LETTERS ON SYPHILIS.

LETTERS

ON

SYPHILIS.

ADDRESSED TO THE

CHIEF EDITOR OF L'UNION MEDICALE.

BY

PH. RICORD,

Chirurgien de l'Hopital du Midi, &c. &c.

TRANSLATED FROM THE FRENCH,

WITH AN

ANALYSIS OF THE ABOVE LETTERS,

EXTRACTS FROM THE CLINICAL LECTURES OF M. RICORD ON THE TREATMENT OF VENEREAL DISEASES,

AND

AN APPENDIX,

CONTAINING ALL THE FORMULÆ OF THE VENEREAL HOSPITAL AT PARIS.

BY

D. D. SLADE,

Member of the Massachusetts Med. Society ; Member of the Boston Society for Med. Observation ; formerly House Surgeon at the Mass. General Hospital ; late one of the Dispensary Physicians, &c.

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TO

DR. J. MASON WARREN,

WHOSE FRIENDSHIP AND EXCELLENT COUNSELS

IT HAS BEEN MY PRIVILEGE TO ENJOY,

This Volume

IS BY PERMISSION DEDICATED.

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TRANSLATOR'S PREFACE.

In undertaking a translation of these letters of M. Ricord, we have been actuated by a desire to make known to practitioners here, the opinions which are the best received and the most worthy of confidence at the present day, on the subject of venereal diseases.

As a writer and an expounder of his doctrines upon this important subject, we must all acknowledge that M. Ricord has just claims to be considered as the first authority. For who can we consider as authority in medical science, if it is not those who have devoted themselves constantly, for a long period, to daily practical observation and experiment? Every one is certainly entitled to what his experience and observation appear to have taught him; but what, after all, can the opinion of the great majority of the profession amount to, when compared with the immense experience of M. Ricord in these diseases?

It is useless to comment farther upon the claims which our author has to our full confidence.

Whoever has had the advantage of listening to his teachings, will testify to his constant readiness to explain his doctrines, and to render them clear and indisputable at the bed-side.

The language of M. Ricord is in many places difficult of translation. He makes use of phrases and of words which are certainly peculiar and original. We have, however, succeeded, we trust, in making his language sufficiently clear for the understanding of all.

His letters are intended not as a *manuel*, but rather as a general explanation of his views to the practitioner, and to the advanced student, who have already observed the nature and course of venereal diseases.

The Analysis which we have prepared, will, we trust, be found to give a concise view of the opinions expressed in the letters. Certain portions of the letters must, however, be read by him who desires to understand fully the opinion of our author upon particular points. We may fully believe that any medical man may derive benefit from a full and thorough perusal of them.

As to the "Extracts from the Clinical Lectures," they are sufficiently in detail for the practitioner.

We are confident that this volume will enlighten many upon the subject of venereal disease, particularly syphilis, upon which subject we are inclined to believe there is much ignorance and empiricism among us, even at the present day. If it serves to advance a true knowledge upon these points, our object is attained.

D. D. S.

5½ Beacon St.

INTRODUCTION.

TO MONS. RICORD.

My Dear Friend—

My first words should be an expression of gratitude to you.

The Journal, the direction of which is entrusted to me, has been fortunate in receiving your valuable communications, and personally, I am still bewildered by the honor which you have conferred upon me in associating my obscure name with the glory of yours.

Your *Letters* have obtained the highest success which our medical literature has ever recorded. I am well aware, and I ought to give you my opinion upon the point, that some persons, having, alas! too good motives for liking neither the spirit nor the style of your letters, blame them exceedingly. How fortunate you are in not being at the commencement of your professional career; you would certainly be ruined as a practitioner! A physician, a man of energy, who dares to write correctly and gracefully in his own language, and to be sufficiently impertinent to give witticism and piquancy to his stories, who does not flinch from an anecdote, and who does not fear, imprudent though he may be, to make his reader smile, would be undone, as would you, my friend, you who have shown yourself a *spirituel* and shrewd writer, a critic of a charming atticism, and an agreeable story-teller in the midst of the grave subjects upon which you have seriously treated. A physician who aspires to practice, has no worse reputation than being a man of science. Thus at one of the last concours of the Faculty of Paris, a fortunate candidate, although eminently *spiritu-*

el, was obliged to receive this singular compliment, as a home-thrust from one of his judges: I am satisfied with you; you have manifested no *esprit*.

Surely Guy Patin was happily inspired in not addressing his agreeable letters otherwise than under the cloak of friendship. If others besides his friends, Spon and Falconnet, had doubted his original and piquant fancy, the vigorous and *spirituel* enemy of antimony and of Mazarin, would have enjoyed neither his rich practice, the honors of deanship, nor his chair in the College of France.

And yet—trust to my little experience in horticulture, and I refer you elsewhere for authority—the rarest and most beautiful flowers, in order to exhibit their brilliant colors, require a still richer earth than even the most valuable grains.

You have done well, then, in commencing by solid treatises, by a large octavo; in afterwards advancing to a heavy folio, entirely filled with beautiful illustrations; in having annotated the grave and learned Hunter in the beautiful translation with which our learned and modest friend Richelot has gifted French medical literature, before writing your letters. Without this *bagage* so respectable, you would run the risk of not being considered *a serious man*, by a great number of venerable colleagues who value success only by the weight and the volume. You well understood something of this when you knocked at the door of our Academy, which ought to have been opened wide to you, but which they twice made so narrow for you, that your merit could not enter therein. Have you well understood for what they then reproached you? For your teachings, my friend; for these teachings, at once so instructive and so amusing; for your impromptu sayings at the hospital, so picturesque, so colored; for your lectures so attractive and so imaginative, of which your *letters* are such a faithful repetition. In place of putting your audience to sleep, you kept them constantly awake by means of the twofold attraction of science and of wit. Now there are many people who do not desire to be disturbed in their sleep. It is this which drew out the remark from a friend of mine, who with much esprit, had the good sense to make it only in private,—that only fools in medicine had any wit. It is true, my dear master, that this friend placed you in the front rank of fools.

You well understand that it is not solely to thank and to compliment you, that I have ventured to write you this letter. No, and I do not even know how to reconcile my commencement with what is to follow, for I have to reproach you, and to point out to you an omission.

The reproach which I have to make to you, is not only the expression of my individual opinion, it is the sentiment of a great number of our colleagues, men of taste, science and prudence, and whose opinion and advice you are accustomed to hold in high esteem.

Well, as a faithful reporter of what I have heard, I reproach you for having given too much importance to certain recent ideas upon *syphilization* and *syphilism*.*

There are ideas and pretensions in science which we must suffer to progress to a certain extent before occupying ourselves with them. To criticize them too soon, is to give them somewhat the air of martyrdom, which they do not fail to profit by. Science is paved with these misunderstood geniuses, with these persecuted inventors, who run after the inquisition of Galileo. You well know that it is behind this great name that all the follies and extravagances of the human kind shelter themselves! and you know, also, that for one Galileo we find a thousand Cyranos de Bergerac.

It is one of the greatest and most incontestable principles in the Baconian philosophy, that in scientific criticism, an idea, an assertion, a theory, is nothing without demonstration, without proof, without fact. Now this fact which you demand, is not brought forward; this is all which you have to establish. To enter upon the speculative and dogmatic field

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* I ought to say, however, that this reproach ought not to apply entirely to M. Ricord. The publishing committee of l'Union Medicale received the letter of M. Auzias Turenne, and it was asked what should be done with it. Not to publish it, was to give a pretext to the author to cry out against the systematic repression of his doctrines. To publish it without reflections and commentaries, was to assume a responsibility, that none of the members of the committee, and the principal editor in particular, wished to accept. Consequently, M. Ricord was invited to answer, and I consider it as unfortunate, that this invitation conformed too well with his wishes.

was to expose yourself to be beaten by adversaries who had managed the perfidious and so often deceptive weapon of dialectics, better than you, man of practical science. By reasoning, one can prove all which he wishes to prove. Our learned and *spirituel* friend, Malgaigne—another *fool*—proved to us one day, by means of an irreproachable syllogism, that a part was as great as the whole. Very wise men were present who revolted *in petto* against this audacious paradox, but who remained with their mouths closed, so logically unassailable was this paradox.

You practise general surgery with the same success which you do special surgery; and were you not slightly bitten by the tarantula which bit, some few years since, the surgeons of the day, as regards strabismus? Confess that you have also upon the conscience some section of the muscles of the eye. But as you are a loyal and sincere practitioner, I am sure that now you will acknowledge with me, that ocular myotomy caused more strabismus than it cured. Well! I who from taste and duty occupy myself a little more than you with the maladies of the mind, have discovered one which I call intellectual strabismus.

Fix the eye of a person who squints, you never know if he is looking at you. Listen to or read an intellectual squint-eyed person, and I defy you to divine whether he speaks or reads from reason or conviction. If you seek to correct a reasoning which appears crosswise, you do nothing but displace the deformity. He squinted to the right; he will now squint to the left—absolutely as happens after ocular myotomy in visual strabismus.

Do not deceive yourself, your reflections upon syphilization will have had no effect upon the inventors and propagators of this, except to produce a little more irritation against you, since they will accuse you of wishing to stifle the truth.

As to the facts which you demand, take care! Nothing is more deceptive and more perfidious than the medical *fact*. It is a very long time since, in my lucubrations of Journalist, I asked what is a fact? Let some one be pleased to give me the definition, the characteristics of it .Our great philosophers have not yet had time to satisfy my curiosity, and I am compelled, as before, to admit or reject a fact solely according to the feeble light of my own intellect. You, however, know the number of errors and follies which have been put in circulation in medical science by the aid of pretended facts. Syphilography (and you can attest this better than any one) has its part in the quota of absurdities supported by facts.

And observe, it is not only a medical fact already tolerably complex which you will have to appreciate, but an experimental fact, which singularly complicates the problem, and which ought to excite all that portion of your nervous system which presides over attention.

But what am I doing? I am preaching to a convert, am I not? You who have exhibited so penetrating and decisive a criticism against inoculators of secondary symptoms, will not suffer that valiant sword to vacillate in your hands, when the hour shall come, if it ever comes, to combat the theory of syphilism. The public who love you and who esteem your works, repose their confidence upon you as regards this. But what shall I say to you? My affectionate regard authorizes me to say here, that the public have been troubled by some expressions in your last letters. It has seen a little compliance, a little weakness, perhaps, towards syphilism, in that *naïf* avowal, that *it 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 on account of this, you are bound to exercise a greater reserve in recognizing your children. You ought only to have legitimate ones, and if you assume absolutely to be the St. John forerunner of syphilism, you contract, by this, the obligation to announce only the veritable Messiah.

Now, I do not fear to say here, that the theory of syphilization, such as is produced at Turin and at Paris, does not yet merit to fix the serious attention of men like yourself. It may make, it has already made, victims; the best of reasons why you should not give to it an untimely criticism, an appearance of importance. For you know, a theory which one does not contest, remains in the condition of theory; criticize it, and it passes into the state of a religion, and every religion has its martyrs. Do you not think that syphilis makes enough of these martyrs?

You understand incontestably better than myself syphilographic history and literature; you know also, better than I, that this portion of medical science has been, since the end of the 15th century, a fertile soil, from whence eccentric ideas and strange opinions have plentifully sprung. Have you not been surprised in the course of your reading, to see that all these eccentricities, with whatever noise they have been brought forward, have positively had but little effect upon true and positive ideas? Is not this to be attributed to the little attention which men truly serious, give to them?

One example only—for I have a horror of all appearance of erudition; erudition ought to be only an intimate enjoyment, one of conscience; I shall say of it what Voltaire said of self-love, which he compared to the generative organ, which affords pleasure, that one is very glad to have, but which one must conceal.

In 1811, several years before the physiological school, bringing forward again the forgotten theory of Bru, thought of denying the existence of the syphilitic virus, a pamphlet appeared, entitled

"UPON THE NON-EXISTENCE OF THE VENEREAL MALADY, *a work in which it is proved that this malady, invented by the physicians of the fifteenth century, is only the reunion of a great number of morbid affections of different natures, the cause of which has been falsely attributed to a contagious virus, which has never existed.*" Surely, therein was an exciting title, and from the parenthesis you see that it is cousin german to the title of the more celebrated work of M. Richond des Brus. This pamphlet suited the entire doctrine, I might say the entire religion, upon the venereal virus. More than temerity was necessary in thus daring to brave all the medical belief of the time. The author well knew this. Observe the proud disdain of his commencement:

"Let one of those incredulous persons who believe only what they see, or one of those men disposed to believe every thing, be placed upon an elevated tower; let him be made to examine the sun from morn till night; he will see the sun rise on one side and disappear on the opposite side, and he will be well persuaded that every day it takes the same course."

"This is what the philosophers of Greece and Rome believed, the wise men of Judea, Arabia and China, and what we should still believe, if men of genius, elevating themselves above opinions generally received, had not proved that the sun always remains in the same place, and that it is the earth which moves around it."

"We know"—here Galileo comes in—"what persecutions Galileo met with for having proclaimed this truth. I find myself in nearly the same condition as Galileo," &c.

Thus the author awaits all sorts of persecutions. But re-assure 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 do not know of any. I have never found any trace, souvenir, or mention in the literary history of the time; the Ricord of the epoch, Cullerier the ancient, perhaps read it, but certainly did not speak of it. And yet that work is not without value, I assure you; in form it has expression, style and much spirit; and at bottom we find many ideas, which, although paradoxical at that period, have become truths in your hands; as, for example, the distinction between benign and virulent blennorrhagia. I even believe, Heaven pardon me, that Jourdan only developed and extended the different chapters of your anonymous friend.

This doctrine, however, was extinguished in silence and forgetfulness. It required all the revolutionary power of Broussais, all the ardent proselytism of his school, to bring it to life some years after; fortunate resurrection for you, my friend, who have had the glory of crushing it to nothing, and of seating the doctrine of virulence upon the solid basis of observation and experiment.

But I have also to express to you the regret, and particularly in the name of your most fervent readers, that a deficiency exists upon a subject which it appears to me was suitable and apropos to your *letters*.

Where is syphilis chiefly contracted at the present day?

This question, if you had proposed it, would have led you to treat upon one of the most serious and the most delicate points in public hygiene and medical police. I shall point out the problem, without being able to solve it, happy if I succeed in making you take up the pen once more to expose to the public what your particular and favorable position has taught you upon this subject.

Two facts, equally important, but between which we see no relations, strike at the present day the attention of all those who occupy themselves with the study of syphilis, as regards the public hygiene.

On the one hand—and I speak particularly of the civil portion of the population, for it appears that in the army it is not exactly the same since the employment of certain measures ordained in 1842—the number of men affected with syphilis does not sensibly diminish.

On the other hand, the number of diseased prostitutes diminishes in a considerable degree; to such a degree, in fact, that according to an official communication which the learned M. Trebuchet, the chief of the sanitary bureau of the prefecture of police, lately made me, the dispensary met now with scarcely one diseased woman in four hundred.

Whence comes this apparently contradictory result, of the decrease of the disease at its very source, while the number of syphilitic men is almost equal what it formerly was?

It is, they assure us on all sides, because the source of syphilis at the present day is changed. The disease, pursued as regards public prostitution by measures so wise and efficacious taken by the administration, has tended to concentrate itself entirely among that population of women which is becoming more and more numerous, who carry on clandestine prostitution, against which the police, believing itself without authority, leaves the public without protection.

Who can say, better than one placed as you are, making observations at once in a vast clinique, and in an immense civil practice, how much there is which is well founded in this assertion?

If all this is true, would it not be better for the interests of the public morality and health, to extend and enlarge the definition of prostitution?

Is there not occasion for calling the most serious attention of the vigilant magistrates of the city, to the necessity of putting a stop to this prostitution, a thousand times the more dangerous, as it is more attractive, and from whence syphilis is contracted and propagated with a terrible rapidity?

We call this kind of prostitution *clandestine;* singular secrecy, when it is practised behind the scenes of theatres, in the public balls, in those places of pleasure, which are to-day only immense brothels! What! the public thinks it has the right to shut up at Saint-Lazare, an unfortunate prostitute, without process or trial, who shall have transgressed some point of the severe discipline which rules her, and yet finds itself disarmed before this cohort of females who can compromise the fortune and health of young men with impunity! What! the police has the right to penetrate at all hours the houses where fools and dupes deliver themselves up to the chances of games, and yet it stops undecided upon the threshold of a courtesan who poisons ten or twelve lovers a day! What is prostitution, then, if it is not "the publicly known commerce of one's charms"? It is necessary, they say, that there should be some provocation in the public street in order to make arrests. Therein lies a very bad test of prostitution. The tolerated houses which are the most frequented, take good care not to give any direct provocation; such a course would immediately deprive them of their prudent and rich run of practice, and yet the police do not the less hold them under its beneficial inspection. Yet what is it, if it is not the most manifest provocation, which is seen in those strangely lascivious dances at the balls of Asniere and of the Mabille; those nights at the opera where the provocation is in every thing, in the costume, in the gestures, in the voice; in those nocturnal orgies in the private saloons of some famous establishments, the description of which puts into the shade the frightful pictures of the Romans at the decline of the Empire.

What pen is better competent than yours, to describe the ravages of that prostitution called clandestine, the evils which it occasions, and the troubles which it excites in families? Who, better than yourself, could follow the syphilitic poison, taken from sources at the present day so numerous, infiltrating the highest classes of society, infecting the most chaste and purest wife, rendering her barren or unfitted to carry to full term the fruits of conception? Who, better than you, could trace the affecting history of him who has *inherited* syphilis—the subject, I well know, of your gravest and most serious studies? Who, better than you, in fine, could point out to the administration, the surest prophylaxis, the only one which would be certain and efficacious, and which the hand of the police ought to confide to medical science?

I am well aware that all this is very difficult and delicate to treat. I well know that in spite of the estimable works,—and in the first rank we must place the wise and prudent work of Parent-Duchatelet—there remains much to speak upon, and particularly much to act upon as regards prostitution; I well know that the administration too often finds itself powerless in repressing abuses of which it is not ignorant; I well know that prostitution at the present day is imperfectly and very arbitrarily regulated, and that the administration itself demands a power less contested, and a jurisdiction more legally constituted; I well know that great and numerous efforts in this sense have been tried by governments which have existed since the Convention; that it is more than doubtful if a legislative assembly ever consents to occupy itself publicly upon this sad and painful subject; finally, I am well aware that the researches upon prostitution and upon its causes are connected with the most intimate inquiries into social economy, with the condition of women in modern society, with their wages, &c., and that some recent exaggerations in this respect have thrown trouble and indecision into the most generous minds.

Yes, all this is full of difficulties; but in presence of this fact immeasurably grave, to wit, that this prostitution which I do not wish to call legal, much less official, which exists at the present day in the city of Paris, is a social evil incomparably less than that which results from what we may call prostitution free and without shackles. I believe that there is something to be done here, to employ a vulgar expression, but one which is very applicable here, and it is your ideas upon this subject that I should be happy to be able to transmit to the readers of your *letters*.

For, like myself, you think that the noblest mission of our science and art, consists not in curing diseases by therapeutics, but in preventing them by hygienic measures. I therefore deposite these ideas with confidence and as in a propitious soil, in your mind and heart. You owe to syphilis, to the pathological and therapeutical study of it, the finest part of your legitimate renown; it is to you especially, that reverts the glory of having almost extinguished the poison in public prostitution by your intelligent counsels upon the use of the speculum in the researches for the virus. You must complete this truly humane trilogy, my friend; pursue and cause this frightful malady to be tracked even into the perfumed boudoirs of our modern Lais. The poison incessantly pursued, tends to disappear in the Venus of the cross-way; having taken refuge in the libidinous and covetous *alcove* of unpunished courtesans, it thinks itself safe from the investigations of the *bureau des mœurs*. Prove that syphilitic virus ought not to enjoy the right to an asylum, any more than robbery and murder, and the public morality will be grateful to you.

Yours, affectionately,

AMEDEE LATOUR.

LETTERS ON SYPHILIS.

FIRST LETTER.

My Dear Friend,—The modern doctrine upon Syphilis meets the fate of every scientific discovery. For nearly twenty years, I have endeavored by my teachings and my works to instil this doctrine into the minds of my cotemporaries. I see, however, that it is not equally understood by everybody; certain adversaries still raise objections, which I have a hundred times refuted; and more curious still, certain others again take up objections started by myself, and imagine, a little ingenuously perhaps, that they can subdue me by arguments which I have introduced into this discussion.

At this, I am neither astonished nor indignant. I find in it, on the contrary, a new incentive to continue my task, and far from complaining of my adversaries, I shall thank them rather for not suffering my zeal to languish, by thus keeping it awakened.

Therefore, I ask of you permission to give to the world, through the columns of your widely-spread Journal, the true doctrines of the "Hopital du Midi." I ought to tell you that it is a general exposition, that I intend to make, rather than a special reply. I shall meet with objections, in my course, and I shall try to answer them. I shall pre-occupy myself also so far as I ought, with a recent publication from the pen of one of your fellow-laborers, who to find *followers*, had no need of seeking them modestly, "*en Province*."

I present to you, my friend, a preliminary reflection induced by that publication to which I have just alluded. Although an observer is not permitted to see all the facts in one entire department of pathology, and to establish a general system, we must not conclude that this observer has seen, done or established nothing, that his studies and his researches ought to be regarded as useless, and that we ought to consider his teachings as worthless. This manner of philosophizing in medicine, perhaps a little too common at the present day, is convenient and expeditious, but it is neither true nor just. In syphilography especially, this manner of proceeding would lead to deplorable errors. A serious study of the history of our profession demands more moderation in language, more justice in its appreciation. For myself, I am pleased to acknowledge and to say, that very far from entertaining the idea that every thing in syphilographic literature should be despised, those who know how to search for them, can find worthy and curious observations, good precepts, even sometimes doctrinal whims which no one thinks worthy to bring forward, since they discredit their source. Certainly the long discussions upon mercury, guaiacum, sarsaparilla, are not entirely void of utility ; light can be thrown upon the history of blennorrhagia by the observations of those who have preceded us. Without doubt, the spirit of charlatanism and of speculation have left too frequent traces of their passage, but you will often find the marks of judicious minds, of a true scientific tendency, and of praiseworthy efforts to arrive at a classification and a doctrine. These works, if they had no other interest than that of giving the ideas and opinions of past times, would not merit the disdain, in my opinion unjust, which some have wished to throw upon them.

I shall say the same of modern observers. The critic, I am aware, and I think that I have proved it, finds frequent opportunities to exercise his skill upon their works. But is that saying that we should hold them of no account? Far from me this unjust thought! On the contrary, I hold in great estimation the works of Bell, of J. Hunter, and of Swediaur; the time has come to render complete justice to Cullerier, to M. Lagneau especially, whose reputation was justly popular; in fine, to all those industrious and intelligent laborers in our science, who by conscientious studies have with difficulty opened the road in which we can freely march.

Would I be unjust towards my cotemporaries? Heaven forbid, my friend! Whatever may be our differences, it is with pleasure and spontaneously, that I render the most sincere homage to the works of MM. Baumes, Gibert, Cazenave, Cullerier neveu, Bottex, Ratier, Puche, Diday, Reynaud, Payan, Lafont Gouzi, Venot, in France; of Wallace, Carmichael, Babington, and of my pupils Acton and Meric in England; Thiry, Herion, in Belgium; to the remarkable publications of laborious Germany and industrious Italy.

I feel no sentiments of injustice or of hatred either towards the past or towards the present. You will excuse me from declaring this very distinctly before entering upon my subject. I explicitly say that I do not partake in any way the opinion of those unreasonable critics to whom ancient and modern syphilographic literature is but trash, unworthy of attention. I believe, on the contrary, that this branch of pathology is as fertile as any other in useful works, and in valuable researches.

However, the labors of ancients and moderns could not preserve this portion of our science from the general revolution brought upon medicine by the physiological doctrine. The school of Broussais, in blotting out the past, had again questioned everything. Was there a syhilitic virus? The verole, did it exist? You know how physiologism resolved these questions. The greatest confusion reigned in the science, and was introduced into the publications of the times. Doubt was everywhere, certainty nowhere.

It was at this time, that having become by "Concours" surgeon of the central bureau of hospitals, chance caused me to enter the hospital "du Midi." There I encountered a man, honest and loyal, a practitioner earnest and strict, M. Cullerier, who, abandoning the traditions of family, so to speak, took upon himself to doubt his own observations, and appeared no longer to credit what he had seen.

Everywhere doubt had taken the place of belief. The cause of syphilis was doubted, its effects also, and, in consequence, its therapeutics.

And observe what they called the modern doctrine was presented surrounded by much scientific display. M. Richond des Brus had written an enormous book filled entirely with facts; M. Desruelles supported new ideas upon statistics, which passed for being indisputable; all exerted themselves from the desire they had to oppose the speciality of the disease, and the specificity of the remedy.

History was brought largely under contribution by a very learned writer of our century, who in one of the most remarkable works of our time, amused himself with taking the observers "corps à corps," and placing them in opposition to each other. An easy triumph, if the critic, in a severe and impartial analysis, does not know how to establish a marked difference between the author's own ideas, those which result from his researches, from his own observations, and those which he draws from the scientific medium of his day. The former are useful materials and worthy of preservation, the latter constitute the prejudices of the epoch, and have no historical value. Jourdan did not make this distinction; it sufficed for him to combat the speciality of syphilis, to show the confusion existing in the contradictory opinions of our predecessors, and this he did with a profuseness of learning which would have been extolled in a sounder critic.

