other with that of the fourth.

furnished us, at best, but a single observation, of which you may conjecture the value; and there seem to be others, at which

"On the 29th of October, two inoculations were made with the pus of the fourth chancre.

"On the 30th, two inoculations were made upon the arm with the pus of the first and second chancres.

"Thus the number of inoculations amounted to eleven.

"1. Although ten inoculations had been made, this did not prevent an eleventh from indurating, and from being regularly followed by constitutional syphilis.

"2. The new successive inoculations, which were made in view of syphilization, were all successful.

"3. The chancres had not a less extent in proportion to the inoculations made.

"Thus, the diameters of the successive chancres were indifferently greater or smaller than the diameters of chancres which preceded or followed them.

"4. Most of the inoculated chancres assumed the phagedænic form, as may be frequently observed in individuals who, having a constitutional syphilis, contract new chancres

"5. It is to be remarked that the most intense chancres were produced by the pus of M. Auzias's syphilized individual, who had contracted his sixtieth chancre.

"6. The non-serpiginous phagedænism did not depend upon the source whence the pus was taken, for most of the chancres produced by the pus of the syphilized individual assumed indifferently the phagedænic form; whilst of the three chancres produced by the pus of the patient in the wards of M. Ricord, affected with a non-serpiginous phagedænic chancre, only one took on the phagedænic form.

"7. The phagedænism of the first chancres was not destroyed by those which followed them, and which in turn became phagedænic.

"8. Hence, the phagedænism appeared to depend upon the general state of the patients, influenced by conditions in which he was placed: for, while most of the chancres inoculated upon the arm assumed this form, those inoculated upon the penis, the same day, with the same pus, remained very small, and quickly went on to reparation.

"9. Not only did the successive inoculations, made with the object of producing syphilization, and which took so grave a course, fail to influence favorably the accidents of constitutional syphilis; but, on the contrary, these accidents seemed to assume a new intensity in proportion to the phagedænism of the chancres of inoculation.

"10. It is to be remarked that, while all the inoculations made with the pus of primitive ulcers, were followed by positive results, those made with the pus of secondary accidents pertaining to the gravest forms of the disease, and in all its intensity, remained without effect.

"11. The case of the courageous and learned Dr. L——, which he will hereafter publish, with all its developments, should serve as an important lesson to those who, crying up the doctrines which lead to such results as we have just contemplated, have not the courage to experiment upon themselves."

I am not surprised, because cases of this kind are necessarily very rare. In fact, in order to submit one's self to such experiments, more confidence in the doctrine is required than he who teaches it exhibits; for *the teacher does not set the example.* I have been told that what prevents *some* from inoculating themselves, or from making known that they have inoculated themselves, is the fear that a knowledge of the fact might injure their reputation in the world with respect to marriage! This is perhaps true, and I do not dispute the legitimacy of this apprehension; but I am astonished that these benefactors of humanity can be accessible to such common apprehensions. The school of the prudent Fontanelle is not dead, my dear friend; and there are still people whose hands, when opened, are not full of truths.

Be this as it may, let us return to the truth which I seek to establish. In the present state of science, the best means of preventing constitutional accidents consists in destroying the primitive accident as soon as possible; a fact which I mentioned while speaking of chancre.

But when we do not see a given case until it is too late to count with certainty upon the abortive method, is it necessary, in all cases, to have recourse at once to a general specific treatment? I answered this important question in the negative a long time ago. The infecting chancre is the rarest of all forms of the disease. In other forms, whatever may be the number and duration of the accidents, and the degree in which they are repeated, constitutional infection does not occur; and in that case treatment becomes not only useless, but it may be sometimes injurious.

Some specialists, convinced like myself that the greater part of primitive accidents soon get well by hygienic measures or by simple medications, wish the physician to wait for proofs of the general poisoning, before he has recourse to energetic special treatment. Others, who recognize the necessity of this treatment as soon as the chancre presents the characters upon which I have insisted, are also unwilling to employ such treatment except when general accidents are manifested in such a way as not only to demonstrate its actual necessity, but also to make the patients comprehend that the treatment must be continued for a long time.

For my own part, when I have to deal with an infecting chancre, I resort, as soon as possible, to special medication—that is to say, to the mercurial treatment.