Such, then, was the state of minds and of science when I entered the Hospital "du Midi." For some there was a destroyed edifice to rebuild; for others, at least, it was to be consolidated.

What was especially necessary was to take up again the study of the cause of syphilis.

Is there a special cause, a virus ? or do venereal affections spring from a common cause ?

For this research and study, two modes of investigation were offered to me.

The first was the simple observation of phenomena, such observation as our predecessors had practised, and which had conducted them to opinions so different ; to observations similar to those of Devergie, analogous to facts already reported by Vigaroux, by Blegny, &c. ; to that observation, for example, relative to three officers, who had connection with the same young female suffering from a discharge, and who all three found themselves infected : the one with an urethritis, the second with a chancre, and the third with vegetations. It is true that Devergie has deprived history of a very trifling information—that of the precise state in which he found this young woman, whom he had not examined with the *speculum*.

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

The second mode satisfied my mind better ; besides it was more in conformity with the demands of modern science ; it seemed to open a sure way to study, and to conduct to incontestable results. I mean *experimentation*.

I proposed to myself the following obligations :

To trace the cause of syphilis to a known source ;

To place it upon a region visible and easy to observe ;

To note the effects.

You see, experimentation alone could fulfil these conditions.

But already experimentation had been consulted, and by means of it contradictory conclusions had been arrived at. When J. Hunter said yes—Carron, Bru, Jourdan, Devergie and M. Desruelles said no. To what could such different conclusions be owing, after the employment of the same method of investigation? I did not know at that time, but I have learned since. What my reason then convinced me, was, that experimentation, well and accurately made, ought to conduct to precise results, and that the differences of experimenters should not discourage me.

These researches were difficult and delicate. Conviction was necessary, and, I do not hesitate to say courage, to undertake them. It was necessary to be sure of thoroughly appreciating the conditions in which I was about to act; it was necessary to aid myself by antecedent experimentations; it was especially necessary to support myself upon the purity of my intentions, and upon the testimony of my conscience.

I was not contented, in fact, with the great name of Hunter, with the experimenters cited by Bell, with the work of Hernandez, although crowned by the Academy of Besançon; with the authority of Percy, and other great names as commendable; but I wished to study the question in itself, to place myself in the condition of a true inventor, in order to assume all the responsibility of the results.

How was it necessary to proceed to this experimentation?

I could inoculate a healthy indidual from a patient.

I could experiment upon the patient himself.

The first mode, that is, the inoculation of a healthy individual from a patient, appeared to me one that should be always rejected by the physician. I do not think that we have the right to make such experiments. Not only the physician cannot make use of his natural authority to induce an individual to undergo experiments of this nature, but I think the physician ought to resist the wishes of those, who seduced by a generous devotion, desire voluntarily to expose themselves to the risk of experimentation. I do not cast any blame upon those who have acted differently. I repeat, only, that, for myself, I did not wish to proceed in this way; the experimentation upon the patient himself remained.

Would this offer inconveniences and dangers for the patient?

In case it did not, would it conduct to conclusive results?

Here is what history, observation and experience taught me in this respect.

It was generally admitted that a first contagion would not prevent a second, and the old proverb of "*verole sur verole*" had yet all its authority. We now know what this means.

As to the inconveniences and dangers, we see every day that it is rare that the primary symptoms are isolated, that they multiply themselves with great rapidity, and that, to speak explicitly, the gravity of the disease is not in relation to the number of these symptoms.

Thus, in order to throw light upon such an important question of etiology and of practice, art could, without inconvenience, do what nature constantly does.

A much more important question presented itself here. The grave and consecutive symptoms of infection being feared, ought they to be in accordance with the number of the primary lesions?

Strict observation, and the clinical observation of all times, has proved and proves every day, that the constitutional verole is not in ratio with the number of primary symptoms *existing and developed at the same time.*

One symptom more does not add any more chance of infection—*if we know how to direct the experimentation.*

The question of surface remained, to know if an extensive ulceration exposes more to a general infection than an ulceration of small size. Well, here again observation has shown that a greater or less extent of primary ulceration has no influence upon the production of consecutive symptoms. A very small chancre exposes just as much to a general infection as a very extensive one; and, *vice versâ*, a large ulceration exposes neither more nor less than a small one.

In fine, the question of the seat of the ulceration remained, of the place of election for experimental inoculations. It had been said by Boerhaave, among others, that venereal symptoms contracted in other ways than by the genital organs, presented a very grave character; but clinical observation proved to me, and it has since shown me, that this opinion was erroneous.

I well know that upon this point a great noise has been made about diseases contracted by physicians, by midwives, in consequence of examinations, of wounds, &c. There are very good reasons, but I do not wish to point them out here, why these accidents should give rise to a great commotion. What I can say without injuring any rules of propriety, is, that medical men to whom these accidents happen, have no motive to conceal them, while others attacked by syphilis have always strong motives to keep quiet.

I remained, then, convinced that the seat of ulceration could have no unfavorable influence upon the production of consecutive symptoms, but that it could even diminish or annihilate certain grave consequences, such as the production of buboes. Thus, observation had already proved that the primary chancres of the thigh were almost never followed by enlarged glands, and in fact in my numerous experiments, I have never seen enlargement of the glands follow from the punctures of inoculation upon the thigh.

Thus, my friend, from history, the clinical observations of all times, from experimenters who had preceded me, from the testimony of my own conscience strictly interrogated, I arrived at this encouraging conclusion, in experimenting upon the patient himself:

I did not communicate another disease;

I did not increase the gravity of the affection by which he was already attacked;

I did not exposé him more to the chances of a consecutive infection.

These first and capital conditions being ascertained, it was necessary to search out those which offered to science and art all the security to be desired.

An explanation upon this point will be the subject of my second letter.

Yours, &c. RICORD.

SECOND LETTER.

MY DEAR FRIEND,—I am not writing a didactic work; I should like much to do so, but you know that at this moment I cannot. I address you some letters familiarly written, and for which I ask all the privileges of the epistolary form—that is to say, freedom of style and spontaneousness of thought. Therefore, what I have not said in my preceding letter, I shall say unceremoniously in this, without a too rigid adherence to plan, method, and other restraints of composition, elsewhere so useful.

In order that my first letter should be complete in my rapid sketch of the attempts made at experimentation, I ought not to omit to recall the fact of the attempts at inoculation of syphilis from man upon animals. Either to avoid the inconveniences which could result from the inoculation practised upon man himself, or to resolve the curious problem of the transmission of syphilis to animals, Hunter and Turnbull had already attempted in vain this inoculation from man to animals. I have repeated all those experiments, and have arrived at the same negative results. However, lately a young and industrious fellow-laborer, M.

Auzias Turenne, has repeated these experiments, has varied them, has employed other methods than those which were known, and has thought that he has arrived at the experimental demonstration of the transmissibility of syphilis from man to certain animals. It was my duty, then, to renew these experiments, and I was convinced anew that syphilis was decidedly not communicable to animals, and that the facts as stated by M. Auzias were illusory. M. Cullerier, at the Hospital "de Lourcine," has studied this subject with much care, and has arrived at the same conclusions as myself. My colleague, M. Vidal (de Cassis), has experimented in his turn, with I believe the same results.

The direct observation, the experimentation upon the patient himself, were then the only sources to which I could have recourse; to these alone, then, I resolved to apply myself.

It was necessary, first, to seek a sure source from which I could draw the principle, towards the search of which I wished to direct all my investigations. One could no longer rely upon the stories of patients; it was necessary, also, to avoid the objections justly brought against the experiments of Hunter and of Harrison, against the facts stated by Bell, against the experiments of Hernandez; and for this purpose, I first endeavored to well ascertain the state of the tissues from which I took the principle reputed specific.

It was no longer enough, as Petronius formerly said, that a woman should be considered *diseased;* it would no longer do, to take at hazard a morbid secretion coming from the genital organs of the woman, and to make of it, according to the picturesque expression of Alexander Benedictus, a venereal dye, throwing a uniform color upon all the symptoms which could result from it. No, the scientific tendencies of the minds of my day, and the demands of my own conscience, required of me the employment of a more authentic method and of strict proceedings.

I do not wish to lay stress upon the facility with which effects are drawn from the cause. But who would not be surprised, that in a question like that of venereal maladies, where ignorance and *fraud*, according to the expressions of Hunter, are such frequent sources of error—that in a disease which above all, and almost always, is a flagrant proof of immorality, the observers, even the most judicious, should so often trust to the reports of patients, and invoke without ceasing the moral worth of the testimony.

The testimony! under such circumstances, is there anything more deceptive? and especially as regards women? Let me cite to you two little examples, where you will see one of the most strict observers caught in the snare of feminine testimony.

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

"A married woman was taken with the ordinary symptoms of gonorrhœa, which much surprised her, as her husband was free from all disease. However, the husband having been questioned, confessed that he had had relations with a suspicious woman, about eight days before his wife perceived herself diseased, but he positively affirmed that he had no discharge, nor any morbid sensation, and certainly he offered no symptoms of disease. At the end of four days, that is to say, about fifteen days after the impure connection, and one week after the time when he should have communicated the disease to his wife, a gonorrhœal discharge manifested itself in him.

"A traveller exposed himself to the risks of a syphilitic infection, and arrived home at the end of three days. About four days after his arrival, his wife was attacked with gonorrhœa. It was not till ten days after the infection that he perceived, for the first time, a discharge, and that he was attacked by the other symptoms of gonorrhœa."—(John Hunter's complete works, vol. xi., page 167. Notes by Babington.)

If, in presence of similar facts, Babington had not sought to obtain more complete confessions (there are some confessions that women never make, even, as I have had the opportunity of too often seeing, under the fear of the greatest dangers), but had assured himself by a rigid inspection of the true state of things, he would have seen that in these cases the infecting cause was not in the genital organs of the candid husbands.

It was no longer possible, then, to think of basing any pathological truth whatever, in syphilis, upon the morality of the testimony of the patients. I had no longer confidence in the doctrines and in the facts based upon recitals of this kind.

It was necessary to be removed from the mysteries of the " *alcove*," to bring to the light of experimentation the principle which I wished to find.

This principle—where ought I first to seek for it?

At its source; that is to say, in the genital organs of the woman, in their external portions as well as in their deepest folds.

Chance was propitious for me. The Hospital "du Midi" then received the unhappy beings that the dispensary sent there.

Here you will permit me to recall, my friend, that before my entrance into the Hospital du Midi, the manner of examining a woman consisted in making her sit upon the border of a chair, in separating the external genital organs, and if no lesion of the tissue was found, every morbid secretion coming from higher up, was invariably considered as a blennorrhagic discharge. At the circle of the vulva my predecessors appeared to have placed the columns of Hercules of chancre.

I could not, nor ought I, to have been satisfied with this superficial and incomplete examination. We were at no great distance from the time when M. Recamier had so fortunately exhumed the *speculum* from the surgical armentarium. You are aware of the happy applications that this celebrated practitioner had made of it, in the diagnosis of diseases of the uterus. But this valuable instrument had not as yet been applied to the diagnosis of syphilitic diseases; its employment, even in these cases, appeared and was reported to be contra-indicated. I did not pay any attention to this widely-spread opinion. I made a general use, on the contrary, of the speculum upon all the women in my wards.

I know not if posterity will partake of the opinion of one of my learned critics, who reduced to a very small compass what I had to do in syphilopathy. However, my friend, when I call to mind the profound obscurity which enveloped the diagnosis of syphilitic diseases before the application of the speculum—when I compare the embarrassment of practitioners of that epoch in settling upon their opinion, with the truly wonderful facility of modern practitioners in giving an undeniable diagnosis; when the recollection of all the services that the speculum has already rendered to this part of practice comes to my mind, I think, that should my participation in its progress be thus limited, this opinion might appear rather too severe.

The employment of the speculum permitted me to examine with great care all the surfaces venereally affected, and to ascertain with precision the condition of the tissues which furnished the secretions.

These conditions established, I had to study all the symptoms reputed venereal, and comparatively with other morbid secretions.

I commenced with blennorrhagia.

You understand, that I must suppose the state of the question, at the time when I undertook my experiments concerning blennorrhagia, to be perfectly understood by my readers. Once more, I do not here write volumes with a complete history, but a simple and concise exposition of facts which belong to me.

I sought to solve by experimentation that problem already differently solved, by the observations which you know—Does blennorrhagia recognize a specific cause?

Hunter had taught that the pus of a chancre inoculated produced chancre. If blennorrhagia recognizes a specific cause, said I to myself, the muco-pus which it secretes, being inoculated, will produce without doubt phenomena similar to those which pus coming from a chancre produces.

But in order to well ascertain the result, to isolate it from every complication, and from every cause of error, it was necessary first to inoculate the muco-pus coming from perfectly simple blennorrhagias; it was necessary to take this muco-pus from tissues completely free from ulceration; and you see how valuable the employment of the speculum was to me. Without it, these experiments were not possible.

Now these first experiments, made in great number, and a long time perseveringly continued, conducted me to this fundamental result, which I here give in the form of a proposition.

PROPOSITION.

Every time that the muco-pus has been taken from a mucous surface not ulcerated, the results of the inoculation have been negative.

All experimenters who have followed me in this course have arrived at the same conclusion; and this, whatever has been the period of the blennorrhagia in which the experimentation has been made.

Thus, it is with great surprise that I have read in your Journal the following passage, where M. Vidal, in his *letters upon syphilitic inoculation*, reproaches inoculation for being very often fruitless in the question of blennorrhagia; "In fact," says my learned colleague, "a distinguished Interne, M. Bigot, has tried, under the observation of M. Puche, physician at the Hospital du Midi, sixty-eight inoculations with muco-pus coming from the urethra, and these sixty-eight inoculations have been followed by no result!" I am astonished at the surprise of M. Vidal. These sixty-eight negative inoculations conform entirely to the facts which I have before advanced; they confirm and corroborate my opinion upon the rarity of *syphilitic* blennorrhagia; and when my opposer asks you—"Do you believe that of these sixty-eight blennorrhagia there were none, where virus was present, no one that contained the seeds of a verole?" Answer him confidently, no; and for this reason, that the inoculation has been negative.

A logician as skilful and as exact as M. Vidal, could not be prevented from perceiving that the results of experimentation, upon whatever subject exercised, are either positive or negative, but that scientifically speaking, the negative results are no less valuable than the positive. The inoculation of vaccine does not give rise to any phenomenon upon those subjects who have already had the variola; is that saying that the negative result is without importance and without consequences?

But we shall soon see how much value and force these negative results have derived from the positive results of inoculation. I notice, in passing, a first objection which will at a later period find its complete refutation. Some writers on syphilis have thought with Hunter that blennorrhagia was a form of syphilis peculiar to mucous membranes. I confine myself for the moment to remarking that the experiments before indicated destroy entirely this opinion; we shall see later that the virulent virus of chancre, placed upon a mucous surface, produces there, in every respect, the chancre.

From experiments shown, I shall draw this conclusion.

CONCLUSION.

The blennorrhagia, the muco-pus of which being inoculated produces no result, does not recognize the syphilitic virus as cause.

This conclusion, as you know, has given rise to numerous and grave objections. But I fear that you cannot to-day afford me sufficient room to undertake the refutation and exposition of these. This will be, with your permission, the subject of my third letter. Yours, &c.

RICORD.

THIRD LETTER.

My Dear Friend,—The conclusion which terminates my last letter,—*The blennorrhagia, the muco-pus of which being inoculated produces no result, does not recognize the syphilitic virus as cause,*—this conclusion, deduced from undeniable facts, re-places the history of blennorrhagia exactly where it has been transmitted to us in the book of Leviticus. Old as man, older than he, for animals created before him are subject to blennorrhagia, and not to the verole, this disease in its simple state has nothing in common with the syphilitic infection.

In spite of those, who, since Paracelsus, Bethencourt and Fallopus, have wished to make of blennorrhagia, not symptomatic of chancre, a new disease identical with syphilis, the researches that I have made, corroborating the descriptions so exact of Alexander Benedictus and of Cataneus, have given to the doctrines of Balfour, of Todus, and of Duncan, the value and the solidity that Bell would have himself given them, if he could have explained the facts supposed to be exceptional, as we can explain them at the present day.

But blennorrhagia, as I understand it, absolutely different from syphilis in its causes, in its form, and in its consequences, does it depend upon a specific virus?

There is no reason why we should not admit a special cause which has the power to produce blennorrhagia and its consequences specifically and constantly. Nothing is more apt, in fact, to determine a blennorrhagia, than the muco-pus furnished by certain inflamed mucous surfaces.

But when we review strictly and with the most rigorous observation, the causes determining *the best characterized blennorrhagia*, we are forced to see and to confess that the blennorrhagic virus ordinarily has no share in it. Nothing is more common than to find women who have communicated the most severe and obstinate blennorrhagias, with the most varied and the most serious *blennorrhagic* consequences, and who were only affected with uterine catarrhs, sometimes scarcely purulent. Quite often the menstrual flow appears to have been the only cause of the communicated disease. In a great number of cases, in fact, we find no cause save some errors in diet, fatigue, excess in sexual relations, the use of certain drinks, such as beer, or of certain articles of food, such as asparagus. From these circumstances springs that frequency of belief, very often correct, that a gonorrhœa has been caught from a woman perfectly healthy.

I am certainly aware of all the causes of error upon this point, and I pretend to say that no one is more careful to guard against the frauds of every kind strewn in the pathway of the observer than I am. It is, therefore, with full knowledge of such causes that I sustain this proposition:

PROPOSITION.

Women frequently give blennorrhagia without having it.

Blennorrhagia, such as some individuals persist in understanding it, that is to say, as the consequence of a contagion, is as rare in women as it is common among men. I do not believe that I advance too much when I say that women give twenty gonorrhœas where they contract one. And this is easy to understand, for women so subject to non-syphilitic discharges from the genital organs, are the most frequent source of those discharges which in man cannot be considered as an effect of contagion.

It has been impossible for me to consider as serious the doctrine of my learned colleague, M. Cazenave, who very readily admits that many women under the influence of chronic utero-vaginal catarrhs, can have sexual relations without communicating any thing, provided that they are not "*echauffées*" to the degree of virulence, or that they are not raised, so to speak, to a red heat.

Is it not more easily understood, and is it not more rational to say, that with a less degree of excitement, the secretions are less irritating, and that the being habituated to these secretions, would produce an immunity for some persons, and a sort of acclimation.

It is thus, as I have frequently seen, that a married woman can cohabit with her husband without communicating any thing; but should a lover come, this last contracts a blennorrhagia.

The husband was acclimated, the lover was not.

When one studies blennorrhagia without prejudice, without preconceived ideas, he is forced to acknowledge that it is often produced under the influence of most of the causes which give rise to the inflammation of other mucous surfaces.

The experience of Swediaur is here to prove this. This observer injected volatile alkali into the urethra, and produced a blennorrhagia. Does this experience show that a blennorrhagia can be always produced, and at will, by irritating injections? No, certainly not, no more than a coryza or an ophthalmia could be produced by the same means. For a blennorrhagia, as for every other inflammation, the pre-existence of predisposition, that great unknown influence which dominates over all pathology, is necessary. This is proved by the fact that a blennorrhagia is not always taken in those same conditions in which it is the most evidently communicable. Without this fortunate immunity which the absence of predisposition gives, blennorrhagia, already very common, would be still more so.

An experience of twenty years has taught me, and permits me to affirm, that excepting blennorrhagic discharges symptomatic of chancre, it is often perfectly impossible to recognize the cause of a blennorrhagia.

I know that many of my colleagues obstinately refuse to admit this opinion; every blennorrhagia awakens in them the idea of syphilis, and their therapeutic prescriptions are but the logical result of their prejudices.

Here, my friend, I ought to make you a confession, and I shall make it publicly. This persistence of some of my honored and learned colleagues, to always consider and treat blennorrhagia as a symptom of a syphilitic nature, has often astonished me. Thus I have often had recourse to a stratagem, not to satisfy a frivolous curiosity, much less to yield to a culpable, slanderous motive, but to enlighten and re-assure myself, the avowal of which I wish to make with all the reserve and the delicacy that I owe my honorable brethren.

It was under the following circumstances:—A man presented himself at my consultation with well-marked blennorrhagia. He stated to me that he had had relations with only one woman, and that this woman was his wife or his mistress. This man was uneasy or alarmed. He brought with him the woman the cause of his trouble, and she protesting her innocence, together with the patient, supplicated me to submit her to the most rigorous examination. This examination, made with all the attention and care of which I was capable, showed to me the sexual organs of this woman in a perfectly healthy state. There was nothing, absolutely nothing, in the deepest folds of those organs which could explain the blennorrhagia of that man. I begged the woman to pass into a neighboring room, and, alone with the patient, I made use of all the means possible, of which I spare you the details, to arrive at this certainty; that the patient had had sexual connections only with this woman; it was in these alone that he could have contracted the disease which he had. I re-assured the husband or lover; I acquitted the wife or the mistress; but I begged them both to be accomplices in a little stratagem, which it remains for me to explain.

I sent them both, separately of course, to such of my learned colleagues as I knew to be my direct antagonists upon the question of blennorrhagia. I said to the patient, ask plainly this question: is my blennorrhagia syphilitic? I said to the woman, ask distinctly: could I give a blennorrhagia to a man?

The couple returned to me, the man with a diagnosis thus written—"*syphilitic blennorrhagia ;*" the treatment followed *ad hoc*. The woman returned with this—"*the perfectly healthy state of the organs permits me to affirm that madam could not communicate a malady which she has not.*"

It is not an isolated fact that I point out to you, my friend; this experiment I have often renewed, and sufficiently often, with some variations, to corroborate my convictions and to reëstablish my opinion.*

What do these facts signify? That the cause of a blennorrhagia cannot be always known; that this disease can be produced by causes common to all inflammations, if there is a predisposition; but that the most special cause of blennorrhagia is the muco-pus furnished by the inflamed genito-urinary surfaces.

This manner of regarding it appears to me more rational than that which would attribute the blennorrhagia called venereal to a sort of half virus imagined by our very

* There are some facts still more curious than those relating to blennorhagia contracted with healthy women. A case analogous to the following has not been presented, perhaps, to the notice of M. Ricord, but of its authenticity it is not possible for me to raise the least doubt.

A man of thirty years of age, a physician, had been continent for more than six weeks, and his last sexual relations were not of a suspicious character. A fortuitous circumstance permitted him to pass almost an entire day in company with a young woman whom he loved. From ten o'clock in the morning until seven o'clock in the evening, he made vain efforts to overcome the resistance of this woman, whose virtue did not yield. But during all this time, this physician remained in a constant state of excitement. Three days after, he was taken with blennorrhagia of the most violent and painful kind, which lasted forty days. Most assuredly here is the form of a blennorrhagia not syphilitic. —*Note of the Editor.*

learned brother and ingenious writer on syphilis, M. Baumès. To this practitioner, blennorrhagia is a degenerated kind of chancre; it can give rise to a constitutional syphilitic infection, more feeble, however, than that produced by chancre, but without being able to re-produce this latter by means of contagion or inoculation. "One can then foresee," adds M. Baumès, "the greatest similarity between the constitutional symptoms which are the consequence of both of these diseases; and in fact, experience proves that the difference between these symptoms lies not in their nature, but only in their degree of intensity, in their gravity, and in their situation, which after blennorrhagia, extends generally to fewer tissues, and to a smaller number of organs, than after chancre."—*Baumès, Précis théorique et pratique sur les maladies vénériennes*, tom. i., page 529.

Here is a true half-way doctrine. This mere theory is neither justified by facts, by observation nor by experience; only one condition is wanting to it—the proofs.

Hitherto, then, I have believed, and it is certainly my present opinion, that simple blennorhagia is entirely different from syphilis as regards the causes which can produce it.

But it has been objected to this, that the pus of chancre, that is to say the syphilitic virus, can produce blennorrhagia. This opinion is very old; it has been sustained since the appearance of the verole in Europe, and it can with reason be still sustained. But what does this mean? Are the observations of the ancients to be relied upon? They are incomplete and insufficient; it is impossible with these to proceed scientifically from the effect to the cause.

Would you appeal to experiments similar to those of Harrison, who drew his conclusions from the production of a blennorrhagia by the introduction into the urethra of pus furnished by a chancre, without knowing what it had physically brought about? No, we shall arrive more simply and more logically at the conclusion of the possibility of the production of a non-virulent blennorrhagia by the pus of a chancre, by considering this pus as having the power to act in the manner of simple irritants. A woman having chancres at the inoculable period, could thus produce a blennorrhagia in a man which could not inoculate. We can thus understand the observations of Swediaur and others, supposing that they had not committed some error in diagnosis, inasmuch as these observers made use of neither speculum, nor inoculation—observations, which prove that men affected with chancre, have communicated blennorrhagia to women.

Here is what clinical observation teaches, and what experiment can demonstrate. It is not rare to see patients with a chancre of the gland or of the prepuce, successively affected with balanitis or with balano-posthitis, determined by the irritating action of the pus of the chancre. But while the chancre furnishes pus inoculable, the pus furnished by the balano-posthitis is not so. (We shall see later, that in order that the virulent pus should act specifically, some conditions are necessary which are not always met with.)

Faithful to my first conclusion, reducing to their just value these first objections, I affirm that when Harrison produced blennorrhagia with the pus of the chancre, either this pus acted after the manner of simple irritants, or it produced an urethral chancre which was not verified. We shall see also later, that when Hunter produced a chancre with pus supposed to be blennorrhagic, it was with the product of a true urethral chancre that he had operated.

But if inoculation has proved that the cause or causes of blennorrhagia, *whatever may be its seat* in the two sexes, differ from the specific principle, from the virus which chancre *inevitably* produces, the consequences of blennorrhagia ought always to differ from those of chancre; and yet many constitutional veroles are attributed to blennorrhagia.

These are the questions which will make the subject of my next letter. We shall see, also, if it is possible to establish a differential diagnosis between two affections which some wish systematically to confound.

You will permit me first to say a word upon the incubation of blennorrhagia. Yours, &c. RICORD.

FOURTH LETTER.

MY DEAR FRIEND,—As I promised, I shall say a few words upon the incubation of blennorrhagia.

Incubation has been made a condition of virulence. Every virulent disease ought to present a period of incubation. Thus, those who admit that blennorrhagia is the product of a virus, admit equally that this virus does not produce its first effects till after a longer or shorter period of incubation.

I say a longer or shorter period, and it is not without reason. Authors have allowed a most convenient period for the incubation of blennorrhagia, as well as for syphilis properly called. The term of the incubation has been fixed between some hours (Hunter and others), and fifty and some days (Bell). What shall I say? MM. Cullerier and Ratier have reported the history of an incubation which lasted during five months. Assuredly a very elastic incubation!

You know that it is far different in the virulent diseases where the incubation is incontestable. The limits of the period of incubation can be more accurately fixed in the variola, in vaccinia, in scarlatina, in measles, and in hydrophobia. The fine works of M. Aubert Roche have even told us the certain limits of the incubation of the plague, which never exceeds eight days. For blennorrhagia, it is a far different thing, as you will see; here there are no certain limits.

What is, then, this incubation of blennorrhagia, which they have made me very recently again deny? We must understand this matter; it is clearly a question of words. I do not deny the evidence; and consequently I do not deny that between the action of the cause, and the appearance of the first phenomena of blennorrhagia, there is a longer or shorter period; but is there present an incubation properly called, an incubation similar to that of the variolic or vaccine virus? I contest this, and I explain that longer or shorter time, which exists between the action of the cause and the appearance of the phenomena, by the disposition and by the particular susceptibility of the tissues which have undergone the influence of the cause. There is no more incubation present in this case, than there is between the action of an exposure of the feet to cold, and the appearance of a coryza. One does not blow muco-pus immediately from the nose after such exposure to cold; there exists a certain period between these two actions. Do you call this period the incubation of the coryza? Why, then, make use of a similar expression for blennorrhagia?

In those cases where blennorrhagia does not appear till a long time after one is exposed to the suspected cause which produced it, is it not more rational to admit another cause which remains unknown, than that pretended incubation which nothing explains, nothing justifies? Is it not so in almost all the inflammations? Can you always go back to the direct cause of a pneumonia, of an arthritis, of a phlegmon? Without doubt, in man, the sexual relations are the most direct source of blennorrhagia; but we should fall into strange errors, if we wished to refer all blennorrhagias to a virulent cause. I could give you some very singular examples which prove the contrary, but I refer the reader to the interesting note with which you have accompanied my preceding letter.

From this exclusive manner of considering the etiology of blennorrhagia, there results often, in practice, a singular manner of interpreting facts. A man affected with blennorrhagia has had connection with several women; he hastens to make a sort of moral choice among these women, and by this process of elimination he happens to fall often upon the most innocent. The application of the law of suspicion in this way has caused strange errors to be committed, of which I have often been witness.

Let us then conclude upon this point, that the effects of blennorrhagia can follow at some distance from the cause which produces them, but that nothing proves that the period which exists between the action of the cause and the appearance of the morbid phenomena, is the result of a true virulent incubation.

I should prefer, my friend, not to make too frequent digressions from my programme, but how can I avoid deciding incidental questions when they present themselves beneath my pen? Such is that of the specific seat of blennorrhagia. You know that the question of this seat has been much agitated. In man, it has been made to travel from behind forward, from forward backward; to advance or to retreat, at the will of the fertile imagination of writers upon syphilis. From the spermatic passages, passing successively by the glands of Cowper, the fossa navicularis and the follicles of Morgagni, the seat of blennorrhagia has travelled a good deal. It is true that Bell, in establishing different degrees in blennorrhagia, has made its seat retrograde from before backward. But it is not with these questions, so well known, that I wish to detain you. I will call your attention, however, to a singular prepossession of Hunter. This great observer admitted, as you know, that a virulent blennorrhagia was identical with chancre; he placed the seat of it in the fossa navicularis; but he inquires if the inflammation which propagates itself by degrees towards the posterior portions of the urethra, continues to be virulent beyond the fossa navicularis! We must confess that the genius of Hunter yielded strangely to the spirit of system. Besides, in studying Hunter, we see his observing genius constantly in contest with his theory of blennorrhagia. He started with a false idea; facts come constantly to prove it to him, but theory is present to obscure his intellect, and in place of allowing his theory to be overcome by facts, he endeavors, on the contrary, to make facts agree with it—an excellent example of the dangers of pre-conceived and systematic ideas in the cultivation of the science of observation.

In the female, Graff placed the seat of the virulent blennorrhagia in the follicles in the neighborhood of the urethra. One of our brother physicians of Bordeaux, who died a few years since, Moulinié, thought he had seen in the glands of the vulva (so well described by Bartholin, and of which Boerhaave has traced the pathological history, resumed and completed in our day by M. Hugenier) a sort of organ of virulence in a blennorrhagic point of view.

In the midst of all these opinions, strict observation shows that those portions of mucous surfaces the most exposed, are those which are the most easily affected. Nevertheless, we must allow that the mucous surface of the urethra in the two sexes is more often affected after sexual intercourse than the other mucous surfaces of the genital organs. This fact is an argument for the partisans of the virulent contagion. I will corroborate it, if they wish, by this proposition, which appears incontestable, that a woman attacked by blennorrhagia of the urethra can be considered as having most commonly contracted it from a man suffering from blennorrhagia; and you see that this proposition might have its importance in legal medicine. Thus, for me, I should be ready to admit that a woman in whom I discovered a blennorrhagia of the urethra, had taken it from a man. But does this fact come in aid of the existence of a virulent contagion? No, and I explain it by this other fact, alone true and incontestable, that pus furnished by the urethra is the most irritating of all pus for certain mucous surfaces.

While certain writers on syphilis contest the existence of blennorrhagia of the urethra in the female, others admit of a blennorrhagia in her, only when it has its seat in the urethra. These two extreme opinions are erroneous. Observation has led me to admit every variety of blennorrhagia upon all mucous surfaces.

Whilst I am on this point, will you permit me to disembarrass myself of some other incidental questions relative to blennorrhagia? I shall proceed more freely and more rapidly afterwards, on the great questions which remain for me to treat of.

If I examine the lesions of tissue which blennorrhagia produces, whatever may be the mucous coat affected, I find nothing that simple inflammation cannot produce. There is sometimes a slight erythematous condition without secretion. This is *the dry blennorrhagia* of some writers, a denomination ridiculous and absurd, introduced into the writings upon syphilis; consequently we can admire the persevering efforts of M. Piorry to bring about a reform in the nomenclature. Sometimes we have to do with a mucous catarrhal element, and with all its products in different conditions—mucous, mucoso-purulent; in fine, there are some true phlegmonous complications which we meet with, from which result in the male urethra, blennorrhagia accompanied with chordée, and the quite frequent production of abscess upon the course of the canal.

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

Are the consequences of blennorrhagia comparable to those of syphilis? It has been said so, but it has not been proved. There are some analogies between them, without doubt, but some notable differences also!

Thus one of the first symptoms, which blennorrhagia can produce, and which resembles one of those produced by syphilis, is bubo. But in the first place, enlarged glands are infinitely more rare as the consequence of blennorrhagia, than of chancre. In the next place, the bubo is never met with in the two sexes, except in blennorrhagia of the urethra, the other varieties never giving rise to enlarged glands. I well know that one of our fellow medical men of Belgium speaks of *buboes péri-auriculaires*, which ought to manifest themselves in blennorrhagia of the eye, but I must confess that I have yet to look for an example. In fine, the blennorrhagic bubo has this peculiarity, that, purely inflammatory, it has very little tendency to suppuration, and when this happens *it is never inoculable.*

Do you wish to follow out what blennorrhagia may commonly produce upon the two sexes? Take blennorrhagic ophthalmia, which never manifests itself except during *a blennorrhagia of the urethra;* in good faith, is it possible, unless we wish to confound everything, to establish the least comparison between this ophthalmia and syphilitic iritis?

With regard to blennorrhagic rheumatism, is it reasonable to establish the least difference between this affection and those produced by syphilis upon the osseous system? For example, is there anything in the world more dissimilar than blennorrhagic arthritis and exostosis?

What shall I say of the cutaneous affections, unless it is that I am profoundly astonished that some physicians have wished to discover a resemblance between the cutaneous affections produced by certain remedies employed in the treatment of blennorrhagia, and the special affections of the derma that syphilis produces. The entertaining a false doctrine previously has here produced some very strange confusions. Blennorrhagia, it has been said, produces cutaneous affections like the chancre; and the roseola which succeeds the use of copaiba and of cubebs has been cited as an example. I assure you that these cases of roseola appear only when these resins are given. They answer me—but they do not appear except when there is a blennorrhagia existing. I answer, in my turn, that copaiba and cubebs are only given when there is a blennorrhagia. I add, and this is important, that I have administered copaiba in cases of vesical catarrh, and I have often seen these exanthemata make their appearance.

But these *resinous* exanthemata have characteristics so marked, that with the strongest disposition in the world, it is impossible to confound them with genuine syphilitic exanthemata. They are developed generally with great rapidity; they are very vivid, of rubeolic form, or often connected with lichen urticarius; if they are not very confluent, they are grouped generally in the neighborhood of the articulations, and in the sense of extension, such as about the wrist, elbow, knee, instep, and around the ears; they are commonly accompanied with much itching, which is the contrary of syphilitic eruptions, and a most important condition; so that we can say of them—*sublatâ causâ tollitur effectus.* In fact, they rarely survive a week the cause that produced them.

These exanthemata bring to mind a curious fact, which I ask you the permission to relate in the form of an episode; it has also its instruction.

Two or three years since, one of our most distinguished brother physicians presented himself at my house very much frightened. Until now, said he to me, I have had faith in your doctrine, but I find it at fault, and in my own case; that is truly hard. So saying, he took off his clothes and said, "What is this?" showing his chest and back. I examined and said,

"That is a beautiful syphilitic roseola."

"Syphilitic, do you say; and are you very sure of it?"

"Perfectly sure."

"Ah, well, you convict yourself. I never had any other venereal affection in my life than a blennorrhagia, and that was twelve years ago."

"On your side are you very sure of that?"

"Just as sure as of my existence."

I examined my friend from head to foot, and having done so, I said to him gravely, and with a certain air of solemnity, "Friend, you have *recently* had a chancre upon the right hand, and the chancre was situated neither upon the thumb, nor upon the index finger, but upon one of the three last fingers."

"You are joking," said he. "I am joking so little," I added, "that you still carry a bubo."

And I made him feel, in fact, an axillary gland still enlarged.

Then my friend, recalling his thoughts, told me that some months before, he had attended and dressed a woman who had chancres; that an ulceration had come upon the middle finger, that he had not taken care of it, and that this ulceration had cicatrized.

There is the source of your roseola, said I, and act accordingly.

Finally, what physician at the present day could confound the blennorrhagic epididymitis with the syphilitic sarcocele? It is no longer possible, since the time of Bell, still less possible since the works of Astley Cooper, and since what I myself have done in this matter.

You will permit me to pass in silence the pretended tuberculous diathesis invented in Germany as a consequence of blennorrhagic virulence. The question of tubercles in general is already sufficiently obscure, without adding new darkness to it.

You see, my friend, that I approach at last the programme that I had traced out for myself. In my next letter I shall enter upon it resolutely.

4 Yours, &c. RICORD.

FIFTH LETTER.

My Dear Friend,—I promised to commence to-day the great questions to which the study of blennorrhagia gives rise. I shall endeavor to do honor to this important engagement; important it is, for, as I hope to be able to show, the point that I undertake to discuss at this moment may be considered as the key-stone to the syphilographic edifice.

All that I have thus far said upon blennorrhagia, relates to simple blennorrhagia, which may or may not be considered as the product of a specific virus, but a virus completely different from that which syphilis, properly called, produces.

However, this blennorrhagia, according to a great number of authors, can produce consecutive symptoms perfectly identical with those which chancre produces.

It is incontestable that a great number of patients, affected with constitutional syphilis, accuse blennorrhagia alone as the cause.

These patients are sometimes right. I do not deny the fact, but after having verified it, I do not confine myself to leaving it in a crude state, and to crying out emphatically, it is a fact, and then obstinately set myself against every explanation.

The entire question can be reduced to these terms: when a blennorrhagia has been the point of departure of a constitutional syphilis, has there not been something else besides what we have previously studied in blennorrhagia properly so called?

Experimentation has proved—and pathological anatomy has lent its aid to this proof—that the urethra, and the deep and concealed points of the other genital mucous surfaces, can be the seat of chancre, the necessary source of syphilitic affections.

It is for not having recognized the *concealed* chancre, that the doctrine of Balfour, of Tode, of Bell, and the great scaffold built upon the experiments of Hermandez, have very nearly given way.

With the doctrine of the existence of urethral or concealed chancre, the virulent blennorrhagia cannot be doubted ; it is identical with chancre, it is the chancre itself.

This idea is not new in science, and I am astonished that those who would take away from every one the credit of originality, have brought nothing against me as regards this. However, it is a long time since the ulcerations of the urethra were recognized. Mayerne, in the seventeenth century, attributed at that period the urethral blennorrhagia to pus produced by ulcers within the urethra, and gave to it the name of *pyrroia*. Many others still, whom I will not recall, have verified the presence of ulcerations in the urethra; but do you not consider it strange to see Swediaur, who sustains the identity of blennorhagia and of chancre, say precisely what cannot be denied, viz., that blennorrhagia is virulent when ulcerations exist in the urethra !

If in three autopsies of persons hung, who were affected with blennorrhagia, Hunter did not prove the presence of ulcerations in the urethra ; if in an autopsy of which M.

Philippe Boyer has given an account; if in some others still, nothing has been found, it is because simple blennorrhagia was alone present. I have shown to the Academy of Medicine, two specimens of pathological anatomy, the designs of which and the accompanying observations may be found in the *clinque iconographique* of the Venereal Hospital, and upon which MM. Cullerier and Lagneau have made a report. These specimens presented some chancres of the urethra at different depths, which previous to death had been recognized by inoculation.

Thus, inoculation first, and pathological anatomy afterwards, have proved, in an incontestable manner, the existence of chancres of the urethra. To tell the truth, no one denies this, even those who wish to ascribe to simple blennorrhagia the consequences of syphilis. The chancre concealed in the urethra is not, then, an hypothesis, but a fact proved as certainly as any other medical fact.

And yet, singular phenomenon! those even who have best studied the chancre of the urethra—those who, like M. Baumès, have been able to recognize it *at the depth of an inch* in the canal—when there is question of establishing the facts of its existence, love better to launch into the field of hypothesis, than to admit that which observation and good sense point out to them. Observe, in fact, M. Baumès and others, establishing, with a rare sagacity, the differences which exist between chancre and blennorrhagia, tracing with clearness their differential characteristics, and arriving, at the end of their comparison, at the conclusion of the identity of those two affections.

It is always, dear friend, the same contest between the logic of facts and the preconceived ideas, the results of which I have noticed even in the great mind of Hunter. I have again very recently observed singular manifestations of this, in a pamphlet, otherwise interesting, of M. Lafont Gouzy fils.

But here some serious objections present themselves.

"The existence of chancre in the urethra cannot explain all the cases of constitutional syphilis, which appear to have blennorrhagia as a point of departure."

"The number of urethral chancres is too small relatively to that of the constitutional veroles with blennorrhagia as antecedent."

"In fine, there are some cases of blennorrhagia in which it has been impossible to verify the urethral chancre, and which have been followed by constitutional symptoms.

Here I am going to greatly astonish my antagonists by making this concession; that all this is true. But you will see that this concession is only apparent; for I hasten to add: the explanations which have been given of these facts are not true.

It is very certain that relatively to the immense number of blennorrhagias which exist, the blennorrhagia symptomatic of concealed chancre in the urethra constitutes the exception. In fact, they say to me, with an appearance of reason, but how is it, then, that the number of cases of syphilis coming on after the pretended chancre of the urethra, should be almost in proportion with the veroles coming on after the external chancre? Here, my friend, I ask all your attention, not because I wish to be subtile or captious, but because the form of reasoning which I am forced to employ to answer this objection, itself very subtile and captious, has need of being followed in all its conditions.

Yes, the chancre concealed in the urethra is rare.

No, the number of veroles, the consequence of chancre concealed in the urethra, does not appear rare.

You are about to cry out, sophistry ; but hear me.

That chancre in the urethra is rare, is incontestable ; my experiments, those of my honorable colleague and friend, M. Puche, and those of many other observers, have proved it without reply. Do you wish that I establish a proportion ? I will do so with pleasure. Let us admit 1 in 1,000, which is, I am convinced, far greater than the reality.

Let there be, then, on one hand, 1 chancre of the urethra in 1,000 cases of simple blennorrhagia.

Do you recollect, on the other hand, how frequent and extended is blennorrhagia ? Do you recollect that Lisfranc, with perhaps a little exaggeration, said that out of 1,000 adults, he counted 800 who had had, who had then, or would have, blennorrhagia ?

However this may be, out of 1,000 cases of blennorrhagia, there are 999 of which you never hear mention, which will have had no unhappy consequences, against a solitary one, which will have determined the constitutional infection.

It is a small number, without doubt, but make your calculations upon the hundreds of thousands, upon entire populations, upon the population of Paris, for example, which numbers three to four hundred thousand adult men ; compute the number of blennorrhagias contracted in this great city; calculate for the concealed chancre only the small number of 1 out of 1,000, and you will still arrive at a sufficiently large number of blennorrhagias which would afterwards produce the verole.

Well, what happens in practice? That you only see in the hospitals and at the consultations of physicians, those patients in whom the syphilitic infection has been preceded by a blennorrhagia with a concealed chancre. A physician of a hospital devoted to these diseases could meet, in the course of his practice, with ten, twenty, thirty examples; but what is that in comparison with the number of simple cases without any unhappy consequences? But those patients who have no other antecedent than the blennorrhagia for their constitutional infection, arrest the attention of observers; the remembrance of them remains deeply fixed; their number, relatively small, increases in their imagination, and they do not fail to present this as a formidable objection to the doctrine of the non-identity of blennorrhagia and syphilis.

You see to what this objection is reduced; I hope that I have destroyed it. I am accused of founding an hypothesis with the concealed chancre, of establishing a system. However, I have proved the fact of its existence by means of pathological anatomy. I have deduced it also from my experiments in inoculation. Is it not true that blennorrhagia in the immense majority of cases is exempt from every consequence of syphilis? To what, then, can we attribute the affection when it comes on after blennorrhagia? I myself attribute it to concealed chancre! and my adversaries—to what do they attribute it? To a pretended identity, which the observation of every day, and great abundance of facts, incessantly contradict. And it is I whom they accuse of being systematic, I who raise a doctrine upon the basis of observation, of experimentation and of pathological anatomy. What then, are my adversaries, who, for the sole support of their doctrine, invoke only a rude fact, the interpretation of which reposes upon none of the elements required at the present day by the demands of science !

Believe, then, that it is my adversaries who launch themselves into the way of hypothesis, whilst I, on the contrary, strive to bring them back into the path of reality. You see now that it is easy to conciliate these two terms of my proposition :

Yes, the chancre concealed in the urethra is rare ; but the number of veroles, the consequence of chancre concealed in the urethra, does not appear to be small.

It does not appear small, because we see again those patients only who have been suffering from this concealed chancre ; but if a strict proportion could be established between the cases of blennorrhagia not followed by syphilitic symptoms, and those which have given rise to them, we should see that the last are proportionally very rare, and that this appearance of frequency is entirely illusory.

Moreover, in all cases where the constitutional verole has been referred to blennorrhagia, have all possible precautions been taken in order not to be led into error ? I do not believe this when I see that some are contented with a diagnosis offered by the patient, and with his history as told by himself. We might truly say that the physician has in some way declined his jurisdiction. You will see some striking examples of this confidence of the physician in the story of his patient, in the works of MM. Martins, Cazenave, and in the thesis, in other respects so well written, of M. Legendre.

How many causes of error there are in the stories of patients! Blennorrhagia is ordinarily a very painful and annoying affection, and one which leaves behind, to those who have had it, some smarting recollections. When you interrogate patients upon their previous history, it is always of their blennorrhagia that they first speak; they do not suspect the importance that the chancre can have, which, while it infects, is ordinarily indolent, suppurates but little, has little tendency to extend, and often cicatrizes of its own accord; it is rare that they make mention of this symptom, and if by a pressing inquiry you cause them to bring the circumstance to mind, they will tell you that it was a superficial chancre, a simple excoriation. I may certainly recall the fact that it is only since my works, that the manner of considering blennorrhagia as regards the symptoms of constitutional syphilis, has been a little more strict. In following the course which I have marked out, we are brought forcibly to confess that the great number of urethral blennorrhagias which do not furnish inoculable pus, were not followed by constitutional symptoms.

Among other statistics advanced, I shall cite the most recent, those made last year by M. Lafont Gouzy, who, out of 380 cases of urethritis inoculated, found only two cases in which the inoculation gave any results. One of the two presented, four months later, symptoms of constitutional syphilis.

In this work of M. Lafont Gouzy, he has mentioned two cases in which the inoculation gave no result, and which were, however, followed by syphilitic symptoms. We shall have occasion later to explain these exceptional cases.

M. Baumès cites five examples of individuals affected by *simple* blennorrhagia, in which the constitutional infection is nevertheless seen to appear at a later period. From these facts our honorable colleague draws an argument in conclusion, that the blennorrhagia non-symptomatic of chancre, can, like the chancre, produce the syphilitic infection.*

But, first, are all the veroles which have been attributed to blennorrhagia really the consequence of it? If we were not careful about the manner in which statistics were made, we should find, as M. Cazenave and others have, that blennorrhagia is the most frequent antecedent of the constitutional verole, because it is really rare to find individuals who have not had one or more attacks of blennorrhagia. But when, knowing the value of the chancre as a necessary antecedent, we seek to know what its frequency is, even among the authors where its valuation leaves so much to be desired, we find, in the statistics of M. Cazenave, for example, that out of 72 observations, blennorrhagia existed, alone or with buboes, only 18 times, while chancre occurs 38 times. From which, M. Caze-

* One of the five patients of M. Baumès had had a chancre previously; it is, then, to this chancre that the verole of this patient must be referred.

nave concludes, very logically, as you see, that blennorrhagia is the most frequent antecedent of syphilis. The same results from the summing up of the observations of M. Legendre, and the same logical conclusion follows.

It remains established for science, and in my opinion, even by the statistics of my antagonists, that the chancre visible and acknowledged by the patient, is still the most frequent antecedent of syphilis. My wards of the Hospital du Midi enclose at this moment 61 cases of well-marked constitutional syphilis; all, without exception, have had chancre as precedent.

Now, in cases where we cannot go back to the pre-existence of an external chancre, neither by means of the recollections of the patient nor by interrogation, what reason is there to absolutely deny the existence of an urethral chancre? You see, then, what we ought to think of the opinion of M. Cazenave, expressed in these terms: "Far from blennorrhagia never giving place to secondary symptoms, it would appear, on the contrary, to determine them more frequently than the chancre."

You know, for it appeared in your own Journal, that this opinion of M. Cazenave has been warmly approved. M. Vidal (de Cassis) has expressed his sentiments upon M. Cazenave in the following manner, which he says is not an academic authority, but which has the advantage of being an authority altogether peculiar.

"We know what the position of M. Cazenave is, the vast theatre upon which he makes his observations, his taste for statistics, for all the means, in fact, which, according to my adversaries, conduct to certainty. Well, M.

Cazenave *has succeeded in establishing* that the symptom, the virulence of which is rarely verified before experimentation, is exactly the symptom, which, according to observation, is the most virulent and the most infectious."

It is true that to prevent M. Cazenave from being too much in a hurry to felicitate himself upon this warm approbation, M. Vidal hastens to add, on the following page:

"However, I should not dare to go as far as M. Cazenave, who, according to my ideas, puts too many syphilitic eruptions to the account of blennorrhagia. Blennorrhagia, in my opinion, is an affection much more contagious than infectious."

That is just my idea, Monsieur Vidal, as you are well aware; only permit me to express my astonishment that it is yours, you who believe that M. Cazenave *has succeeded in establishing* the contrary. I do not wish to insist longer upon this flagrant contradiction, which is, after all, perhaps, only a conciliatory criticism.

As to the cases of blennorrhagia, the inoculated muco-pus of which has given no results, and which have been followed by a general infection, the observations which have been reported of them, leave much that would be desirable to know, and are, I ask pardon of my learned brother of Lyons, to be received with exceptions. The astonishing credulity, the truly blind confidence of some physicians, although rendering their works very respectable, are far, from this fact alone, from carrying conviction into all minds. In these particular cases I do not wish to spare the symptomatology of constitutional symptoms, which is incomplete, as regards some important points, upon which I shall wish to return; I desire, also, that in these cases, constitutional syphilis should really be the subject of inquiry. I admit that the appearance of these syphilitic symptoms agrees, as regards the time, with the development of blennorrhagia; but are we very sure from this fact alone that the patients have had nothing but blennorrhagia—that syphilis could not have penetrated by another way? My brother physician at Lyons has somewhere said that I denied the possibility of a constitutional syphilitic infection from a simple blennorrhagia, because I had never seen an example of it. It is, on the contrary, because I have seen many patients in whom physicians, who do not think as I do, have recognized only a simple blennorrhagia, where I have found another door for the entrance of syphilis, that my convictions have become more and more strengthened.