The mercurial treatment may prevent the constitutional manifestations, or simply retard them for a time, which time it is difficult to limit by months or years. Every practitioner has seen patients who, after treatment, have enjoyed for ten, fifteen, twenty, or thirty years, excellent health, and who, at last, have presented, either for the first time, or as a relapse, the characteristic accidents of syphilis. In view of such facts, unfortunately so numerous, how is it possible to deny that the persistence of the diathesis is compatible with apparent good health; how is it possible to conclude that, in all cases, there is an absolute destruction of the acquired syphilitic disposition, as some speculators have, on such slight grounds, inferred?

The certainty of being able to destroy the diathesis by judicious medication, which, in fact, ought not to be impossible, would be obtained by means of well-authenticated, well-described, and carefully analyzed cases of individuals who have had indurated chancres twice or oftener, and who have each time presented the series of constitutional accidents in the natural order with which we at this time are familiar. Now, these cases—which may possibly exist, but which I have not yet found—are yet to be seen by close observers, whatever may be the assertions of some who are but little versed in the study of syphilis.

Honorable therapeutists, then, are able to affirm that they prevent constitutional accidents, or cause them to disappear, in a great number of cases; but they are not able to affirm that these accidents will never reappear.

Neither the form, nor the daily dose, nor the absolute dose of the remedy always gives immunity from the disease, whatever accessory attentions be paid to the case.

It is especially necessary here that the profession—the art, I was about to say—should have proper regard for science. It is essential to acknowledge that our calculations on this subject are merely based on probability; for those of Hunter, which have an appearance of mathematical precision, are far from being true.

To continue the treatment only until the disappearance of the symptoms, is the method which leaves most chance for the return of accidents. The treatment of the case for as long a period after as before the removal of these symptoms does not lead to more satisfactory results; for it may often be either too short or too prolonged. Finally, salivation, as a measure of the treatment, presents still more inconveniences, and fewer guarantees, than the other methods.

Six months' treatment by a daily dose which perceptibly affects the accidents we seek to combat, and which indicates, after they have been destroyed, that the medicine still produces its physiological effects, is, at present, that which gives satisfaction to many practitioners, and which appears to give the best established cures.

But, whether administered to remove the primitive accident alone, or whether it is resorted to in order to combat secondary accidents, the treatment, as I have stated, may modify the time of appearance and the order of filiation of the symptoms. More powerful against secondary than against tertiary accidents, mercury sometimes prevents the manifestation of the first, and permits that of the others. Thus, after a chancre has been treated by mercury, the first constitutional manifestation may consist of an exostosis, and appear, to certain minds which cannot count excepting on their fingers, a secondary accident of the tertiary form, as though it were merely this character which decided its nature. In the same manner, and under the same influences of treatment, secondary accidents may be manifested after the tertiary, and thus give color for a moment to those criticisms with the force of which you are familiar. But all this, my dear friend, is, you know, so far from being disorder, that it is only an effect of art, and demonstrates its power. When the disease proceeds without complication, this result never takes place. I will farther add that my colleague, M. Cullerier, believes this order to be so inevitable, that it cannot be interrupted by medication. Thus. in his opinion, the accidents ranked as tertiary, are always preceded by secondary ones; but my experience is opposed to this opinion, which is not sustained by the results of treatment.

The manner in which I have comprehended the evolution of the disease, and the methodical classification which I have made of syphilitic accidents, have permitted me to resort to a rational medication, and only to administer mercury in cases in which it is useful; for this remedy has been too much rejected by some or too prodigally given by others. Thus, it is this superior application of the remedy which the Academy of Sciences has seen fit to recompense.

Thus, I think I am justified in stating that the iodide of potassium, at first recommended as a general medication in syphilis, and which, for that very reason, gave such uncertain therapeutic results, and sometimes results so contrary or at least so little satisfactory, has been reserved more especially, in consequence of my clinical studies, for the series of accidents which I have called *tertiary*, upon which it has an all-powerful action.

The therapeutics of syphilis may, to-day, be summed up in the following manner:—

1. Abortive treatment applied to the chancre as soon as possible.

2. Mercurial treatment reserved for the indurated chancre and for secondary accidents.

3. Iodide of potassium applied to tertiary accidents.

4. Mixed treatment, by mercury and the iodide of potassium, in tardy secondary accidents, or when tertiary accidents exist at the same time.

Permit me, my dear friend, here to close this series of letters; permit me also, while thanking you for the kind reception which you have given them, to believe that, whenever the occasion may arise, you will ever be willing to afford me the hospitality of your journal.

Adieu, then. Yours, RICORD.