When those who maintain that a simple blennorrhagia ought to give place to the verole, have told you that the patient presented no ulcerations, neither upon the genital organs nor upon the fingers, they think they have nothing more to exact. They forget the doors of entrance without number, which the surface of the body presents, secret, concealed doors, which close as soon as they are opened; of which patients are ignorant, or a knowledge of which, it is for their interest to conceal. How many students have come to me from the other hospitals of Paris, in whom a blennorrhagia alone has been diagnosed, and in whom I have found chancres in unusual places. While upon this subject, here is a story, analogous to many in my practice.

A lady came to consult me for a disease of the rectum, the symptoms complained of being those of a fissure. Upon examination I found absolutely nothing about the anus. But the finger introduced into the intestine, discovered, at the height of the superior sphincter, a fissure situated upon the anterior portion, and reposing upon a callous surface. I proposed an operation; the patient refused, and I ordered her enemas of rhatania. This treatment had scarcely lasted fifteen days, when in another visit I perceived an exanthematous eruption, having all the characteristics of a confluent syphilitic roseola. Upon farther examination I recognized the swelling of the postesterior cervical glands. The patient suffered from nocturnal cephalalgia, and already scabs commenced to developе themselves upon the scalp. To me there could be no farther doubt upon the nature of the symptoms. I then examined the genital organs; but I perceived only a slight uterine catarrh. Interrogated as to the circumstances in which this lady could have been placed so as to contract syphilis, she confessed that her husband was diseased, that he had ulcerations on the penis, and that in the fear of communicating them to her, he had had relations with her *a preposterâ venere.* Thus the nature of the fissure was revealed to me.

In this case, is it not true, that without the painful symptoms brought on by the fissure, this ulceration would have passed unperceived? It would have then happened that we should have had for the sole antecedent of syphilis, a simple uterine catarrh. But there are still other causes of error which I wish to point out to you. This will be the subject of my next letter. Yours, &c. RICORD.

SIXTH LETTER.

My Dear Friend,—Let us continue this review of facts and arguments which have been opposed to my doctrines.

There is an observer upon whose works my antagonists place great value, and they are in fact worthy of much esteem. I have honorably cited them in my preceding letter, and you see me disposed to accord to them the value which they merit. This observer, whose results have been unceasingly opposed to me, is M. C. Martins. Well, what do the results of M. Martins prove in the elucidation of the great question of the consequences of blennorrhagia as cause of syphilis? Observe that it is precisely on account of the accuracy of the observation, of the scientific method employed by this observer, and on account of his statistics, that so much noise has been made about his figures and his conclusions. What, then, do his figures say? I find them very favorable to my doctrines. Is it by complaisance? Judge of it.

M. Martins gives a statement of 60 observations of syphilitic eruptions. Now how many times has the chancre been noted as antecedent? 46 times, my friend. In 14 cases only, M. Martins assures us that he has found no other antecedent than simple blennorrhagia, two of which were accompanied by bubo, and two by orchitis. But M. Martins adds that he had no opportunity to diagnose these cases of blennorrhagia, and that he trusted to the testimony of the patients. You know what I think upon this point. There are some testimonies, without doubt, that we ought to believe; but I shall always maintain that, when there is a question of diagnosis as difficult as that of chancre in the urethra, the testimony of people, entirely strangers to the profession, often ignorant and narrow-minded, and who understand neither the sense nor the bearing of the question, is of very little value. Without doubt we accept testimony in some questions much more important; in those of life and death; but it does not follow that the testimonies are always true, and the judgments always equitable.

Permit me to offer you a general remark, which finds here its place. In many of the observations of M. Martins, as in several of those of M. Cazenave, and as in nearly all those of a large number of authors, you find in their summary, these words—*many primary symptoms*. These primary symptoms, which have necessarily produced the constitutional verole, are the chancre and the blennorrhagia. If my antagonists, through some reasonable motives, attached the consecutive infection rather to blennorrhagia than to chancre, we should have to examine this doctrine. But no, you know it, you have read it, and you ought not to be greatly astonished, that it is together that they group these primary symptoms; that it is without considering the distance which separates their appearance one from the other; and that it is in giving to them all the same value, the same consequences and the same results. In truth, is this good science, is it strict observation? What should you think of a physician who should tell you: here is a man suffering from hydrophobia; he has been bitten ten times; three years, two years, one year, six months since, and very recently. But his disease is evidently owing to the successive inoculations which he has undergone. Or, here is a varioloid patient, who has gone through five or six epidemics of variola—at the last one, the disease manifested itself; it is but the consequence of contagions and successive infections.

I confess that it is not thus that I understand science. I am astonished that a mind as strict as that of M. Martins, who agrees with me that blennorrhagia is most generally due to causes entirely foreign to syphilis; who is logically forced to admit that the blennorrhagic antecedents as causes of syphilis are extremely rare, and that the chancre consequently is the most frequent antecedent of the verole—I am astonished, I say, that in order to arrive at the conclusion, that a simple blennorrhagia can produce syphilis, he is content with his sixty observations, of which he chooses three, and particularly one, which I ought to bring forward here.

"An apothecary, aged 23 years, contracts a blennorrhagia, but it troubles him so little that he continues at his occupations. He goes hunting, and even has sexual intercourse. Then follows an orchitis, which forces him to take care of himself. The blennorrhagia is cured, after having lasted six months. Seven years afterwards, an *ulceration appears at the opening of the left nostril, another one at the internal surface of the lower lip.* These ulcerations extend; the two lips are attacked upon the entire left side, then they are partially cured, and ulcerations follow at other points. *The ulcerations have rounded borders, and are cut perpendicularly;* the cicatrices are delicate, rosy and pliable. The patient, admitted into the wards of M. Biett, is cured in a month by the use of the proto-iodide of mercury. Shall we say that this patient, half physician, who examined himself carefully, as we have seen him do at the hospital, had chancres without perceiving them ?"

Yes, certainly, I will say that that patient had very well-marked chancres, from the description which M. Martins gives, and that the patient had not recognized them, on account of the unusual seat which they occupied. As to the manner of the contagion, M. Martins will not ask me, and I shall not take upon myself to point it out. He knows, however, as well as I, how these symptoms can follow, and without seeking malice therein, even in the exercise of the duties of this good apothecary.

You are aware, my friend, that the chancres, unusual as regards their situation, and difficult to discover, are less rare than is thought to be the case. I cited to you an example in my last letter. Here are others.

Some years ago, M. Lustermann, professor at Val-de-Grace, brought to my house a lawyer, having a tumor upon the lower eyelid, at the inner angle of the eye, hard, resistant, elastic, with a red granulating surface, and in process of cicatrization. This tumor had been already seen by many physicians, and, if my memory serves me, some oculists had been consulted, but its nature had been until now unknown. I was asked if it was connected with any venereal antecedent more or less distant. Pushing my examination further than my brethren, I found the glands about the ear, those of the parotidean region, and the submaxillary, enlarged, indolent and elastic. The posterior cervical glands were already tumefied. The surface of the body was covered with exanthematous spots proceeding from the best characterized syphilitic roseola; lenticular spots of a dull red, leaving in some places, under the pressure of the finger, a tawny yellowish color; absence of fever and of pruritus.

To the great astonishment of M. Lustermann, this was my diagnosis: *Indurated chancre at the inner angle of the eye (successive engorgement of the glands about the ear, also of the parotidean and submaxillary); secondary affection of the cervical glands; syphilitic roseola; precocious secondary symptoms.*

To the great astonishment of the patient, I said to him —It is two or three months, sir, more or less, that you conveyed to your eye the contagious matter, which inoculated you with syphilis.

Recovered from his surprise, the patient said to me, "In truth, I remember having slept with a woman, and after certain contacts, I was seized with great itching about the eye, which I rubbed with my hand for some time. It is from that moment that my eyelid has become diseased."

Is it not true, that if this gentleman had been attacked with a blennorrhagia, either antecedent or accompanying, it would have been to that, that the chancre of the eye and the secondary symptoms would have been attributed? Very well, I must say that I believe that the nose of M. Martins's apothecary probably found itself in the same condition as the eye of our lawyer.

M. Cazenave ought to recollect the history (no longer ago than 1847) of a very intelligent student in medicine, in whom he diagnosed a constitutional syphilis *d'emblée*, characterized by a roseola without antecedents. This young man presented himself at the Hospital du Midi, and there we were able, before all the students, to show the existence of an indurated chancre, extremely well marked, which had passed entirely unperceived, seated upon the left cheek, and concealed under a thick tuft of whiskers. The submaxillary glands—unobjectionable witnesses—were engorged and indolent, with that character of resistance peculiar to these glandular enlargements, symptomatic of indurated chancre. This ulceration, to which the patient had attached no importance, being revealed to him, he was able to state with precision the origin and the date of it, which agreed perfectly with the appearance of the secondary symptoms.

At this same time in the wards of the Hospital, was a patient having a chancre (primary symptom) upon the sinciput. I showed, at my clinical lecture, a woman who had an indurated chancre upon the left eyebrow, with a symptomatic enlargement of the glands about the ear, which had preceded by two months a nocturnal cephalalgia, enlargement of the posterior cervical glands, and a roseola.

I should never finish, if I attempted only to state those cases of chancre seated in unaccustomed places, which have passed under my observation, and which would be confounded, by observers little accustomed to accuracy, with secondary symptoms attributed to a blennorrhagia of shorter or longer standing. I have, even at this moment, in ward the first, of my hospital, a patient affected at the same time with a simple blennorrhagia of the urethra (inoculation negative), and with an indurated chancre of the upper lip, accompanied by an indolent enlargement of the submaxillary glands ; concomitant affections, but independent one of the other.

Here is sufficient, it appears to me, to prove to you how frequent and insidious are the causes of error under similar circumstances, and to justify my scepticism regarding certain observations.

But I ought not to forget that my learned brother of Lyons is waiting for me with five observations with which he opposes my doctrines. I ought the more to return to him, as these five observations have sufficed to convince the strict and reserved mind of M. Legendre.

First, as I have already told you, one of these observations is done away with, for the patient who is the subject of them had previously had chancres. Four cases of simple blennorrhagia followed by syphilis remain. But of these four cases, I shall allow myself to do away with two, for M. Baumès did not practise inoculation. These cases ought, then, to enter into the numerous category of those blennorrhagias, for which there has been no strict diagnosis. One remarkable fact, which you will permit me to notice in passing, is, that M. Baumès, who is certain of having inoculated the greater portion of patients who have presented themselves to him, has fallen precisely upon two cases of syphilitic blennorrhagia, in the diagnosis of which he deprived himself of the precious aid of inoculation. We are then reduced to two other cases, where inoculation has been practised with a negative result, and which have been followed, nevertheless, by constitutional symptoms.

In one of these cases there is question, also, of a nose, which again appears to me excessively suspicious. Here is the history of it, as reported by M. Baumès :—

"Of the two patients inoculated, one remained at Antiguaille two months. His blennorrhagia was difficult to cure; he had still a white discharge when he left the Hospital. He entered it again three months after with a syphilitic eruption, in red patches, copper colored, partly furfuraceous, partly scaly, and a rounded ulcer with a greyish ground, with perpendicular borders, and with an erysipelatous circumference, situated in the left nostril. At this period the discharge did not exist. This patient had had no coitus since his leaving the Hospital."

You will find here, again, a very complete description of the primary ulcer; and how does it happen that in presence of a fact so important, and as regards a question so litigious, M. Baumés did not try the inoculation of this chancre? I regret it sincerely, but in the absence of all strict diagnosis, I ought to place this nose in the same category with the nose of the apothecary.

Here I am, then, face to face with only one observation of M. Baumès, and that the last one. My learned colleague well says that he inoculated from the seventh to the tenth day of the appearance of the discharge; but how much time had passed since the the infecting coitus? M. Baumès knows perfectly well that a knowledge of this is not unimportant. He knows also, as well as I, that the chancre which is ordinary followed by secondary symptoms, generally extends itself but little; that it is perfectly indolent; that its suppuration is so little, that it may pass unperceived. Upon all this, M. Baumès is as well edified as myself, I am very sure. These ulcerations do not in any way prevent a blennorrhagia from being produced, a long or a short time after, and it is not astonishing that the one in question did not furnish inoculable pus, the chancre having arrived at the period of reparation, or having completely disappeared. It is moreover necessary to suppose, that before his first entrance into the Hospital, or after his departure up to the time of his return to it, the patient had not undergone another contagion, and by a way which escaped the sagacity of our colleague.

All these objections apply equally to the observation of M. Lafont Gouzy, in which secondary symptoms came on after a blennorrhagia which had been inoculated without result. He does not say anything of the time which separated the coitus from the manifestation of the symptoms, a period sufficiently long for the cicatrization or reparation of a chancre.

It appears to me, after all this, that my colleague of Lyons, who maintains that the simple blennorrhagia can give rise to the same symptoms as the chancre, will permit me to send back what he addressed to me, viz., "that he establishes as principle that which is in question, and advances an hypothesis devoid of strict foundations."

Thus fall to the ground, one by one, the objections, apparently so serious, made against my doctrine. Thus, I continue to believe—

With Girtanner, "that syphilis recognizes most generally chancres and buboes for cause, and that it very rarely follows a blennorrhagia."

With Swediaur, "that the symptoms of syphilis are rarely manifested after blennorrhagia."

With M. Rayer, "that the secondary cutaneous eruptions with blennorrhagia are rare; that we observe them in a much smaller proportion, than after superficial and deep venereal ulcers."

These opinions, as you see, agree very well with the relative scarcity of the chancres of the urethra with symptoms of blennorrhagia.

I could still cite many other authorities. But I have not finished with the objections. In my next letter I shall examine some of another nature.

Yours, &c. RICORD.

SEVENTH LETTER.

MY DEAR FRIEND,—From this fact alone, viz., that chancres have been submitted to a treatment called methodical, it has been thought that the consecutive symptoms of a constitutional infection, which ought to be the result of chancres, could be attributed to a blennorrhagia which came on afterwards. M. Baumès pretends to prove it in one of his five observations. But what is a methodical treatment? What is the treatment upon which we can absolutely depend for neutralizing effectually the syphilitic diathesis? For myself, I do not know of an infallible one. I well know that a great number of very distinguished practitioners think that with a certain dose of mercury, administered during a given time, we ought to consider the patients as radically cured. And in order not to go beyond the limits of my hospital, I shall cite my very honorable colleague, M. Vidal, who has recently given out, that with one hundred and ten of Dupuytren's pills, neither more nor less, we ought to put an end to syphilis.

As regards creeds, I am the most tolerant man in the world. Nobody more than myself respects the religion of others; but I have the right, it appears to me, to refuse a participation in all their convictions, when I see every day the proof of the great errors into which a blind faith may conduct one.

M. Vidal ought to have seen many patients return to him; and if this has not happened to him, let him permit me to say, that I myself have seen a great number of those, who have not only taken the one hundred and ten sacramental pills, but even 120, 150 and more, all of which has not prevented the symptoms from re-appearing.

I shall not longer insist upon this point, for I shall have occasion to return to it later. What I want to establish here is, that those persons are often deceived, who have thought that they ought to ascribe symptoms of constitutional syphilis to a blennorrhagia which has come on after a chancre, from the simple fact that the chancre which had preceded, had been submitted to a mercurial treatment.

Here is a point more astonishing, something which will surprise your reason and baffle your logic.

My opposers have established several categories of veroles, according to their origin and their source.

Thus they admit, and in this they are perfectly right, that constitutional syphilis can be transmitted by way of inheritance.

They assert, and they have pretended proofs for this assertion, that constitutional syphilis can be taken *d'emblée.*

They assert, and they publish facts for the support of this assertion, that sometimes no kind of antecedent to constitutional syphilis can be found, although they dare not ascribe it to the syphilis *d'emblée.*

They pretend that an individual under the influence of a syphilitic diathesis, without actual manifestations, without apparent symptoms, can, however, under certain circumstances, transmit syphilis.

They maintain that the duration of the incubation of syphilis should be unlimited, that the manifestations of the contagion should appear as well after a few days as after a few months, or after several years, twenty, thirty and more.

All these categories, all these distinctions, you will find established particularly in the writings of M. Cazenave; but upon what grounds? Here is what I in vain ask myself. I inquire by what process, by what means of diagnosis, we can come, in a patient affected by a constitutional verole, to attribute this disease to one of these circumstances rather than to another.

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

Can constitutional syphilis, *d'emblée* be distinguished from the other kinds by any pathognomonic sign?

Do the cases of verole in which the antecedents have not been made out, give rise to disorders different from those in other cases?

What is a verole without antecedents, unless it is a verole *d'emblée*?

Do we find that those cases of syphilis which have succeeded to *simple* blennorrhagia, assume forms less grave, or have less extended seats, as M. Baumès pretended to find, in writing his book, but which he has not been able to meet with in his practice?

I answer boldly, No, to all these questions. Constitutional syphilis presents a symptomatology alike in all cases; and it is not I who prove it, it is my opposers themselves. Read again their writings, and see if you can find in the descriptions given by MM. Cazenave, Baumès, &c., one single characteristic trait which justifies these arbitrary distinctions.

Again, one thing in my opponents astonishes me. How does it happen that in these cases of constitutional syphilis, whether *d'emblée* or without antecedents, when it has been impossible for them to be assured of the conditions of the contagion—to state precisely the when and the how—if it is well proved that the patient has presented no primary symptoms, since they have found no entrance for the verole; when they are well convinced that the patient is not mistaken, and that he has no motive in deceiving; when, in fine, they have the certainty of not being themselves deceived; how is it, I say, that they do not admit what Cullerier admitted to explain the inexplicable cases, viz., spontaneous syphilis in man!

M. Richard des Brus has taken this great step. Among other facts which brought him to this conviction, he cites one which is very curious. A young man and a young woman yield themselves to the pleasures of love. In his ardor the young man scratches himself with a hair of his mistress. He does not stop for such a trifle, and he consequently communicates his *écorchure* to his mistress. The amorous couple are soon simultaneously affected with the constitutional verole. M. des Brus, who had examined neither of them, did not the less admit a previous good state of health; but not being able to explain the appearance of the verole, he declares it spontaneous.

I am not as far advanced as this learned colleague, and the so frequent opportunities that I have, of seeing constitutional affection succeed to a well-determined primary affection, causes me to rank the exceptional cases, where the patient does not know or does not wish to enlighten me, and those in which I arrive too late to find the entrance of the syphilis, in the category of observations which M. Cazenave entitles *unknown antecedents*, and which I call *overlooked.*

Alas! is it not more satisfactory for the mind, more conformable to our manner of reasoning in medicine, to admit in those cases where syphilis has really succeeded to a blennorrhagia not symptomatic of chancre, that the cause has not been *recognized*, rather than to lose one's self in that crowd of subtile distinctions, of arbitrary categories, and of sterile explanations? How, otherwise, will my contradictors undertake to prove to me what they say, and to convince me of error? It is not my habit to challenge any one; this sort of argument ought to be banished from scientific discussions; but I much wish that they would engage to prove to me once only, yea, once, that, in those cases where all my researches having been vain, I have said *antecedents overlooked*—that they would prove to me, that something more affirmative could be substituted for this formula.

From this long discussion, my dear friend, it will appear to you without doubt legitimate to conclude,

That, if in this immense majority of cases, blennorrhagia is simple and benign, there exists also a virulent blennorrhagia;

That the blennorrhagia is virulent when there exists a concealed chancre in the urethra.

Now, does the means of making the diagnosis of concealed chancre exist?

Is it possible to distinguish a simple blennorrhagia from a blennorrhagia with concealed chancre?

Here is the grand question. I commence its discussion.

Some persons have made light of the diagnosis of blennorrhagia. Hecker, and some others who have followed him, have not thought that the diagnosis was very necessary. Very recently I read in your valuable Journal that the diagnosis had no relative importance. A certain number of physicians have retained ideas which have been in vogue, and which ought greatly to astonish the public.

Have you caught blennorrhagia from a wife who was not yours? Virulent blennorrhagia.

The blennorrhagia is virulent for the lover, for the husband it is benign.

You have contracted a blennorrhagia, and you ought to remain bachelor.

Simple treatment.

But you wish to marry:

Antisyphilitic treatment.

The position of bachelor, or of future husband, has the privilege of making the blennorrhagia pass from the benign into a virulent state.

In a question as serious and important as this, I do not wish to insist upon the absurdity of these contradictions. All have understood the necessity of a more strict diagnosis. The latest of my opponents, M. Vidal himself, with whom my proceedings in diagnosis have not found favor, has made some attempts in this matter. In the first edition of his Treatise upon External Pathology, he gave out the hope that it would be possible to distinguish a virulent discharge from a benign one, by the *odor*. It is to be regretted that his hopes were not realized, for this passage disappears in the second edition.

I hold rather more to my ideas than M. Vidal appears to hold to his. Will you, then, permit me to give out once more, both my ideas and my experience upon the diagnosis of blennorrhagia, and to examine the objections which have been made to them.

But I cannot treat of this subject in the short space which remains for me, not wishing to abuse to-day the generous hospitality which you afford my letters. This point will be the subject of my next epistle.

Yours, &c. Ricord.

EIGHTH LETTER.

My Dear Friend,—It is my purpose to-day, as I promised you, to see if it is possible to distinguish a simple blennorrhagia from one with a chancre concealed in the urethra.

You see that I lay down the problem as boldly as my opponents.

In the study of this diagnosis, it is important to establish two conditions:

The one an absolute, unequivocal and undeniable diagnosis;

The other a rational diagnosis.

An absolute diagnosis can only be obtained by artificial inoculation.

Every time that muco-pus furnished by a mucous surface will give the characteristic pustule, which we shall soon have to examine in studying chancre, we can affirm, whatever has been the duration of the disease, that this muco-pus is virulent, that there is a chancre somewhere; the chancre alone being able to give rise to the positive results of inoculation.

Here is the incontestable fact established by my researches, and the absolute and unequivocal diagnosis in all its strictness.

When by the inoculation of muco-pus from the urethra you obtain the characteristic pustule—pronounce boldly, and without the possibility of error, it is a virulent blennorrhagia.

But only ask of inoculation, as of all the other means of investigation, what we have the right to expect from it.

We must have variolic or vaccinal virus to produce the effects of variola or vaccine.

If at the side of a variolic or vaccinal pustule an abscess is developed, and you should take the pus from this abscess for inoculation, you would not obtain the specific effects of the vaccine nor of the variola.

Take some muco-pus from the neighborhood of a variolic pustule developed upon the Schneiderian membrane, and this muco-pus will not give the effects of the variolic pus.

If we have, then, a patient actually affected with an urethral chancre, and at the same time with a simple blennorrhagia (a frequent complication), and in the place of taking pus from the chancre, we take it from the blennorrhagia, the result will be necessarily negative. It does not require much mind to understand so simple a thing, and I am astonished that M. Vidal, who has so much good sense, should make of this, an objection against inoculation. I have too high an esteem for his understanding to admit that he could believe that pus furnished by chancre of the urethra, when a blennorrhagia coëxists, ought necessarily to be mixed with all the blennorrhagic pus; or that a drop of pus from a chancre, acting after the manner of leaven, renders the other necessarily virulent. Without doubt, the complication of morbid elements, as regards the diagnosis, often renders the analysis difficult, but an exact knowledge of each of these elements permits us, under any circumstances, to distinguish between them.

The chancre of the urethra, which can never have a very great extent or large surface, can furnish only a very small quantity of virulent pus. Even in the indurated chancre, the secretion is sometimes almost nothing, generally insufficient to stain the linen of the patient. A very fine example of this can be seen at this moment in No. 15 of the first ward of the Hospital du Midi.

Every time, then, that we have to deal with a very abundant discharge, we have the right to suppose that there is something else besides the product of chancre. We must guard against concluding upon the absence of chancre in the urethra, from the negative results of inoculation.

But if the inoculation is repeated several times—if, moreover, care has been taken to press out the secretion of the urethra in order to obtain the more immediate product of the ulcerated surfaces—and if the results have always proved negative, there is a very great probability that it is a simple blennorrhagia and without the complication of chancre. Without doubt the diagnosis here is neither absolute nor complete; but does it not present at least something more than the diagnosis which is generally made?

In order to draw a conclusion from the negative results of inoculation, the epoch at which the experiment is made must be kept strictly in view. We shall see later, in studying the chancre, that the virulent secretion has a term, and that there is a moment when the ulcer passing into the state of simple ulceration ceases to furnish specific pus. If, then, experimentation is made too late, less can be concluded from the negative results, than if the inoculation had been made during the first or second week following the infecting coitus.

In examining inoculation under this point of view, does it not offer all that strict reasoning can demand?

If the results are positive, this gives you the most absolute demonstration that diagnosis can give. If they are negative, the results conduct then to a rational diagnosis, of which they may be the most valuable elements.

Let a more sure or a more fruitful diagnostic sign in human pathology be found. What! would not that be a sign of great importance, which permits us necessarily to establish, if present, the existence of a lesion with grave consequences, and which when not existing, can conduct us with a sort of certainty to a rational diagnosis!

And because this demonstration has also its uncertainties, shall we not pay attention to those circumstances in which it presents a value and a mathematical precision?

Are we, then, so rich in absolute diagnosis, that we ought to show ourselves indifferent, sceptical, or scorners, regarding a sign the existence of which smooths over so many difficulties?

What other means but inoculation, in a case of legal medicine, will permit us to state strictly that a blennorrhagia is or is not symptomatic of chancre?

But is it asked if inoculation is always applicable? Do we always arrive in time? Can we and ought we always to count upon it? Must we always have recourse to it? Certainly not; I have written and repeated this a hundred times in my lectures, and it is incredible that objections should be again sifted over which I have myself made so very often. Inoculation, since it is again necessary to repeat it, is an excellent means of diagnosis, but of which we are often deprived. Is this a reason for renouncing the research into the distinctions between simple and virulent blennorrhagia? Without doubt, no; and fortunately, a well-directed, minute study of all the elements of the disease, gives, in the great majority of cases, whatever my opponents say, a diagnosis sufficient to enable us to conclude upon the prognosis, and to furnish the indications of a truly methodical treatment.

It is not sufficient, as we shall see later, merely to have before us a primary ulcer in order to fear the constitutional verole, and to necessitate a mercurial treatment; other conditions are ordinarily sufficiently well marked to enable us to recognize them.

Permit me, then, to pass over again very briefly in review, the ordinary elements of the diagnosis of blennorrhagia, of which there has already been a little question, on account of the etiology.

You recollect what I said of women considered as a focus of infection, and the value which we can put upon the source, as regards concluding upon the virulence or the simplicity of blennorrhagia. The patients have a singular naïveté upon this point, and entertain a strange idea of morality. How many times have I seen young people enter my office and say to me—the blennorrhagia which I have caught cannot be otherwise than benign, for I contracted it from a married woman, the wife of one of my friends, and I am very sure that it cannot be anything more than an *échauffement*. At this I am accustomed to answer—Sir, if your wife had a lover, would you consider her as a very honest woman? This question perplexes nearly all of them, and they see very quickly, that in order to fix upon my diagnosis, I have recourse to means rather more certain than the morality of the source.

A perfectly healthy woman, I have already said, may be a source of infection.

Among the curious and singular facts which have passed under my eyes, permit me to relate to you the following, which has also its moral, as you will see.

A young and small household had invited to breakfast a friend of the husband. The repast was almost terminated, and the appetite was not satisfied. It was decided that a morsel of cheese should be added to the feast. The husband leaves the table, descends four pair of stairs, and runs to the neighboring grocery to seek the complement of the friendly repast. Alas! he does not return sufficiently quick. During his short absence, and between the pear and the cheese, his unfaithful better half committed adultery with his perfidious friend. The husband returns, the repast is finished, coffee and its accompaniments are taken, the friend retires, and the husband in his turn consummates the conjugal act.

Three days after, the husband comes to me with a chancre of the urethra, with blennorrhagic symptoms. He was accompanied by his wife, and he assured me that he had had relations with no other woman. With the most careful examination of the genital organs of that woman, I could not discover anything suspcious. My prescription made, these individuals went away, leaving me without explanation of this virulent blennorrhagia of the husband.

But the next day the wife returned, to ask me if I was very sure that she was not diseased. I examined her anew, and again I assured her that she was perfectly well. Then she related to me the history which I have just told you, and she added that the delinquent was there, and begged me to examine him. I found upon him a magnificent chancre on the corona glandis, in the specific period.

This fact confirms the curious experiments made at the Lourcine by my young and learned colleague M. Cullerier. He placed some virulent pus in the vagina, let it rest there some time, took it again upon his lancet, and inoculated with positive results, and the vagina, submitted to the treatment of injections only, was not infected.

You will conclude with me, my friend, that the source from which the cause of the blennorrhagia has been taken, cannot give a great value to the diagnosis.

I shall not return to what I have said of incubation as a means of diagnosis. The chancre of the urethra is sometimes developed very quickly, and can furnish pus at an early period. So that, far from considering the blennorrhagia as virulent which has taken more time to appear, it is the contrary that we must very often admit.

The *violence* of the blennorrhagia has been made a synonyme of its *virulence*. In truth it is just the contrary. As a general rule, it is those cases of blennorrhagia which are the least violent, the least painful, which ought to cause us the greatest fear of the existence of a chancre in the urethra.

The duration of the discharge is a sign not to be neglected. It is not the most tenacious discharges which make us fear the existence of a chancre in the urethra.

The nature of the secretion has great value when we know how to appreciate it. The secretion which is the result of an ulceration of the urethra, is much more purulent than mucous; it is ordinarily sanious, rust-colored, and charged with blood; the least pressure, moreover, upon the urethra, renders these characteristics very sensible. But to give to this symptom (the presence of blood) all its value, we must be certain that the patient has not previously used a caustic injection, that foreign bodies have not been introduced into the urethra, or that he has not had a rupture of the canal during chordee; and that, moreover, the sanguinolent matter is not expelled with the last drop of urine, in which case it would be the sign of cystitis with vesical tenesmus.

I do not speak to you of the speculum for the urethra as a means of diagnosis of the ulcerations of this canal. It is an ingenious method, which has not afforded what it promised. To distinguish chancre, situated even at a considerable depth in the urethra, it is sometimes sufficient to make the meatus gape by stretching open the lips.

Wedkind had thought that he found in the enlargement of the follicles in the neighborhood of the urethra, near the frænum, a symptom of virulence; but these enlargements are generally only phlegmonous, and independent of every other complication.

The most important symptom consists in the engorgement of the canal, especially in the region of the gland, which is the most frequent seat of chancre in the urethra.

I have already said, that it is not so important to be able to verify the presence of an ulceration, either by the aspect and the nature of the secretion, or by inoculation, as it is to know if one has to do with an ulceration capable of determining the syphilitic infection. It is this which all authors have had in view, when they have spoken of virulent blennorrhagia.

Well! as we shall soon see, it is the indurated chancre which is the fatal antecedent of the constitutional verole. Now nothing is generally more easy to prove than the presence of an indurated chancre of the urethra with symptoms of blennorrhagia.

If a blennorrhagic complication does not exist, the patients scarcely suffer in micturition; the jet of urine is generally twisted and impeded on account of the diminution of the calibre of the urethra; the erections are not painful, when the chancre is seated in the region of the gland.

In order to well ascertain the presence of these ulcerations, it is necessary to explore the urethra by the aid of pressure which is exercised from above downwards, from the dorsal face to the inferior, as when we wish to make the meatus gape. In exercising this manœuvre, we perceive a cord, more or less extended, which some writers on syphilis have designated under the name of *corde balanique*. In the greater number of cases, it is easy to ascertain the side of the canal upon which the ulceration is seated. Independently of the indurations plainly limited upon one side, we see that side form a convexity, whilst the healthy side separates, in forming a crescent. When the pressure is exercised from right to left, nothing is felt, and the induration ceases to be appreciable.

Doubtless the swelling in the region of the gland or of the follicles may be only the result of a simple inflammation, without virulence; but to complete the diagnosis we must have recourse to the accessory symptoms.

Thus the affections of the glands are very rare in the blennorrhagia non-symptomatic of chancre. When they take place, as I have already pointed out, they are acute, terminate easily by resolution, or when they suppurate, it is simple pus that they furnish.

With the urethral chancre, dorsal lymphangitis of the penis and the affections of the glands are much more frequent. If the chancre is non-indurated, the glands suppurate almost inevitably, and when the seat of the pus is opened, the suppuration furnishes incontestable marks of virulence. In the indurated chancre of the urethra, which is the most important to recognize, the affections of the glands are inevitable and necessary; several glands are affected at once, and they remain indolent and do not suppurate—upon all which conditions, I shall have occasion to return hereafter.

Finally, if all these conditions have not been appreciated—if these symptoms have not been seized upon, either because we have arrived too late or because they have been overlooked, we may rest assured, that if the patient has been attacked with blennorrhagia symptomatic of chancre, six months will not pass without the appearance of the symptoms, if the constitutional affection has taken place.

We shall have next to examine whether, as a last resource, it is not better to wait this length of time to give a diagnosis, than to cause the patient to undergo, during the same period, a mercurial treatment, which, after all, does not afford more certainty. Yours, &c. Ricord.

NINTH LETTER.

My Dear Friend,—If I could think that your readers had remarked the interruption of my correspondence, and especially if they had complained of it, I should ask you to have me excused, on account of other imperious duties which have taken up the few and short instants which I could devote to you. I could easily contract the pleasant and charming habit of these periodical conversations with the numerous public, which your talent and that of your fellow laborers have known how to gather about your Journal. But you are so rich and so varied in this respect, that my absence could not cause any loss. I shall, however, do all in my power, in order that the good will of your readers may hereafter accompany me as regularly as possible.

I wish to terminate to-day the subject of blennorrhagia, by some words upon its treatment. You understand that in these letters, details would be idle and useless. I confine myself to the generalization of all these questions; the developments making the subject of a special and extended treatise, which I soon hope to be able to offer to the judgment of my friends. Here, I touch upon all the doctrines of the Hospital du Midi, and I ought to conclude that which treats upon blennorrhagia by some considerations upon the treatment of this disease.

When we see the obstinacy of certain writers on syphilis, in retaining the old ideas concerning blennorrhagia, recognizing and admitting only virulent blennorrhagia, it would seem that these writers ought not to be satisfied of the existence of any discharge, without applying as soon as possible a mercurial treatment. But, it is not so. The greatest number of them content themselves with a rational treatment, and among them you will range M. Vidal, who does only what I do, and perhaps less; for in what he has written upon blennorrhagia, although nowhere establishing an absolute differential diagnosis between virulent and benign blennorrhagia, he does not speak at all of the antisyphilitic treatment properly so called. Look at the Treatise on External Pathology by M. Vidal, and you will be astonished like myself, that with his ideas upon the virulence of blennorrhagia in general, the treatment of my colleague should be so benign.

I have already said a word upon the astonishing and ridiculous custom of those who prescribe copaiba and cubebs for the blennorrhagia of bachelors, and who reserve mercury for whoever wishes to marry. This mode of therapeutics with two aims, recalls to me the history of one of my old colleagues of the Hospital du Midi. He had in his youth, like many others, contracted blennorrhagia. At a later period, he was to marry the daughter of an old writer upon syphilis, who was imbued with the doctrines of the treatment of precaution; he obtained the hand of his intended, only upon the condition of a long-continued treatment with the liquor of Van Swieten. The treatment finished, the marriage was accomplished—all those who lived in intimacy with this colleague, and even those persons who were present at his clinical lectures, might have heard his frequent and bitter recriminations against this treatment of betrothing. After all, this treatment was very useless in the case of our colleague, for he preserved an habitual discharge from the urethra; a final and peremptory argument, which he was in the habit of presenting to the individuals whom he did not succeed in curing of a similar inconvenience.

Others, more logical in appearance, in admitting the virulent blennorrhagia, and confessing nevertheless that they cannot distinguish it from the benign blennorrhagia, give at all hazards and notwithstanding, a mercurial treatment. Hunter is of this number, and his manner of reasoning upon the treatment of blennorrhagia is very curious. If Hunter had no other title to the thanks and the admiration of the wise, his writings would not have come down to us, and M. Richelot, your learned and modest collaborator and friend, would not have gifted France with his beautiful translations of the works of the great English physiologist. Let us hear Hunter. The following passage is not foreign to the question:—

"Whatever may be the method adopted for the treatment of gonorrhœa, whether locally or internally, we must not lose sight of the fact that a certain quantity of the matter of the discharge may be absorbed, and show itself afterwards under the form of constitutional syphilis. To guard against this effect, I think that small doses of mercury ought to be given internally. It is not easy to determine at what epoch this mercurial treatment ought to commence; but if it is true, as I have before explained, that the syphilitic diathesis once formed cannot be cured by mercury, while this therapeutical agent has the power to prevent a similar diathesis from being established, it is important that it should be commenced early, and should be continued until the end of the disease, not only until the secretion of pus has ceased, but also some time after. Mercurial frictions can be employed, when the stomach and intestines cannot support the medicine.

"This practice is much more necessary, if the discharge has existed for a long time, especially when the treatment is composed of simple evacuants only. In fact, when the discharge is of long duration, the absorption has more time to exercise itself; and when recourse has been had to evacuants only, there is more reason to fear that this has taken place, inasmuch as this treatment has no faculty to expel the virus from the economy.

"To prevent the establishment of a constitutional virus, the consequence of the absorption of the venereal pus, it suffices to prescribe a grain of mercury every evening, or morning and evening; but it is necessary to continue the employment of it in proportion to the duration of the disease.

"The success of this practice can never be verified in any particular case, because it is impossible to say if the pus has been absorbed, excepting in those cases where it forms buboes; and every time that we remain uncertain as to the reality of the virulent absorption, it is impossible to affirm that a constitutional syphilis will be manifested, if mercury has not been given; for among those patients who have not taken mercury, we see few who are attacked with constitutional symptoms, consecutive upon a gonorrhœa. However it may be, it is prudent to prescribe a mercurial treatment; for it can be reasonably admitted that we shall often thus prevent the establishment of a constitutional syphilis, as takes place when we administer it to patients affected with chancres or buboes, which under this treatment would certainly determine a general infection, as experience has taught us."—(*Complete Works.*)

I ask pardon for this long citation; you know that it is not my custom; but it appeared to me so much more necessary, as this doctrine still serves as the basis for the reasonings and the practice of a great number of writers upon syphilis.

Must I first insist upon the manner in which Hunter admits the constitutional infection from blennorrhagia? It is not the part actually diseased that infects, it is the pus secreted! Evidently Hunter has never reflected upon this singular mode of infection, and those who have followed him do not appear to have reflected any more.

It is true that this doctrine has been singularly revised and augmented. Thus, you will find in a modern writer upon syphilis, that in blennorrhagia, the affection does not take place by means of that portion of the mucous surface which is diseased, but through the portion of the mucous surface of the neighboring part which has remained healthy, this alone having the power to absorb the virulent muco-pus; from whence it is necessary, my friend, to draw this absurd conclusion, that if the entire length of the urethra was diseased, the consecutive infection would never be feared.

The *coques muqueuses* of Hufeland are also an emanation from the Hunterian doctrine. You know that he pretends that if the blennorrhagia does not oftener infect, it is because the pus is enveloped in some small mucous follicles *(coques)*, from which it has not always the power to escape.

Let us return to Hunter, and we shall be painfully surprised to see this great mind wishing to prevent infection by mercurial treatment, assuring us that the longer the disease has lasted, the more chances there will be of infection, and the more it will be necessary to give mercury; and not perceiving that if the mercury only acts by preventing the infection, its administration would be useless after a long continuance of the blennorrhagia, inasmuch as the infection would be already established, and the mercury would have no power upon it; we shall be astonished that in spite of his uncertainty upon the action of mercury against the infection, he affirms in so absolute a manner its efficacy in doses so rigorously and mathematically determined; we shall be confounded at meeting in the passage cited only a tissue of wrong constructions and of contradictions. The mercurial treatment the most ordinarily excites blennorrhagic discharges, and Hunter wishes that it should be continued until the complete cessation of all secretion! How many patients, whose discharge does not stop, would be thus condemned to mercury forever! My colleague, of whom I lately spoke to you, would have been literally choked with mercury. What would have become, under the weight of a treatment so prolonged, of an old soldier whom I attended, who contracted blennorrhagia at the peace of Amiens, and who had it still in 1845—that is to say, for more than forty years ?

The whole of Hunter's doctrine is lamentable from its discrepancies. Shall I afford myself the pleasure of demonstrating this singular confession—" The success of this practice can never be verified ;" and that one, more singular still—" We see few patients who are attacked with constitutional symptoms consecutive upon a gonorrhœa." Is not the entire matter, even by the confession of Hunter, reduced to this—that the mercury is useful only in the small number of those patients, whose blennorrhagia is due to an urethral chancre !

Thus everything, even error, comes to confirm the exactitude and the truth of the doctrine of the Hospital du Midi.

Lastly, the treatment of blennorrhagia brings us again into the presence of the theory of the half-way treatment of M. Lagneau, who regards blennorrhagia as a light form of syphilis, and advises for it a demi-treatment. We see peep out here the demi-virus, and the demi-virulence, of our brother at Lyons, M. Baumès.

Demi-treatment ! Light form of syphilis ! Alas ! there is unfortunately nothing light as regards the verole, unless it be the certain opinions of very grave men. Syphilis exists, or it does not exist. If there is syphilis, as complete a treatment as possible is necessary ; we must make use of all the guarantees that a serious and methodical treatment can give. If the verole does not exist—good heaven, for what good is an anti-syphilitic treatment?

How must we treat simple, benign blennorrhagia? I repeat again, that I confine myself to the generalities of the question. First, one word on the abortive treatment. You know all that has been said upon repercussion, upon the theory of the wolf shut up in the sheep-fold; you are aware of all the apprehensions which have been manifested in regard to the metastasis and the wandering about of the virus in the economy, occasioned by the abortive treatment of blennorrhagia. This doctrine has always astonished me in presence of the facts which present themselves in great numbers, and that, too, every day in practice.

First, it is incontestable that the greater part of the symptoms to which blennorrhagia can give rise, never manifest themselves before the end of the first week; and it is from the second week, and most generally later, that we see these symptoms take place.

On the other hand (and those who frequent the Hospital du Midi well know it), the greatest number of these symptoms manifest themselves only in those cases of blennorrhagia where no treatment or an insignificant one has been made. Do you wish me to give you a singular proof of this? Here let me inform you incidentally that I profess a great deference for medical statistics, that precious instrument, which managed as it has been by the skilful hands of M. Louis, has rendered such incontestable services to our science. But M. Louis is the first to recognize and to proclaim that nothing is more difficult and more delicate than medical statistics ; nothing which by its faults, or by its vicious application, could conduct to greater deceptions or to more deplorable errors. This profession of faith being made, I hope that no one can consider what I am going to say relative to the causes of the symptoms produced by blennorrhagia, as an attack against statistics, or as a mockery of that precious instrument of research.

I said that the abortive treatment of blennorrhagia had nothing to do with the symptoms which may be manifested in the course of this disease. Do you know, in truth, what the statistics absurdly interpreted would teach in this respect? Why, that the most frequent antecedent of epididymitis is flax-seed tea. I possess upon this point enormous statistics, and the students of my clinique wait every day with a mirthful impatience, this final question, which I never fail to address to the patient affected with an epididymitis—have you taken flax-seed tea? The answer is invariably affirmative.

What shall we conclude from these statistics and facts? Evidently that epididymitis, like the other symptoms of blennorrhagia, is neither a repercussion nor a metastasis, nor any of those chimera by which some have wished to prevent the timely and abortive treatment of blennorrhagia.

I am profoundly convinced by my observation and by my long experience, that a blennorrhagia arrested the first days of its appearance, far from being followed by those symptoms which are feared, will prevent, on the contrary, the manifestation of them. The abortive treatment of blennorrhagia is at the same time the prophylactic treatment of the consecutive symptoms. Thus, in practice, I have adopted the abortive treatment applied at the first moments of the appearance of the blennorrhagia. This is a point of doctrine upon which I cannot too much insist—the commencement of the disease is known, its end and its consequences are always uncertain. It is, then, of great importance for the patient to disembarrass himself of his discharge as soon as possible.

In spite of an old prejudice, of which the practice of Bell might be the pretext, I profess, that the injections which constitute one of the most important parts of the abortive treatment, far from producing strictures of the urethra, as has been said and still repeated, form the best abortive treatment for these strictures. We can be assured that the quicker a discharge shall be arrested, the less shall we have to fear the organized alterations of the urethra; these latter are, as for all other mucous surfaces, the consequence of the duration of the inflammation. I well know that here, again, statistics have been invoked, and that cases sufficiently numerous have been brought forward, in which strictures have manifested themselves after injections. But this is a little like the flax-seed tea in the cases of epididymitis. We must not infer a relation of cause to effect from this alone, that injections are found among the antecedents of strictures. Analyze well these observations, and you will see that they apply to long-standing cases of blennorrhagia, which have resisted everything—even injections; it is precisely because these injections have not cured the inflammation, that the stricture has followed—which fact does not necessarily imply their unskilful or untimely employment.

I do not wish to terminate this letter, without saying a word upon the prize which my honorable colleague and friend, M. Diday, of Lyons, has just established. You know that he offers the sum of 300 francs to whoever shall bring to him ten observations upon simple blennorrhagia which shall have produced constitutional syphilis. This idea is good, but do you think it sufficiently generous? Thirty francs for each observation so difficult to find—frankly, is it enough? I consider as beyond price one fact of syphilis coming on without syphilitic cause; thus I shall not put any value upon this point. Let my wise and spiritual friend permit me to say to him, that he would neither compromise his present nor his future fortune, if he increased a hundred fold the value of the observations which he demands. Yours, &c. RICORD.

TENTH LETTER.

MY DEAR FRIEND,—To-day I shall speak to you upon syphilis.

As you may have remarked, I have not lost sight for an instant of my point of departure.

What was it?

To seek out the specific causes of those diseases considered venereal; to study in a more rigorous manner their mode of action, in order to arrive at last at a more exact knowledge of their consequences and of their treament.

In the preceding letters, I have endeavored to show that if blennorrhagia can have a special cause, it was not always easy or even possible to distinguish this special cause from the common causes of the inflammation of mucous surfaces. I have endeavored to establish that this cause was not that which produces syphilis properly so called; that its consequences were entirely different, and that its treatment, unless empirical, cannot be that which we ought to oppose to syphilis.

I should have been very happy to have merited in all respects the criticism of M. Vidal, who asserts that my efforts have tended only to prove "that two and two make four." If I should apply this to all that still passes in syphilopathy, this proof would not for every one be equally easy to arrive at.

The cause of syphilis not existing in blennorrhagia, where must it be sought for?

Do not require that I should precipitate myself into the depths of history. I have often descended there, and I declare to you, that I think it impossible to discover the truth therein. The farther one descends, the less light penetrates, and he arrives at a point where the obscurity is complete. So that, arrived at this point, authors only proceed by groping; they wander about unceasingly, and lead us astray with them.

Where did syphilis commence?

Through whom did it commence?

I much fear that these questions are forever insoluble. What we can affirm is, that syphilis, such as we recognize at the present day, is not developed spontaneously in man;

it appears to be always transmitted. And yet, as we have already remarked, we do not meet with it in any other class of animals. I well know that very recently your Journal announced that syphilis had just been found in Italy in the horse. In order to believe this news, I await some more complete descriptions of the symptoms. It would, nevertheless, be rather singular, that syphilis, which they accuse of having been propagated for the first time in Italy upon the human race, should appear also for the first time in Italy upon the horse.

What strikes every man who studies history without preconceived ideas, is, to find in the ancient authors, and especially in those who were anterior to the epidemic of the fifteenth century, perfect descriptions of all that we know to-day, and which we range among the primary symptoms. Could we trace out at the present day a description more exact and more true than that of Celsus? Galen goes even so far as to find some relation between the symptoms of the genital organs and those of the throat. William of Sallicet knew that the primary ulcerations of the penis had been contracted by connections with filthy women; he established perfectly the relations which exist between ulcerations of the genital organs and buboes, &c.

The more exact knowledge of the filiation of the symptoms, of the connections and origin of the primary and constitutional symptoms, is what has been wanting to observers and historians of the verole, from the earliest times. But what was the leprosy of that epoch? Was the leprosy of the Greeks or of the Arabs, which we recognize at the present day, similar to the leprosy of those times? In no respect; for the leprosy was then often contagious, and it was frequently communicated by sexual intercourse. Evidently, it was not our leprosy. The Bible, in spite of all the efforts of commentators, enlightens us but little upon this point. Probably the divine inspirer of the sacred books might have had serious motives in leaving some obscurity on this point.

I have no pretension to retrospective science; the works of Astruc have frightened me too much, and I confess I am little tempted to undertake so great a work for so small a result. But whoever studies syphilis, however little he may have his mind tormented by the anxiety to know, will ask of himself, what I have done a hundred times, what was this terrible epidemic of the fifteenth century, and where did it come from?

Some cotemporaries have made it come from the stars. I do not know that they retrospectively searched out what passed astronomically at that period; I am myself unable to do this. But it is certain that syphilis always reigns, although Jupiter is to-day much more temperate, and Saturn and Venus no longer deliver themselves up to connections which had such unhappy consequences for the human race. We are, then, forced to seek our explanation upon the earth, and to take our subject from a less elevated point of view.

This terrible epidemic, this veritable '93 of the verole (1493), which at first no cotemporary thought of making come from the new world, found this origin in the writings and in the active propagandism of Oviedo, from motives into which it is useless to enter, and of which we shall find the application in the religious, political and jesuitical history of the time.

We know that it is this fable which has become the theme of the great romance edited by Astruc. Heaven preserve me from discussing this ; it is a work that has already been well done by Sanchez. I will allow myself only a trifling observation in a pathological point of view.

In order to have brought about an epidemic upon such a grand scale, it must have been necessary that all or nearly all the sailors of Christopher Columbus should have been infected with syphilis.

It was necessary that during a very long voyage, which was then not made by steamers, the primary symptoms should have remained at the period of progress, or of specific *statu quo*, susceptible of furnishing the contagious pus that we shall soon study.

One thing is very remarkable, that the sailors of the fleet, having arrived at Lisbon and at Bayonne, did not first infect the women of those ports : and yet is it probable, that, contrary to the habits of the sailors of all times, these should have, after a long voyage, exercised continence upon arriving in harbor ? Well, it is not to the women of Lisbon and Bayonne, that they communicate their disease ; they leave for Italy, where they go to meet the army of Gonzalve de Cordova in May, 1495, and it is there that they communicate the verole—to whom ? We know nothing, excepting that it was in Italy in the midst of three armies—Spanish, Italian and French—that a disease, then known since 1493 or 1494, raged with fury, each of the belligerent parties repelling the disgrace of having communicated it to the others.

I do not wish to insist longer on this historical question so confused and obscure, and which I have not the pretension to desire to clear up. I only ask myself if this epidemic of the fifteenth century resembles our venereal diseases of the present day ; and I find certainly not. The symptoms that we observe to-day resemble infinitely more those that the ancients have described, than the epidemic of the fifteenth century.

Here, permit me to communicate to you, with the reserve and the discretion which similar things require, an idea which I believe to be a fruitful one. I submit it as a simple hint to some young and industrious colleague, who shall have the good fortune to find himself in that happy period when consistent researches are possible.

In studying with care the descriptions of the epidemic of the fifteenth century, I am struck with a fact, which appears to me to be of marked interest. The mode of the transmission of the symptoms, their gravity, the predominance of the constitutional infection over the local phenomena, which are wanting, or which passed unperceived, all this appears to me to resemble much more what we recognize to-day as the acute glanders, and the farcy, than the verole. Van Helmont has published an analogous idea which has been considered perfectly ridiculous. He makes the verole come from the farcy, as the consequence of I do not know what ignoble beastly relations. Apart undoubtedly from the shameful source from which he drew his ideas, Van Helmont was perhaps not far from the truth.

Observe, that a knowledge of the glanders and of the farcy in man is very recent, and yet the liability of man to contract this disease, which has existed from all time in the horse, is undoubtedly not a recent fact. How many men suffering from the glanders and from the farcy have been liable to be, and have been, taken for syphilitic patients!

The manner of the transmission of the epidemic of the 15th century must strike us. The disease was often communicated by the breath in churches, in confessionals, to such an extent that Cardinal Wolsey, accused of having the syphilis, was brought to judgment for having spoken in the ear of Henry VIII. This mode of propagation is entirely inexplicable for syphilis, which requires an immediate contact.

I well know that all the authors of the time do not admit this mode of transmission by the sole contact of the breath. Fallopius ridicules in a pleasant way Victor Benoit who had seen some holy daughters of a convent catch the verole through the thick grates of the parloir. Fallopius believes that there was mixed with this, a little *holy water* (*eau bénite*). But in all cases could not the epidemic, which certain authors already, and Paracelsus among others, considered as a mixture of the ancient venereal diseases and of the leprosy, be more probably considered as a mixture of the ancient venereal diseases with the glanders and farcy—the glanders, so spontaneous and easily produced upon horses, and especially in time of war, and with the incumbrances which follow in its trail.

Study the symptoms, and you will see the gravest first manifested, and as if *d'emblée*, which does not happen in the syphilis of the present day. You will see that inoculable pus was produced in all parts of the body, which you do not see in the syphilis now known to us.

I know not if I am mistaken, but it appears to me that there is in this, a truly interesting subject for research. I seem to see the first dawning of a truth which has escaped us, even to this hour. We shall owe this truth to the beautiful works of M. Rayer, and of his school, and of M. Renaud (of Alfort), upon this terrible disease with which man is so sadly endowed, and in which I find such striking resemblances with the epidemic of the 15th century.

What glorious things there are to be done in this matter!

Are we aware of what the glanders, transmitted from man to man, and removed from the horse, can produce?

Do we know what its hereditary influence is? For individuals suffering from the glanders or from farcy can procreate, and we are completely ignorant of what may become of the product of these procreations.

I should be happy to awaken the zeal of some laborer in our science. There is here, it seems to me, an ample harvest of glory to reap.

But I confess it, all these ideas are still agitated in my mind, in the vague domain of hypothesis. Your readers I can understand must be desirous to see me enter into the field of reality. I arrive there; adopting the conclusion of Voltaire, I say that syphilis is like the fine arts, of which no one knows the origin nor the inventor. But what I know is, that it is found to-day at a source, alas, too certain, and it is from this source that I shall draw it in my next letter. Yours, &c. RICORD.

ELEVENTH LETTER.

My Dear Friend,—We must now determine the source from whence the specific cause, the morbid poison which produces syphilis, is derived.

This poison, we can at the present day call by its name, the *syphilitic virus.*

Well! this virus — I must needs recall the circumstance, inasmuch as endeavors have been made to obscure it — was formally contested and denied, when I undertook my first researches in syphilopathy. This was the time when numerous physicians did not dare to give it this name without fear of compromising themselves. It was the time when the learned Jourdan, in an access of singular anger, cried out — "call it as you will, but do not give it the name of virus."

The source of this virus, I have obtained at the point of the lancet, upon which, however, I have not had the pretension of placing all science, as my honorable colleague, M. Cazenave, wittily accuses me.

It is in studying comparatively all the symptoms reputed syphilitic, that I have succeeded in demonstrating that one alone of these symptoms would constantly furnish the purulent matter, capable in placing it under conditions which we shall determine, of producing, in virtue of a special irritation, an ulcerating inflammation identical to that which has been the source of it, and of reproducing in its turn the same special secretion, the same morbid poison, and this without limit.

The syphilitic lesion, source and origin of the secretion, placed in favorable conditions, produces inevitably the phenomena which we have just indicated, and which is the primary symptom to which has been given, and which has preserved the name of *chancre*.

Every time, as I have already had occasion to remark, that we were able to see the surfaces from which we took the morbid secretion, which was to serve for experimentation, it is only when there existed a chancre, that positive results could be obtained, and that we were able to reproduce the chancre.

Must I again say that my excellent colleagues, MM. Puche and Cullerier, at Paris; M. Baumès and Diday at Lyons; M. Renault at Toulon, Serre at Montpelier, M. Thiery at Brussels, M. Lafont Gouzy at Toulouse, &c., have arrived, in their very numerous experiments, absolutely at the same results as myself.

Every time that the chancre could be produced with a secretion which had not been taken immediately from a primary ulcer, the secretion was furnished by surfaces which could not be inspected. The small number of cases, apparently exceptional, in which the chancre could be reproduced with a purulent matter taken from a non-ulcerated surface, find their rational and absolute explanation in facts analogous to those the history of which I have recounted. How can it be concluded that the surfaces which cannot be inspected are not the seat of chancre, inasmuch as they furnish absolutely the same secretion as the chancre? Ah! if it was proved that the primary ulcer, fatal source of the syphilitic virus, could not be seated, excepting upon external surfaces which are always visible; that the depths of the urethra, and the cavity of the neck of the uterus, could not be the seat of these concealed ulcerations — if this was proved, all would be said; but does there exist one sole writer upon syphilis who denies the existence of the primary ulcer upon all these regions, and who does not know and who does not believe that all syphilitic ulcerations are not always visible? How, then, can we deny the possibility of the existence of deep and concealed chancre, when it in itself furnishes the most undeniable proof, that is the secretion?

It has been said that inoculation cannot serve any purpose in proving the existence of the specific cause of syphilis; that it was preferable to confine ourselves to the ordinary results of contagion to arrive at this proof; for with any pus whatever, one can produce what I pretend to produce only with the pus of the chancre, while by the mysterious ways of common contagion, phenomena are observed, which inoculation does not produce.

It is at least strange that these same arguments are equally employed, both by the maintainers of the syphilitic virus, and by those who deny its existence. In fact, what do these physiologists say? That by any pus — by any cause whatever — the same result was arrived at, viz., the production of every variety of venereal disease. And upon what do they rely to sustain this doctrine? Upon motives which might appear reasonable; upon all the uncertainties which ordinarily exist under the circumstances in which the venereal diseases are contracted; upon the non-examination of women; upon the great number of symptoms produced by the same woman upon several men, while this same woman might leave other men entirely indemnified from evil consequences; finally, upon all the fables that we have already signalized and combated, and upon which one is truly astonished, after what the speculum has discovered, to see men of merit as incontestable as M. Cazenave, still wish to ground superannuated doctrines.

But I am profoundly astonished that the partizans of the syphilitic virus, those who recognize in syphilis a specific cause, and in its virus a specificity of action, sustain, that with any kind of pus, effects can be produced analogous to those of the inoculation eminently virulent. Do the partizans of these doctrines think that we could produce vaccina or the variola by any kind of pus? If they had to experiment upon purulent matters, the source and origin of which they were ignorant, what would be their criterion for determining the nature of them, if it was not the effects produced? Is it not in this way that I arrived at distinguishing the syphilitic pus?

But to this objection *of any kind of pus* as a proof of the inutility of inoculation, I have something more to answer.

I have inoculated the same patient, and that a hundred times, with the pus of chancre, of balano-posthitis, with the muco-pus of urethral blennorrhagia, with the muco-pus of blennorrhagic ophthalmia, with the pus furnished by the phlegmonous inflammations of other regions; and while that of the chancre inevitably reproduced the chancre, the other kinds of pus remained without action. What do they want more than this proof, and what can they answer to it?

Another objection, however, has been made. They have said, inoculation does not prove any thing as to the nature of the cause, from the effects that it can produce upon an individual already submitted to the infection; in other words, in inoculating the patient with the secretion that he himself furnishes, no conclusion can be arrived at, inasmuch as, that if infected, every wound can and ought to become syphilitic.

Herein is a strange error, the consequences of which might be very grave; a dangerous prejudice, which I am astonished to see again brought forward in our day, with the sanction of observers who make pretensions to exactitude and precision. The facts which I have just recalled, peremptorily destroy this objection. I well know that facts relating to leech-bites, for example, have been cited, which have afterwards taken on the character of venereal ulcers. But be assured, my friend, these bites, like every wound in a syphilitic patient, do not become virulent ulcers, unless they are finally infected by contagion. Apply leeches where there has been no contact with inoculable pus, bleed the syphilitic patients as much as you wish, practise any other operation whatsoever, and never, unless there has been virulent contact, will a virulent transformation be possible. Among the numerous observations, which I have collected in proof of the truth of this assertion, I will recall the following fact of the Hospital du Midi.

At the period when I had women in my wards, a patient affected with a phagedenic chancre of the vulva, with abundant suppuration, was seized with a pain in the tibio-tarsal articulation. Leeches were applied upon the painful spot. Some days after, the patient complaining of pain at the seat of the bites, it was easy to recognize that some had undergone a veritable transformation, and that they had become veritable chancres. One might be inclined to believe for a moment in the influence of the general condition of the patient, and some of the students were so inclined. As to myself, I had not the least doubt about the mechanism of this transformation. In the first place, all the bites were not ulcerated — first proof. Secondly, the patient was seized with similar pains in the articulation upon the opposite side; a new application of leeches was made, but this time, in guarding the bites from every infecting contact, none of them underwent the least syphilitic transformation.

I have made an experiment still more conclusive. It has often happened that I had to experiment with the pus of a chancre upon a patient at the time under the influence of a constitutional syphilis determined by a preceding contagion. Some comparative inoculations were made, and then again the matter of the chancre alone gave place to positive results.

Thus, whatever may be said, it is impossible to compare a syphilitic patient to a bottle full of virus, which it would allow to escape through the smallest opening. The image is poetical, but it is not just.

But in order that these results should be necessarily obtained, reason tells us, in the first place, that the *virulent* *matter* ought to be taken from a chancre at a certain period — that is to say, at the period of progress, or of specific *statu quo*. It is very easy to conceive this, and I am sure that I shall not weary you if I try to make you understand, that if you take the pus for inoculation from the surface of an ulcer which is in process of reparation and of cicatrization, you will have a simple and inoffensive pus, which will give you negative results, and that the same symptom interrogated at two different epochs will say to you, "yes" and "no." You will conclude, then, with all observers of good faith, that here there is no contradiction in the results of experimentation, nor uncertainty, and that it is no evasion, no subtilty of doctrine, to explain facts opposed to the principles which I sustain, and which are similar to those of Bru. When Bru did not succeed in inoculating the pus of chancre, one of two things happened; either he made a false diagnosis and directed his attention to other ulcerations, or he took the pus from chancres *at the period of reparation*. There is no way of escaping from this dilemma; for I repeat it, and I am ready to prove it to the incredulous, if there are any still, *the pus of the chancre is inevitably inoculable.*

You will perhaps find, that I indulge myself too much in the pleasure of writing to you: but it is your fault, you never stop me. Profiting, then, by your good will, I will say that if the *virulent matter* composed of a special morbid poison and of a vehicle, is ordinarily formed of a thin, ichorous, sero-sanious pus charged with organic detritus, it does not always present itself with the same characters; it can offer all the known varieties of pus or of muco-pus.

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It can be acid or alkaline, contain animacules or not. These different conditions which appear contradictory, and have also served as an argument to those who deny the existence of a virus, belong only to its vehicle, and change nothing of its nature, which remains always the same. There is only one circumstance important to signalize, and which experiments upon inoculation have verified—viz., that the putrid pus is not virulent, that gangrene destroys the virus—*it kills it.*

Whatever may be the seat of the chancre from which it has been taken, in order to act, the virulent matter has no need of being recently secreted and warm. Preserved as vaccine is, it acts equally well. Artificial inoculation proved this, contrary to the opinion of Cullerier, which opinion was generally hitherto received.

Inoculation has proved the truth of the different modes of contagion, which have been more or less contested, so far as the belief in the necessity of the physiological action and orgasm of the part which furnished the contagion; and so far as the belief that this ought to be yet warm at the time of infecting. The observations of Fallopius and Hunter, of chancres contracted in touching the seats of public privies; those of Fabricus of Hilden of symptoms taken by sleeping in sheets in which infected persons had already slept; and of so many others, have thus become incontestable.

You will still permit me to say a word upon the condition which the part which one inoculates, ought to present. Whatever it may be, skin or mucous surface, no matter in what region, *a simple solution of continuity* is sufficient, without *the aid of any physiological act, in order that the effect should be inevitably produced;* there is nothing here, as in the case of the variola and vaccina, which resists the primary symptom; there is no privilege of idiosyncrasy; the most perfect equality exists in the presence of a point of a lancet charged with virulent matter.

Thus, then, the inoculation made with the pus coming from a primary symptom, with the pus of a chancre, in the condition which I have just recalled, has always produced identical results, whether experimentation has had for subject the patient who furnished the pus, or whether the pus has been inoculated from an infected to a healthy individual, as some experimenters have done.

It has, however, again been said—it is imprudent, rash and impossible to conclude anything from artificial inoculation; you impose upon nature conditions different from those in which she is placed during the contagion which we can call natural by contradistinction. And condemning this artificial inoculation, it has been supposed that the same could be said of this, that is said of physiological experimentation — "La torture interroge et la douleur répond."

Our celebrated physiologist M. Magendie, to whom you addressed your first, and so remarkable *medical letter*, will tell you what he thinks of this indignation of the poets. I, who do not wish to speak with the same authority, shall say, that I do not contest the mysteries of nature, that I know she does many things by processes which she conceals from us. But I maintain, also, that it would be an unworthy weakness to attempt to render her still more mysterious, and to thicken the veil which covers her; that it would be shameful to shut our eyes when she wishes to disclose herself.

Let us see, then, if there exists any real difference between the natural and the artificial contagion. I shall tell you what I think of this in my next letter.

Yours, &c. RICORD.

TWELFTH LETTER.

My Dear Friend,—Does there exist any real difference between the natural and the artificial contagion? This is the subject of our conference.

The observation and rigorous analysis of facts demonstrate to those who do not suffer themselves to be led away either by prejudice, or by preconceived ideas, that the contagion of syphilis, under whatever circumstance it may operate, is finally reduced to a process of inoculation more or less analogous to that by the lancet. The lancet, in fact, inoculates the symptom (the chancre) which by the confession of all is the most inevitably contagious. It is by this symptom, by the chancre, according to observations well made and *collected at the proper time*, that syphilis commences.

Laying aside artificial inoculation, the chancre is seen to develope itself everywhere upon the surface of the body without choice of seat, and upon all the external or internal integument, which is accessible, and by consequence, without there being need of special functions or of any particular physiological condition, either for the parts which are infected, or for those which furnish the infecting matter. Other conditions are necessary for contagion.

Examine with care all the parts which are affected, you will find that it is those which present the most favorable conditions for mechanical lesions, for scratches, for lacerations, and for solutions of continuity of every kind; you will find, also, that it is there where voluminous and numerous follicles exist, into which the virulent matter can introduce itself, that the symptom is by preference developed.

Is it not true that in the male it is more particularly the border of the prepuce, especially when there is a phymosis more or less pronounced, the neighborhood of the frænum, the adherent points of the semi-mucous surface of the gland and of the prepuce, points which not having the suppleness of other regions are more easily torn, that by preference become infected by the contagion; in the female the fourchette, the points of insertion of the nymphæ myrtiformes, are the parts which the most easily take on the contagion? In the other regions, is it not true that it is when excoriations exist that the contagion is established? Thus, an excoriation upon the finger is often the door where syphilis can enter. But the presence of an excoriation is absolutely indispensable. If it was otherwise, should I ever go out of the hospital without having a chancre at the end of each of my ten fingers? The chancre often appears upon the lips, but the lips are almost always cracked; pleasure excites the smile, and smiling extends and dilates the lips. The nipples of nurses are often the seat of chancre, but these parts are ordinarily cracked and torn. The chancre seats itself wherever there has been a cicatrix, but there also there is a loss of suppleness, and consequently cracks and lacerations are easy.

In all this, friend, you see nothing which is, as they say, physiological, which exacts special vital conditions, a particular state of the organism and the exercise of any function whatsoever. All this, for you, as for myself, is reduced to a traumatic and mechanical phenomenon.

Practice, that criterion of all doctrines, justifies, alas too often, my doctrine. Nothing is more common than to see the physiological act of generation rest indemnified from every unhappy consequence, while other acts which have nothing in them physiological, produce painful results. The genital organs, the seat so special of syphilitic affections, do not always take the infection from genital organs. It is not always the genital act properly called, which becomes the infecting cause. Coitus does not become an infecting act unless certain material circumstances come into play. Among the innumerable examples which I could cite for the support of my opinion, I ask permission to cite to you two, which have struck me more, inasmuch as they presented themselves to me suddenly upon the same day. There is no physician who does not know that there are some singular days, when curious facts arrive as if in series.

A gentleman brought me one day his mistress, whom he had infected, and in a manner which much astonished him. He had upon the penis a primary ulcer at the period of specific progress. He had had normal intercourse with his mistress, and in the same night intercourse more culpable, *à prepostera venere.* The lawful intercourse had been much more frequent than the other. The woman presented absolutely nothing suspicious upon the genital organs, but she had a chancre in the anus. What did this mean? That the physiological and natural passages had yielded without laceration, and had escaped contagion, while the abnormal passages, more resisting, were torn and became affected.

Here is another couple. Here, again, is a contest between a physiological act, and a prelude which does not belong to the human species, a prelude which is not at least placed among the genital functions of man. A gentleman surprised at seeing a suspicious bud pushing forth upon one of his lips (bud without a flower, as Jean Lemaire would have called it), without any disease of the genital organs, comes to ask me to examine the woman with whom he had had intercourse. I found upon this woman a chancre at the specific period, situated in the neighborhood of the meatus urinarius. This gentleman had had rather frequent sexual intercourse with this woman during the same night, during which he had gone astray so far as to sadly expose his lips. It is necessary to add that this gentleman was very subject to chapped lips, and that all this passed in winter.

These facts, which I could multiply, prove that the physiological conditions of the genital act go for nothing in the contagion of syphilis. Thus, the doctrine of physiologism finishes upon this point by falling to the ground. Be assured, that in spite of the most intimate contact, in spite of the most voluptuous orgasm, with an entire skin and an irreproachable mucous surface, one can escape safe and sound from the most exposed intercourse. Be assured, on the contrary, that a portion of skin abraded, a mucous surface chafed, will render the slightest intercourse dangerous, and we physicians have a thousand precautions to take in this respect. We know, however, that the medical corps has furnished victims to the martyrology of syphilis, and that it was in the beneficent exercise of our art that the unfortunate Hourmann, and Delavacherie of Liége, met with a tedious and frightful death.

After what I have just told you, what can you think of the pretended physiological inoculation of my colleague M. Vidal, as regards blennorrhagia? You know when and how this latter is really inoculated by the lancet. It is when it proceeds from a chancre, and only then, and this is very rarely the case, as M. Vidal agrees with me. But in other conditions in which blennorrhagia is produced, is there, physiologically or pathologically speaking, anything which resembles the contagion of chancre? Do we even always know, as I have said, if the blennorrhagia is always 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 cannot produce. Hear what M. Baumès says: it would seem that the successive contagions of blennorrhagia were his means of diagnosis, without telling us, nevertheless, how many times blennorrhagia ought to be produced in order to be virulent. Thus one takes a blennorrhagia, he gives it to another; where does the virulence commence? M. Baumès does not say. Suppose that a woman is suspected of having contracted a discharge from a suspicious man—if we should wish to assure ourselves upon the nature of the discharge of this woman, it would be necessary to hold an inquest, to pursue the different sources of the blennorrhagia of the man, going back even to the gonorrhœal flux of the Bible. Yes, but we should not have made one step in this inquiry, without finding ourselves in the presence of that most common difficulty, viz., of two individuals having had commerce with the same woman, the one will have contracted a blennorrhagia, and the other not. For one, we should conclude upon the benignity of the blennorrhagia, and for the other upon its virulence. All this is not serious.

Facts and observation, then, indicate no difference between the inoculation called physiological and the artificial. Let us now invoke analogy.

In every malady *incontestably* contagious, we find that the traumatic conditions dominate, and that under ordinary circumstances art can repeat what nature does. Thus, the vaccine inoculated does not differ from ordinary vaccine. The variola inoculated does not differ from the spontaneous variola. Thus, with the glanders, the farcy, hydrophobia, malignant pustule, and hospital gangrene. This argument from analogy appears to me of incontestable value. Why should the syphilitic virus alone escape from the common rule?

But the chancre, it has been said, is not the only contagious syphilitic symptom. There are some secondary syphilitic symptoms the *contagium* of which the lancet has not yet discovered. Science, in fact, contains many observations which appear conclusive to a very large number of physicians, and which leave doubt in the minds of many others. The numerous tubercles, or condylomata, are considered by a very large number of writers upon syphilis as contagious, and consequently can be transmitted.

When I have studied these symptoms by means of inoculation, considering well all the circumstances which could enable me to prevent error, the experiments have always been negative. However, other observers have obtained contrary results. I can only answer for this exception by stating the result of my own experience.

I inoculated with the pus of numerous tubercles proceeding from the neighborhood of the vulva of a young girl of Versailles, who entertained habitual intercourse with the garrison of the place, and I obtained a positive result. Much astonished, I examined with more care the surfaces from which I had taken the pus, and it was then easy for me to recognize that among the numerous tubercles, there existed a chancre, still at the period of specific progress. Then, some new inoculations being made with the pus taken from this ulceration, and with the matter of the mucous tubercles at a distance, the pus of the chancre gave the characteristic pustule, and the muco-purulent secretion of the mucous tubercles remained without result. This experiment appeared to me decisive.

In the observations which have been cited of mucous tubercles which have communicated syphilitic symptoms—the period which has passed between the time of observing the patient and the infecting coitus has not been taken into account. It is always three weeks, a month, two months or even more after the contagion, that the patients present themselves to the physician, so that not only the real form of the commencement of the disease is wanting, but still it is impossible to determine the true nature of the symptom which has been the source of the contagion. Some individuals forget, and others do not know, that by a succession of changes easy to observe, where one takes the pains, the primary symptom (the chancre) passes *in situ* from the state of an *organ* of virulence to the conditions of a secondary symptom, furnishing no longer specific pus. Where are the observations upon persons seen with mucous tubercles, who have transmitted the disease to another person, who could be examined the second or third day after the infecting coitus, and in whom the disease commenced as we see it commence after contagion from a chancre? Does the disease in this case commence with the chancre, or with the mucous tubercle? There is not one single incontestable fact which can answer this question. Facts upon mucous tubercles are, however, not wanting. I possess very numerous observations of well-characterized mucous tubercles upon men and women, which prove that the patients thus affected could indulge in frequent sexual intercourse without communicating anything. Among all these facts, here is one which will remain deeply impressed in the minds of my readers, as it has in my own.

A gentleman whom I had attended for a chancre two years before, was about to marry. Before his marriage he came to see me again, in order to submit himself to a rigorous examination. I found him in the best state of health; he could be married without any scruples. However, this gentleman, who was very particular, exacted of me another examination the very evening of his marriage. I still found him perfectly exempt from every symptom, and I delivered to him my bill of health as clean as possible. One month after, he sent for me. My dear doctor, he said, my wife has some large pimples upon her which trouble her very much. See what it can be. Before passing into the chamber of the wife, I proceeded to a new examination of the husband. I found him in as healthy a state as the day of his nuptials.

But it was not the same with his wife. I found some confluent and well-developed mucous tubercles, such as to assure me that the origin of the symptoms was anterior to the marriage.

Convinced that the husband had nothing to do with this sad affair, and that he could not communicate a disease which he did not have, I said to the wife in a firm and decided tone—Madam, you are diseased, and it is not your husband that has rendered you so. If I become your confidant, I also become your accomplice; in the contrary case, I shall remain the physician of your husband. I was not long in obtaining a painful confession, which gave me the key to this unhappy enigma.

I recount to you this fact because it contains this which is interesting, viz., that since marriage the husband had not passed two days without having repeated intercourse with his wife, and notwithstanding, he had absolutely no disease.

I have not finished with the mucous tubercles; permit me to return to them in my next letter.

Yours, &c. RICORD.

THIRTEENTH LETTER.

MY DEAR FRIEND,—I return to the *mucous tubercles*. As you know, this symptom, with many writers upon syphilis, is contagious. Among the proofs invoked for the support of this opinion, we must note that one which considers the successive development of these *mucous tubercles* upon those portions of the skin which are contiguous to those where the symptom first developed itself, as a result of contagion. Thus, patients are seen who have these mucous tubercles on the sides of the scrotum. Do they develope themselves upon the inner portion of the thighs—contagion! cry out the partisans of this opinion. If upon one side of the anus these tubercles gain the opposite side—contagion! they again cry, and so on. Those of my brethren who profess this doctrine, and there are among them some very distinguished ones, forget one little circumstance; viz., to consider the cause which has produced the first tubercles, that is to say, the state of the constitutional infection in which the patient happens to be, a state which can cause a second and a third tubercle to put forth, for they do not all appear at the same time. The consideration of the seat of preference of these tubercles cannot in any way aid the doctrine of contagion ; in fact, if there is a contiguity in the parts of the skin where these tubercles appear, we must also observe that there also acrid secretions are more active ; that the skin, in these places, has a tendency to the mucous transformation, as in the neighborhood of the genital organs, of the anus, &c. Moreover, how can we explain, by contagion, the development of these mucous tubercles from one arm-pit to the other?

I shall remain, then, always convinced, until proof to the contrary arrives, that if some have thought that they have seen *mucous tubercles* contagious, if they have admitted that they might be primary symptoms, they have erred in diagnosis. I do not think it useless to recall to mind that *the chancre, at the period of reparation*, often assumes, in granulating, the aspect of mucous tubercles; that it can undergo sometimes a veritable metamorphosis, and become *in situ* a secondary symptom, the physiognomy and the nature of which are those of mucous tubercles. If we have not been witnesses of its commencement, if we neglect to invoke the testimony of the neighboring glands, the remains of the margin of the ulcer and the characters of its base may have been so modified, that the differential diagnosis would be very difficult to make, especially for inattentive eyes and for unskilled fingers. Add to this certain particular seats, where the primary symptoms are not usually observed, and where also the transformation of the chancre is more easy, and more rapid, as upon the lips, upon the tongue, upon the nipples, and you will see how easy it is to be deceived.

All those veroles, transmitted by kisses more or less lascivious, by the utensils of the table, by pipes, razors, masks, &c., have no other origin. And how many times have not these circumstances been *honest* pretexts for concealing other contacts! The mask, moreover, has been from all time, and in our day still, a very convenient article for disguising a compromising diagnosis.

Proofs of secondary contagion have been looked for in certain religious practices; for instance, syphilitic symptoms transmitted to infants by the process of the Hebrew circumcision have been arranged in this category. But these symptoms find their natural explanation in the fact of the presence of primary symptoms in the mouth of the circumcisers. Let me here be permitted to say that I am one of those who have done the most to cause the ancient and dangerous practice of the suction to be rejected by the Israelite Consistoire of Paris.

Many physicians absolutely will not take into consideration the facility with which the chancre passes into the secondary state; they regard only its seat; and when they see a chancre in the mouth, they are induced to consider it, from this fact alone, as a secondary symptom. Herein lies a grave error in observation; this gives me the occasion to say that the primary ulcers become much more frequent in the mouth than in the anus. I meet with these last much less frequently, both in the hospital and in the city, than formerly. It appears to me that certain shameful practices diminish in frequency, and that there is progress in this respect in the public morality. However it may be, from the sole fact that a chancre is seated in the mouth, do not conclude that it is a secondary ulcer. Do not forget the famous genito-labial nerve invented by Voltaire, a spiritual pleasantry which must be sometimes considered as serious. I knew a very distinguished brother physician, who has always remained convinced without other proof, that an ulcer of the cheek had been communicated to him by a *secondary kiss.*

Since I have told you that I have often seen persons affected with different varieties of mucous tubercles upon the genital organs, who transmitted nothing in their sexual intercourse, I ought to tell you, also, that I have seen an equally large number with numerous tubercles upon the lips, upon the tongue, and upon the throat, who lived together, and who practised all lawful contacts with the mouth, without ever transmitting anything. I know a gentleman in the neighborhood of Paris who having, during six months, numerous tubercles upon the tongue and upon the lips, had with his mistress all possible intercourse, was very negligent about his treatment, and convinced that the symptoms which he had, could not be contagious, continued his intercourse without ever communicating anything.

It is, morever, with regard to the transmissibility of these secondary symptoms from the nurse to the child, and *vice versâ*, that this question becomes important. The fact of this transmissibility is generally admitted. Hunter has, however, denied it, and many serious observers partake of the opinion of Hunter. This question is so important that you should permit me to enlarge somewhat upon it. It concerns public hygiene ; it is often a question in legal medicine ; fraud, infidelity, cupidity, may be brought into action ; it is important, then, to guard against all the causes of error, and not to readily accept the stories of individuals who might have more or less interest in deceiving us.

If one consults the archives of science, if one searches for the basis upon which the opinion of the contagion of secondary syphilitic symptoms from the nurse to the child, and reciprocally, rests, he is astonished at the little value of facts, and how many grave men there are, who are content with this little. M. Bouchert, for example, in an article recently published (Gazette Medicale, 20 Avril, 1850), has collected all the facts which have appeared to him the most positive. Well ! read this article, interesting in other respects, and you will be convinced, like myself, that the greater part of these facts are not admissible ; that the observations which appear the most probable are wanting in essential details, and are so incomplete, that M. Bouchert is himself forced to so far confess it, that he finishes by allowing that his conviction upon this point is more moral than scientific.

Here is what I myself have observed in this matter.

I have seen nurses and infants infected, who have been mutually accused of this infection ; most generally I have succeeded in finding the regular and inevitable point of departure, going back to a primary symptom in one or the other. Sometimes I have met with merely simple coincidences. In those cases where it has not been possible for me to go back to the primary cause, I arrived too late ; the children were not presented to me till five or six months or more after their being put to nurse.

I have had, during several years, a ward of nurses at the Hospital du Midi. In this ward, I had often women affected with simple leucorrhœa, to whom I gave children with secondary symptoms, to nurse, sent to me from the Maternité, and never under my observation were these women affected.

On the other hand, nurses affected with very manifest secondary symptoms have given the breast to infants sent to me as infected with syphilis, these latter having in reality nothing but simple eczematous, impeteginous eruptions, or species of porrigo, and never under my observation were these infants affected. My learned and industrious friend, Dr. Nonat, who has had, during a long time, the care of the nurses connected with the hospitals, has arrived at the same results, and does not believe in the contagion of secondary symptoms from nurses to children, and *vice versâ*.

In my private practice, I have seen a great number of facts of this kind. Here is one of the most remarkable, which I observed together with my friend Dr. Chailly-Honoré. The subject of it was an infant born with hereditary syphilis, and in whom, six weeks after birth, various symptoms made their appearance, such as mucous tubercles of the ano-genital regions, humid scaly papulæ upon the trunk and upon the limbs, deep ulcerations upon the lower lip. This infant was given to *a nurse upon the spot* at the moment of its birth. We were able, both M. Chailly and myself, to observe the child as well as the nurse, during the eighteen months that the nursing continued. The ulceration of the lip persisted during more than three months. This was scarcely cured, when, in spite of a careful, methodical and continued treatment, a new ulceration manifested itself upon the velum palati, and also remained during several months. Well, this nurse remained free from all infection; she enjoyed and enjoys still the most perfect health.

Surely this is a fact well worthy of attention. I have just observed an analogous one, with my friend M. Bassereau. A child, who, with other symptoms of hereditary syphilis, had ulcerations upon the lips, was nursed with entire impunity by its nurse.

You see, my friend, how important it is, in the appreciation of similar facts, to take into consideration all the conditions in which the nurse and child could be, if we do not wish to deceive or to be deceived.

The nurse, at the moment of taking an infant, might be under the influence of a syphilitic diathesis which nothing yet indicated. I ought to add that generally, when one takes a nurse, she is not submitted to a complete and careful examination. And even when this is done, we may still be deceived, for the diathesis may exist when every trace of primary or secondary symptoms had disappeared, especially in a case of chancre upon the neck of the uterus. I should still however add that the health of the foster-father is not always, alas! a sufficient guarantee. I have known for a long time how to consider the pastoral sayings about the pure manners of the country.

The child may be born with hereditary syphilis; child and nurse have nothing as yet apparent; but in some weeks or months we shall see secondary symptoms manifest themselves. These may appear in the infant before, during or after a similar manifestation in the nurse. So that the first in whom the manifestation shall take place, will accuse the other, if they do not both accuse each other at the same time, which frequently occurs. Neither are in the right; there is merely a simultaneousness, a coincidence, and with attention and patience we shall succeed in discovering the truth.

It happens sometimes that nurses contract syphilis during nursing, and the contagion can infect them through different parts. Most frequently it is by the genital organs. This fact is not uncommon for nurses who come frequently to Paris. Under these conditions the nurses infect their infants by the aid of their fingers contaminated by the virus. They infect even their husbands, and in these cases the cause of the evil is always referred to the *Parisian child* — to those *rotten* children, as these unchaste nurses are in the habit of saying. It happens very often to M. Cullerier and myself to make our observations simultaneously in our two hospitals; he attends the woman at the Lourcine, and I attend the husband at the Hospital du Midi. These poor rustic husbands, besides, have an extreme candor about the origin of their verole. The infant is invariably for them the origin of all the evil.

A mode of contagion quite common with nurses is the inoculation of the virus which they themselves convey to the nipple. Affected with a genital chancre, they carry their fingers to the diseased parts, they soil them, and then, without previous washing, they draw upon the nipple, more or less irritated, and thus implant a chancre, which they do not fail to transmit to the child. The position of these mammary chancres, of which I have recently seen a very beautiful example in the wards of M. Cullerier at the Lourcine, is very well explained by the manner in which women handle the breast in giving it to the infant. I have caused another very beautiful example to be designed in the *clinique iconographique* (19e *livraison*).

Here is another means of contagion in nurses. I have met with one in whom a chancre had been communicated to the nipple by an individual affected with a primary chancre upon the lip, and who thought that he should render a good service to this woman in drawing off the milk by suction. Very recently there was a young man in my hospital having a primary ulcer upon the mamma, with numerous and indolent swellings of the axillary glands, which were followed at the end of six weeks by an enlargement of the posterior cervical glands, and by a confluent roseola. This young man had been contaminated by his mistress, who, with a chancre upon the lips, had lavished upon him some eccentric kisses.

Another way. I have seen a nurse come to Paris to claim indemnity for a syphilitic affection, which she said she had taken from the infant which she nursed. This woman had an indurated chancre upon the inner side of each mamma; these chancres were placed opposite to each other. As to the child, *rotten*, according to the nurse, it was simply suffering under a porrigo larvalis of the most common description. The parents, who were perfectly healthy, little satisfied with the accusation, and especially with the demand, resisted the pretensions of the nurse, from whom I obtained a complete confession. A man, *who was not her husband*, in the fear of begetting a child and altering her milk, had given himself up to acts upon her, which the pen refuses to trace.

An infant may contract *chancre* at the time of birth, if the mother is so affected at the period of parturition. This is doubtless rare, but it is not impossible. These chancres, which are very often apt to be confounded with secondary symptoms on account of their varied and unaccustomed seats, constitute, as we can easily conceive, focuses of infection for the nurses, and are afterwards offered as proofs of the possible contagion of secondary symptoms. What apparently comes to favor this manner of viewing things, is, that in endeavoring to go back to the source at which the infant could have been contaminated, if we arrive *too late*, we can find nothing upon the mother, the primary symptoms which she had at the moment of the parturition having had time to become cicatrized without leaving any traces. Then if the *legal* father has the remembrance of any blennorrhagia in his early youth, every thing is laid to the charge of inheritance. But what can we say, when we find nothing and have no confessions?

Infants at nurse may be infected by strangers, whom we do not suspect. They may afterwards infect their nurses, and before these latter could perceive the disease of their infant, and especially before they could recognize the nature of it, and account for what they themselves experience, the secondary symptoms, so prompt to develope themselves in young infants, could have already appeared, and masked the point of departure in a manner to render it not easily recognized. I remember a remarkable case of this kind, for which my learned brother and friend, M. Richet, Surgeon at the Hospital de Lourcine, consulted me a few years ago. It was concerning a little daughter of a lawyer of Paris, still entrusted to the care of her nurse, and who was affected with syphilitic ulcerations upon the *ano-genital regions*. The parents being perfectly healthy, and the nurse in a decidedly healthy state, although she might have been suspected, the question arose from whence could come the contagion, when we learned that a clerk in the house, at that time diseased, had the habit of seating this child naked upon his hands, which were often soiled, and which he had not always taken care to wash. Without this discovery, how would they have explained the disease of this little child, and who would they have accused if the nurse had presented any trace or suspicion of syphilis?

In all these cases, with habit and perseverance we shall be able to discover the source of the symptoms. But it is not always so. The mother of the child is perfectly healthy; the *husband* of the mother is irreproachable; the nurse is free from all suspicion; and yet the child becomes diseased with syphilis. In these cases where is the contagion? Permit me to cite to you a fact which may serve as an answer to this delicate question.

A young woman, accompanied by her husband, who was much older, came to consult me for her child which she had just taken from the nurse, and which was infected with a constitutional syphilis, which she accused the nurse of having communicated to it. The child was almost entirely covered with a moist, scaly syphilitic eruption; the region about the anus and the labia was the seat of ulcerated mucous tubercles. The child was six months old, and according to the nurse, it was at the end of six weeks that the first symptoms showed themselves.

However, the mother and the *husband* declared to me that they never underwent any contagion, and in fact, by a most careful examination, I could discover no traces. The nurse, examined in her turn, appeared to me perfectly healthy. Her own child, which she nursed at the same time with the sick infant, was in excellent health.

I was much embarrassed in my endeavors to find the origin of the syphilis of this child, when the next day I received the visit of a young cavalry officer, who came to consult me for a syphilitic plantar and palmar eruption with which he was affected. This officer interrogated me with a touching solicitude about the disease of the child which had been presented to me the day before, and he made me a confidant so far as he was concerned in this question; but as he did not know the laws of inheritance, he was surprised to have begot a diseased child, inasmuch, he said, as he thought himself cured, and as he had no symptoms of the disease when he had connection with the lady, who in fact had not been diseased.

After all that I have told you, my friend, you see how much reserve, prudence, care and attention are necessary, before accepting the contagion of secondary symptoms as a demonstrated fact. Do you not think with me, that in order to establish definitely this law in syphilography, other facts are necessary than those at present deposited in the annals of science? Yours, &c. RICORD.

FOURTEENTH LETTER.

My Dear Friend,—What did I endeavor to prove to you in my last letter? That observation had by no means demonstrated the contagion of syphilis from the nurse to the child, and from the child to the nurse, without the presence of primary symptoms: that nothing was less established than that pretended contagion of secondary symptoms, and that in all the cases invoked as a proof of this mode of transmission, either the essential details were wanting to produce conviction, or evidently it was a question of primary symptoms.

Mark well, I beg of you, that I do not reject absolutely this mode of the transmission of syphilis. I only say, not quitting the field of strict observation and the rigid analysis of facts, that the existence of this mode of transmission is not yet proved, and I add that if it is ever proved, it will only be by inoculation; inoculation alone being able to furnish the undeniable demonstration of this, and to put the subject forever at rest.

But are you going to say to me—do you forget, then, that some persons pretend to have proved the contagious properties of secondary symptoms, even by inoculation? No! certainly not, I have not forgotten it. I wish that I could. I should not thus find myself under the painful obligation to cast too well founded doubts upon experiments made by men whose works I honor, but who appear to have concluded upon this subject a little too suddenly.—Judge of it:

Wallace has published two observations of secondary inoculation followed by results which appear positive. This writer upon syphilis truly says (*Syphilidologie* de Behrend, 1841, page 60 et suiv.) that he has produced primary symptoms, followed at a later period by confirmed secondary, in healthy individuals inoculated with pus taken from patients laboring under the influence of secondary symptoms. It is very certain that as far as results, the observations of Wallace have at first something plausible. But what is not at all demonstrated, is the nature of the symptoms reputed secondary in the patients from whom the inoculated pus has been taken. Here, the most important details are wanting. They are content with saying that in the first observation the patient had syphilitic *psydracious* pustules of fourteen days' standing. In the second observation the same pustules are mentioned as dating from four weeks, and forming little crusts. In the first case, the subject was inoculated upon the shoulders; in the second, upon the prepuce.

But, first, nothing proves that the psydracious pustules from which Wallace had taken the pus were secondary symptoms. The form, the number, the seat of the pustules, would not suffice to give them this character; for this, something more is necessary, which we do not find in the observations of Wallace.

On the other hand, what precautions did he take after having inoculated? In a venereal hospital, where we find the virulent matter everywhere, the subsequent contacts are very easy; and if after artificial inoculation the punctures are not guarded from every contact, as we are in the habit of doing, by placing them under a watch-glass, and causing this *syphilitic grain* to germinate under cover; if the instruments of which we make use have not been washed with the greatest care; if, in a word, the most minute precautions have not been taken, it is impossible, in circumstances so serious and important, to draw strict conclusions.

I am much the more exacting in these observations of Wallace, inasmuch as there passed something unusual in the results of the inoculation.

In the first subject inoculated, the 15th November, *it is not until the 14th December following*, that there formed upon the place of the inoculation a little papule, covered with crusts, below which a small superficial ulcer was discovered. From this the evolution of the symptoms described by Wallace, and which might well have an entirely different origin.

In the second subject inoculated upon the prepuce the 1st of June, *it is not until the 28th of June* that a little crust of a dirty-yellow color, surrounded by an areola, is found upon the parts, until then abandoned to themselves without any precautions being taken. The glands in the two groins are swollen, the spot covered with crusts is scarcely excoriated; the 24th July, the entire body is covered with an exanthema, the characters of which appear to be syphilitic. At a later period, some symptoms are discovered about the anus, the origin of which is not ascertained; without doubt from the description, these symptoms greatly resemble the mucous tubercles, and these tubercles exist also upon the scrotum, upon the back of the tongue and upon the tonsils; but the raphé of the patient is *red and much tumefied*; the patient says that in walking, a *very considerable oozing takes place from the anus.* Now, the tumefaction of the raphé and the intra-anal suppuration are often met with in the chancre or primary ulcer of this region. The primary symptom contracted *à preposterâ venere* has for its favorite seat the anterior portion of the anus where the raphé meets it. There is, then, in the case of this patient, more probability for the existence of a primary symptom which had commenced in that region, and about which no previous inquiries had been made, than there is in placing the commencement of the disease in what had been observed upon the prepuce, which had presented none of the symptoms by which syphilis commences. I add that in well-made inoculations, the evolution of the symptoms may be sometimes slow, but it is always constant, and we never see the interval of *a month* or *twenty-eight days* between the inoculation and the appearance of the symptoms.

Thus, what reasons there are for doubt in these two observations of Wallace! After the analysis that I have just made of them, I cannot think that they will still serve as a support to the doctrine of the inoculation of secondary symptoms.

I have just told you of the possibility of *an anal chancre* in the case of the second patient. This supposition appears to me to be so much the better founded, as that in England they seldom search for this seat of chancre—the English medical customs reflect that sort of far-fetched modesty which characterizes this nation. I recollect that in a trip to London, they showed me at St. Bartholomew's hospital, with much earnestness, some males and females affected with secondary symptoms which were considered as the immediate result of contagion. My friend Dr. Acton was present at this exhibition. You are aware that I think infinitely little of constitutional syphilis d'emblée, by way of contagion; so that, making use of my *right of search*, I commenced. I still laugh at the startled air of the house-surgeon and his assistants, when carrying a bold finger and a scrutinizing look into certain mucous folds, I succeeded in discovering in the *perfidious Albion*, a back door. I ought to add, that immediately the house-surgeon threw a veil, or, less poetically, let fall the sheet upon these too visible marks of a contagion very easily explained.

To return to Wallace; it is very singular that he who has made such a great number of inoculations, has succeeded in inoculating secondary symptoms in two cases only, and that he has so badly demonstrated these. These cases constitute an exception, and there can be no exception here. The secondary symptoms either do or do not inoculate. Please to recall what I have said upon those cases of blennorrhagia of Bell reputed exceptional; there can be for them no exception, and experimentation has in fact proved that the *exceptional cases* came under the law of inoculable chancre.

But if the facts which have passed upon the other side of the channel can, as I think to have proved it, give rise to very reasonable doubts, here is a fact which has taken place very near me, and which appears to present more value.

It was at the Hospital du Midi that this fact occurred. I should not have the liberty to speak to you of this, had not an interested party, too interested in fact, given me the right.

It is concerning secondary symptoms inoculated from a patient upon a healthy individual. The inoculation has perfectly succeeded. One of our brethren, who without being a *casuist*, is not, however, favorable to experimental researches, has himself practised this inoculation, and has planted upon each of the fore-arms of one of the internes of the hospital a chancre which has indurated, and which has determined the indolent enlargement of the axillary glands, and which, in the four months that followed, has given rise to perfectly well-characterized secondary symptoms, nocturnal cephalalgia, falling out of the hair, scabby eruptions upon the scalp, mucous tubercles upon the velum palati (psoriasis of the mucous membranes), &c.; it is the constitutional verole, the least contestable possible, and, moreover, I have no desire to contest it.

But — and therein lies the whole question — of what nature were the symptoms which furnished the pus inoculated? The patient from whom the inoculable matter was taken, according to the observations which have been given me by the interne inoculated, was affected with an indurated chancre of six weeks' standing, which had cicatrized; he had mucous tubercles about the anus — ulcerations about the great toes, numerous pustules upon the thoracic region; large pustules covered with crusts, below which, ulcerations *progressing and having a tendency even to spread*, were seen; there existed some of these in the inguinal regions and upon the side of the chest where the principal group was seated.

Before the pupil was inoculated, the pus of these pustules had been inoculated upon the two thighs of the patient himself. This inoculation had given a positive result, a circumstance which, *without a great passion for experimenting, ought to have prevented the inoculation upon a healthy individual.*

This patient had, then, very certainly, a constitutional syphilis, which presented characteristic symptoms, and of a nature incontestable. *But were all the symptoms in him absolutely of the same nature?* The constitutional verole, as we know, does not in any way prevent the contraction of new primary symptoms, unlimited in their number, and infinitely varied in their seat. In this particular case, the parts from which the pus had been taken, which were very extensive; *ulcers increasing*, and covered with crusts, in an individual only six weeks under the influence of the syphilitic diathesis, offering in the other regions the regular evolution of secondary symptoms of that period, permit me to express a doubt, which for the student who has undergone the inoculation, is to-day a certainty, viz., that the ulcerations from which the pus had been taken *were not secondary.*

I did not see the patient who furnished the inoculable pus; he soon quitted the hospital after this experiment, and *the pupil interested could not find him again.* But the importance of this fact, however contestable it may be, has induced us, my honorable colleague M. Puche and myself, to recommence a series of experiments upon the inoculation of the secondary symptoms. We have already made twenty experiments, all of which have only afforded us the results formerly obtained, that is to say, *negative results.* The inoculations have been made with the pus of mucous tubercles, of ecthyma, of rupia, of ulcerated tubercles, of secondary serpiginous ulcerations; never have we obtained any positive results. Here, upon this subject, are two curious observations which the numerous students who follow my clinique have observed.

Two patients, lying side by side, ward 1st, Nos. 16 and 17, had, No. 16 a scabby ulceration upon the axillary region, progressing and serpiginous; the other, No. 17, an ulceration upon the posterior and right side of the neck, of from six to eight centimetres in diameter, progressing, healing in the centre and extending itself in circumference; this patient had still upon other regions, isolated rupia, confluent ecthyma, and upon the greatest part of the trunk and of the limbs he had characteristic cicatrices due to pustulo-crustaceous syphilitic eruptions.

These two patients were inoculated upon the thigh. Upon No. 16 the inoculation succeeded; success had been predicted: upon No. 17, we had announced that the inoculation would be *negative — it was negative.* Why? Because that the ulceration of No. 17 was truly secondary; while in the case of No. 16 the scabby ulcerative eruption of the axillary region, which had the aspect of pustular crustaceous eruptions belonging to constitutional syphilis, had itself been the result of an inoculation; and mark how. This patient had at first a scrofulous abscess in the hollow of the arm-pit; this abscess had been opened at the hospital; the dressing of it was difficult for the patient himself; one of his neighbors, affected with a phagedenic chancre of the genital organs, rendered him the service of dressing it, and, with his fingers soiled by *the virulent pus of his own chancre*, had inoculated him. Without the very precise history of this case, the patient having had himself formerly symptoms of constitutional syphilis, this accident could have been attributed to the diathesis, and have been given as an example of secondary inoculation.

See, then, what care and precautions are necessary in order to avoid error. Yours, &c.

RICORD.

FIFTEENTH LETTER.

MY DEAR FRIEND,—From the numerous observations collected with care, from the many experiments made by myself, and from the more numerous ones still, made after my example, I have the right to conclude, that up to this day, secondary symptoms have not been inoculated. I have told you that the new experiments which I have very recently made; that the experiments again repeated by M. Puche and by M. Cullerier, have remained confirmatory of the first. But these experiments having been always practised upon the patient himself, a capital objection might be made against me. It may be said, the secondary symptoms cannot be inoculated in those who are already affected; but they can be perfectly inoculated upon a healthy individual. This objection could be made even by those who partake of my doctrines; for I do not think that it has entered the mind of all the school which is opposed to me, and which professes, that, so far from syphilis preventing a new contagion, it is sufficient to make a simple wound in a syphilitic patient in order that this wound should take on immediately a venereal character. I have already elsewhere spoken of this, and I shall ask of you the permission soon to recall what I think of this opinion. However it may be, the first objection remained; and if the observations of Wallace had been more probable, and less contestable, I should have taken the trouble to answer them, for I was completely destitute of experiments which proved the contrary.

It is under these circumstances that the fact of the inoculation from a diseased to a healthy man has been presented, of which I have given you a sketch in my last letter. I have spoken of this fact upon the special authority of the person the most interested in it, he who has voluntarily submitted to the experiment, who undergoes the consequences of it, and with a justice which we cannot reasonably contest, raises up pretensions to the scientific right in this fact, who believes that he has become absolutely the master of it, and that he has the right to draw from it all the scientific and practical consequences which he shall judge proper, leaving to all the liberty to do as much; it is, I say, under these circumstances, that I have thought myself permitted to give my opinion upon this fact.

I repeat, then, that this fact has appeared to me very grave, very serious, and well worthy of being taken into consideration; this is the reason why I have wished to examine it with care. We do not pre-occupy ourselves with common facts, and those which are without value. This one derives its importance both from the nature of the experiment, which might have great influence in the elucidation of grave practical questions, and from the individual who has submitted himself to the experiment. It is an interne in pharmacy, a distinguished and intelligent pupil, who has been occupied with medical studies, and more particularly with syphilis. I considered the fact merited our attention, on account of the experimenter, whose science, talents or character, as you know, dear friend, I have never wished to attack. If needful, you could bear witness to this. I have always deeply detested attacks of this kind, not only because they have often been unjustly employed against me, but because it is not my custom to make them, and because my disposition is repugnant to them.

In these letters, rapidly conceived, and more rapidly written, benevolent expressions might sometimes fail me, but good intentions never. Let this be said once for all, and put to silence susceptibilities which have no right to exist.

I return to the scientific fact which alone occupies me — all the value and all the importance of this fact is in the diagnosis. Has the pus of a secondary or of a primary syphilitic symptom been inoculated upon a healthy individual? I think, and I have given my motives for the opinion, that from this fact alone, viz., that the patient who furnished the pus has been himself inoculated with a positive result, this experiment enters completely into the domain of those which I myself have made. Thus, if success has been met with in this case, it is because, according to my numerous experiments, we have had to do with primary symptoms. Unless, which I do not contest, *but which remains to be proved*, we have discovered for the inoculation of secondary symptoms, *a particular form, a special period*, which until now has escaped us, and which we ought to be able to determine definitely.

For this result cannot be an exception, or the effect of chance. If we can succeed in establishing the conditions in which secondary symptoms can be inoculated, and therefore contagious, we shall have made a great step in syphilogeny, and rendered a great service to science. In all cases, this experiment will confirm this law—that a symptom actually contagious is inoculable; that there is no difference between artificial and physiological inoculation. It would prove that this mode of experimentation can truly have some value, and it would be for me a veritable pleasure to see those persons who have made the most of the *uncertainties and the difficulties of inoculations* ranged under this opinion.

Let me tell you, that I have no intention, as you well see, of changing my position. I do not attack, I defend myself. I do not criticize, I examine. I am not ambitious for the success of the *polémiste ;* I hold to the more modest pretensions of the practical observer. Nobody is more ready than myself to receive light, from whatever source it may come ; or to recognize the truth, whatever may be the voice that proclaims it. I have always uttered what I knew, or thought that I knew, with loyalty and firmness. My experiments I never made secretly ; they have become the property of all, they have enjoyed the right to see them, to judge of them, and to discuss them, and certainly in justice they have not found fault with them ; and without asking me permission, since they were common property. I have entertained opinions which time and experience have modified. I shall cite an actual example of this, and one to the point.

With all the earnest writers on syphilis, past and present, I have thought that *syphilis was not transmissible to animals.* I have made experiments, which like those of Hunter, of Turnbull, and especially of M. Cullerier, who has made more numerous ones, have always conducted to negative results. All these experiments gave me the right to conclude upon *the non-transmissibility of syphilis to animals*, until the contrary is proved.

However, *I was not too hasty to teach and to publish these negative results*, as M. Robert de Welz has imagined, since I had also on my side the essays of Hunter, of Turnbull, of M. Cullerier, and moreover the numerous unsuccessful experiments, publicly stated, of M. Auzias Turenne. M. Auzias has experimented perhaps more than all of us together, and had also more numerous negative results. But more persevering in his researches, he has studied the conditions which could prevent the inoculation of animals; he says that he has recognized them, and that he has at last succeeded in inoculating the primary symptoms from man to the monkey, and in return from the monkey upon man. M. Auzias assures us that one of the principal causes of the want of success was, that the animals licked themselves after the inoculation. He had thought, originally, that the saliva neutralized the virus; but this opinion could not be entertained in presence of the numerous instances that we see in man, of the primary symptoms, which have for seat the lips, the tongue, and different points of the buccal cavity. The whole secret was, that the animals, in licking themselves, must necessarily cleanse the wound of the inoculation.

But the true reason which must have caused the experiment to fail, and upon which M. Auzias Turenne insists the most at the present time, is the very great plasticity of the blood in animals, which allows it to interpose itself between the bleeding part and the virulent matter. It is in taking care to constantly soak the wound with pus after the inoculation, that it has succeeded. I have witnessed the experiments, and I can vouch for the authenticity of them. It is with zeal that I have rectified this point in the history of syphilis, in my clinical lessons.

Until then, I had professed, with our predecessors and with our cotemporaries, that syphilis was the unhappy prerogative of man, and yet that it was not spontaneous in him. I have always greatly insisted upon these two facts, which appear contradictory, *specialty of the disease in man, and not spontaneousness*. I have always thought that syphilis had an origin somewhere, and that it was necessary to search for it. Is the problem solved? The monkeys have not always escaped from wicked insinuations. Already Overcamp and Linder had accused them of playing an evil trick upon the human race, by giving it syphilis; but before M. Auzias, Overcamp and Linder have been considered as calumniators of monkeys. Were they right?

What is incontestable is, that since man was acquainted with monkeys, since he has seen them multiply in the Garden of Plants in Paris and in other capitals, since he has observed them, either in a state of nature or in captivity, nothing has ever been seen upon them or among them which resembled primary syphilis, and more especially constitutional syphilis.

However, M. Auzias has succeeded in planting upon the ear of a monkey a primary ulcer. The pus which served for the inoculation having been taken from a patient in my wards, I ought to note with care the circumstances in which this pus was taken. The patient who furnished it, was afflicted with confluent chancres upon the gland, upon the prepuce and upon the rectum—*non-indurated chancres*, and at the period of specific progress. These chancres were the result of a recent contagion in an individual under the influence of a constitutional syphilis, at the secondary period; and this is very important to note, for according to the principles that I have given out, this circumstance explains why the chancres were not indurated in this patient. Again, these chancres, by their *multiplicity, by the variety of their seat*, could have been, in the eyes of inattentive or of superficial observers, confounded with other constitutional symptoms, and served as a pretext to conclude upon their possible inoculation.

A previous inoculation had been made upon the patient and had succeeded. It was with the pus of the pustule of the inoculation that the monkey was inoculated the first time. A second inoculation was made upon the monkey with the pus of his first pustule, and this second inoculation again succeeded.

It was then that one of our young brethren interposed. M. Robert de Welz, associated professor of a German university, asked to be inoculated, and was effectually inoculated—first, with the pus of the first pustule of the monkey, and then with that of the second. These inoculations succeeded. But until then, the patient who first furnished the pus had not had any specific induration ; the monkey, whose pustules became a little thickened, *had not presented the certain characters of this induration ; the neighboring glands were not enlarged ;* finally, our German brother, who of his own accord submitted to a perilous experiment, in whom, moreover, the pustules of inoculation were not destroyed until at quite a late period, had not experienced the *specific induration.* The pustules of inoculation presented, at their base, a very common sub-phlegmonous engorgement, but one which might often be confounded with specific indurations by inexperienced observers. The axillary glands (the inoculations having been made upon the two arms) were not enlarged.

For the inoculation at which I assisted, and which was made upon M. Robert de Welz, a new lancet was used, but the pus upon the monkey was taken up with a spatula which was not new. Since then, M. Robert de Welz has made a new inoculation, with new instruments, which succeeded.

Thus far, then, we have only purely primary symptoms, essentially local; but this is not yet the verole. Has the monkey served only as a soil for the transplantation of the chancre? This is very possible. We have the right to think so, until we succeed in producing in him constitutional symptoms. This opinion is so much the more maintainable, inasmuch as many writers on syphilis, especially in England, pretend *that the chancre which does not become indurated* is not a syphilitic symptom. Will the experiments of M. Auzias come to confirm this opinion? I shall inform you at a later period what I think of this, and what I think upon the induration of chancre.

However it may be, I shall say to you, meanwhile, that if the *primary symptoms* incontestably inoculable upon man, can be inoculated upon the monkey, the *secondary* symptoms ought also to be inoculated, if, perchance, they have very recently become inoculable.

Is there, then, a versatile character for each particular disease, as for the epidemics in general? Or, rather, is it not the genius of observers which changes?

Yours, &c. RICORD.

SIXTEENTH LETTER.

My Dear Friend,—Most decidedly *we cannot please everybody;* and this old adage, so ingeniously presented by La Fontaine, is particularly applicable when medical science is concerned.

The monkeys have brought me ill luck; I have not satisfied the experimenters, who have pretended to have inoculated them with syphilis, and I have much less satisfied those who do not believe in this pretended inoculation.

However, see how mistaken I was, since I had the naïveté to think that from these two parties I merited some praise. You will see what my error was.

The young Bavarian colleague who has just inoculated his name with syphilis, has reproached us, myself and others, *of having been hasty in our conclusions upon the non-transmissibility of syphilis to animals.* However, if I count correctly, more than *twenty-four hours* have elapsed since Hunter, and the time has been sufficiently long for me to reflect, and that, too, without too much precipitation.

On the other hand, the colleagues whom I respect, and who ordinarily entertain the same ideas with me, have reproached me in almost the same way. They have discovered that I have been a little hasty with the monkeys; they believe—they tell me—that I have yielded to apish tricks. My learned and able colleague of the Hospital du Midi, M. Puche, is yet in a state of perfect incredulity relative to the transmissibility of the syphilis to animals, nor does M. Cullerier, that persevering experimenter, believe in the truth of experiments which make so much noise.

What I recounted to you in my last letter, I have seen with my own eyes; I have also told you the attenuating circumstances, which it is impossible to put to silence, however satisfied of the convictions and good faith of M. Auzias Turenne. But after having told you of this fact of the inoculation of the virulent pus from man to the monkey, all that I know of the matter, *I am astonished at the sudden and premature conclusions which our German colleague draws from the fact;* and to speak frankly, he who exacts in others so much maturity of reasoning and reflection, has not himself set the example. After all, the promptitude of his conclusions can be excused on the ground of the very inoculations to which he has courageously submitted himself, and which he would have been very glad not to have made without some use.

Our German colleague makes much of this proposition: "*One single positive experiment has more value than an innumerable number of negative results.*" Without doubt; but upon one condition, which is, that this experiment should be positive, that it should be incontestable, and that it should present all the guarantees of certainty and exactitude, and, more than this, *that it can be repeated;* without all this, it is worth nothing. The Academy of Science knows the value of this proposition which is constantly brought forward, and by which, periodically, rash and new experimenters pretend to overthrow the laws of physics. This argument has served for all human deceptions.

What says the magnetologist who pretends to transport the sense of sight to the nape of the neck or to the epigastrium? Precisely what our German colleague says—viz.: one single positive experiment, &c.

What says the homœopathist, who maintains that an atom of *bryonia* diluted in the immensity of the waters of the ocean can cure pneumonia? Precisely the same thing as our German colleague.

In the physical and natural sciences, *one isolated fact is worth nothing, if it is not susceptible of being repeated.* This is what all those think who know what the philosophy of science is. Otherwise this would be the most dangerous and the most perfidious stumbling block to progress, if laborious and patient observation did not come in to prove that it was but a sophism, an error, and often only a boast.

My honorable colleague and friend, M. Cullerier, ought himself to tell you what he thinks of the experiments of M. Auzias. As to myself, I have established this: that the virulent pus has been transported from man to the monkey, and from the latter it has been inoculated upon man; nothing more, nothing less. Here is the plain fact; afterwards comes its interpretation.

I said to you in my last letter, "*Might not the monkey herein have served only as a soil for transplantation?* I believe so, for this is what happens—the puncture of the inoculation which has been made upon the monkey, and which is scarcely irritated, scarcely inflamed, and suppurating but very little, although soaked in the virulent pus after it has been made, has a constant tendency to heal up, and this happens with astonishing rapidity. We do not see in the inoculation made upon the monkey that ulcerating, continued, increasing progress which is the character of the chancre upon man, especially the chancre which does not become indurated; we do not find even that period of specific *statu quo* which is so tenacious, so long, which nature keeps up in man, and which he has ordinarily so much difficulty to destroy. There is never in the monkey the least phagedenic tendency; nothing which resembles the specific induration in its commencement and in its consequences. A puncture, scarcely any suppuration, a crust, and a cure! Herein are the effects of inoculation upon the monkey—and all this takes place almost as quick as one of his gestures. We see that it is for the chancre a refractory and foreign soil; the virulent seed is there exotic; in vain do we take much precaution to sow it well, water it, to place it in a green-house, or under cover; it dies before having thrown out any roots, and consequently without having given forth any fruits.

M. Auzias explains all this by the great vitality of their circulation; it would be more easy to explain it by their nature so averse to syphilitic virus, upon which I congratulate them. We can even believe that in the pustule which is produced with so much difficulty, the virulent pus serves only as an issue-pea which irritates, causes suppuration, but is not combined with the tissues; it is mixed with the pus which is produced, that is all. It would be necessary, in order to be able to conclude definitely upon any other result, that the pustules produced upon the monkey should be broken, that the ulcerated surfaces should be frequently cleansed, in order that we should not suppose that there remained some pus of the chancre mixed up, and that we inoculated afterwards the suppuration furnished by these surfaces. We know what happens in man. We may in vain cleanse the surface of the chancres, apply to them even medicated substances ; still the virulent secretion continues to be produced. As long as we shall not have carried out this experimental programme, the sole experiment which has been made, will be insufficient to destroy all which has been established by serious men upon numerous and well-observed facts. The sole acquisition made to science, and which I am perfectly ready to recognize, is, that we can place and preserve the virulent pus upon the monkey, and afterwards make use of it to inoculate man, as one transplants a plant from one soil to another. That is all which I have seen and established, and the only deduction which I can draw from it.

Until a new order of things, then, our German colleague may truly be in the same condition as regards his inoculations, as if they had been made with virulent pus preserved in tubes or between two layers of glass.

This induces me to tell you what the pus inoculated upon man, produces, the course which inoculation follows, and what it teaches as regards the pathology of chancre.

But you inform me that my honorable colleague and friend, M. Cullerier, asks of you permission to speak. I yield to him with pleasure ; we shall all gain from it.

Yours, &c. RICORD.

July 24, 1850.

To M. Amédée Latour, Editor of l'Union Medicale.

Much Esteemed Colleague,—The principal topic of conversation lately, in the special hospitals, has been upon syphilitic inoculations made from man to the monkey—inoculations which have been pursued with so much ardor by our esteemed colleague, Dr. Auzias Turenne. This question is full of interest to me; for although certain persons do not appear to hold it of much account, everybody cannot have forgotten the numerous experiments which I made upon this subject a few years since. Satisfied with what those experiments had taught me, I was not a little astonished at the new results announced, when the last letter of M. Ricord came to give them a great value, and to furnish to experimenters a powerful lever wherewith to upset all that I have advanced. Permit me, then, to tell you my thoughts upon the facts given by M. Auzias.

At the time of the first exhibition, which was made in 1845, at the Academies of Science and of Medicine, as also before the Society of Surgery, of a monkey presenting upon the face the results of the inoculation of the chancrous pus taken from man, it was generally granted that these ulcerations presented all the appearance of veritable primary chancres; borders cut perpendicularly, greyish bottom, induration at the base; nothing, in fact, was wanting to them, and they already ridiculed the experiments of Hunter, of Turnbull, of my father, of M. Ricord, and of others. I was the only one that reserved any opinion as to the nature of these ulcerations, remembering that I had produced identically similar ones upon certain patients, without an atom of virulence; hence I immediately commenced a series of experiments.

I made them upon different kinds of animals, and especially upon the monkey. I inoculated either by a superficial or deep puncture, by incision, or by a solution of continuity more or less extensive. I constantly failed. M. Auzias attributed my ill success to my manner of manipulating; he told me that I went the wrong way to work. I begged him to operate himself in my presence, but established this condition, that he should not constantly irritate the wounds he should make. He operated as I did, by puncture, incision, and by excision. Like myself he suffered the virulent pus to macerate for whole days in these solutions of continuity. Two or three times he believed he had obtained a fortunate result, because a little inflammation was manifested; and in some of the punctures, a raising up of the epidermis, sometimes a purulent secretion, took place; but soon the negative result was evident to every one.

How do they now explain the results obtained? They say that one of the first conditions for success is to prevent the animal from licking itself, because the action of the tongue might cleanse the wound of the inoculation. But does not M. Auzias remember that in all my experiments this precaution was taken? Let him please to read again my work, which is inserted in the first volume of the *Mémoires de la Societé de Chirurgie*, and he will see that it is constantly stated that the animal was prevented from rubbing himself, and that the wound was made in such a way that the animal could not lick itself.

When I give myself up to experimentation, I do it with as much principle as any one, and I take all the precautions possible.

At the time when I made my researches, M. Auzias pretended that the skin of animals was endowed with much less irritability than that of man, and that in order to obtain a positive result, it was necessary to have a certain amount of irritation in the part where the virus had been deposited; and heaven knows that there was no lack of irritating the points which had been inoculated both by puncture and by abrasion. This explained very well to me both the delay in the cicatrization, and the appearance of the ulceration kept up by a mechanical cause. There is now no longer a question of that obtuse sensibility of the skin of the monkey; it is even pretended that it has become much more impressible to the virulence, than the human skin; but it is said that the cause of the failure in the experiments is the great plasticity of the blood in animals, which permits it to interpose itself between the bleeding part and the virulent matter; and in order to succeed, it is advised that the puncture of the inoculation should be constantly soaked with the pus.

Well—what is done then? What has M. Auzias done? He has caused a solution of continuity which became inflamed, which produced pus perfectly innocent at first, but which afterwards, and that quickly, became virulent by its mixing with the pus with which the wound was constantly covered, or with that, which, placed under the epidermis, or in the cellular sub-cutaneous tissue, acted like a thorn and determined in it a phlegmonous inflammation, not as specific pus, but as a foreign body. One can in this way produce successively a certain number of virulent pustules.

What became of the ulcerations upon the monkey? M. Ricord's letter does not say; it leaves us to suppose that they dried up and disappeared; so that there was only, as M. Ricord elsewhere appears disposed to admit, a simple depot of virulent matter upon the animal, which served as a vehicle between the patient of the Hospital du Midi and the courageous German colleague who submitted himself to the experiment. In a word, it is still the history of mediate contagion. The virulent pus, in place of being put upon an inert body, as in the experiments of M. Ricord, and as in some of my own upon mediate inoculation, the virulent pus, I say, has been placed and kept warm in the skin, or under the skin, of the monkey.

I have seen only a part of the results obtained by M. Auzias. This was the ulcerated pustules which M. Robert de Welz carried upon his arm, and which he had the goodness to show me one morning at the Hospital de Lourcine. It would have been perhaps in good taste, so far as science is concerned, if M. Auzias had made me participate in all the stages of the experiment, for he well knew my former labors, in which he took an active part. Does he not know, also, that in all this I am only stimulated by the interests of science, and that I profess the highest esteem for his character and for his talent. If he makes other experiments, I shall be most happy to follow them; but in spite of what has just passed, I declare in advance, that for me there will be no true inoculation of primary syphilis from man to the monkey, *until a suppurating ulceration shall be brought about, which can be washed at different times, so as to free it completely of the pus which shall have produced it, and which can be transferred afterwards either to the monkey itself or to man.* Until this, it will not be possible for me to see anything except a deposit, with or without production of suppurative inflammation.

This is not an exaggerated scepticism; it is a strictness of experimentation which appears to me indispensable, and which a *clinicien* of the character of my excellent colleague and friend, M. Ricord, who has accustomed us to so much accuracy in the observation of facts, and to so much logic in their deduction, will not be surprised to see me require. Yours, &c. CULLERIER.

SEVENTEENTH LETTER.

MY DEAR FRIEND,—I think that I have done justice to the monkeys; for the present, I shall not occupy myself any more with them. If, later, it can be proved to me that they can contract any thing but what I have told you, I shall be found always ready to acknowledge it. Until then, I do not see any motives to change my opinion.

In waiting, let us return to the poor human species, whose claim to syphilis as an inalienable right, no one at the present day contests.

However, before going farther, permit me, after all that I have said to you, and perhaps even after what might have been recently said, to establish the following proposition, which it appears to me impossible to overturn:

THE CHANCRE (PRIMARY ULCER) AT THE PERIOD OF PROGRESS OR OF SPECIFIC *statu quo*, IS THE ONLY SOURCE OF THE SYPHILITIC VIRUS (MORBID INOCULABLE POISON).

I have already told you in what conditions the virulent pus ought to be, in order to act; you know, also, the conditions in which the parts ought to be, in order to undergo the action of it. Let us now study the effects of this action; in other words, the pathogeny of the chancre.

This subject is a serious one, but a little dry. I depend upon all your good will, to follow my details. Please to look for no other interest than that which the question itself presents.

If we make a puncture under the epidermis, with a lancet charged with virulent pus, this puncture, which ought scarcely to bleed, soon grows red, becomes prominent, and its summit is raised up by the serosity, which soon becomes turbid in order to take on afterwards the characters of pus.

Thus, puncture, redness, papule already surrounded with an areola, vesicle, vesico-pustule, and finally pustule; such is the series, the constant succession of phenomena produced by inoculation.

All this follows without interruption, without any arrest, from one hour to the other, from one day to another; it is a pathological riband, which is constantly unrolling in order to arrive at a regular and inevitable period, that is, at the production of a pustule of *ecthyma*, the most perfect, and of the best possible type.

This pustule is often depressed at its summit, even umbilicated at the point which corresponds to the puncture, and upon which we perceive most generally a little drop of dried blood.

If the pustule is not broken, the pus which has formed, dries up, and gives rise to a conical, brown, greenish or blackish crust.

This crust tends to increase at its base; for it covers an ulceration, the circumference of which tends itself to increase.

In this increase of the ulceration under the crust, the epidermis of the areola which surrounds it and the border, is successively raised up by the suppuration; this latter in its turn dries, in order to form a new disk of crust, while a new areola is formed at its circumference, and so on.

Tell me, without ceremony, if I am sufficiently clear in this description; it is of great importance to me to be well understood.

The red circle (the areola) which borders the crust, is ordinarily tumefied, and encloses it as the rim of a watch encloses the glass — only, as there is here an increasing ulceration, and always new pus produced, and as the circumference of the crust is always less hard than its centre, this crust is not generally very adherent.

Sometimes the crust is formed early; at other times the pustule remains in the purulent state during a longer or shorter time.

This pustule sometimes does not acquire a very great volume; often it is at its commencement only the size of a lentil; at a later period its surface might equal that of a five cent piece and even that of a franc; and it is not rare to see it acquire dimensions much more considerable.

The pustule offers, then, those transitions which we observe so often in other forms, and which give to it the aspect of rupia, either before the formation of the crust or when the crust is formed. There is only here, as sometimes in rupia, a difference of volume.

If we break the pustule the second or third day in the cases of quick evolution; or if we break it at a later period in the ordinary cases; or if the crust is detached, we find beneath an ulceration occupying all the thickness of the skin, perfectly rounded, with the borders cut perpendicularly, as if it had been made with a punch.

The borders of this ulceration, slightly separated from the adjacent parts, tumefied, serrated, and turned back, remain surrounded by the red areola which constitutes the margin of it; they are covered by a diphtheritic layer, a special, adherent, pyogenic membrane.

The surface of the ulceration secretes a sanious, sero-sanious pus, often reddish, and charged with organic detritus; this is the virulent inoculable pus. When we cleanse this surface, we find a diphtheritic layer more pronounced than that of the borders, and which is also constituted by a special pyogenic membrane, of a greyish color, of a lardaceous aspect, and which cannot be detached.

Moreover, the bottom of the ulceration reposes upon a base more or less thick, more or less engorged, according to the progress which the ulceration is to pursue — a progress especially determined by the character of the *soil* in which the *syphilitic grain* has been sown.

The ulceration which I have just described, and which has followed an increasing progress, may arrest itself at the extent which I have already indicated, or persist a long time — a month, six weeks and more, or continue to increase in order to take on larger dimensions, and to present also important modifications.

In the numerous inoculations which I have made, things have always happened regularly, thus: — continued evolution starting from the puncture; constant production of an ecthyma, the ulcerating bottom of which, presents in its turn, *par excellence*, the classical and typical characteristics of the chancre; ulceration with a *tendency to increase*, or remaining in a special *statu quo*.

You already see that the artificial inoculation overthrows all that men have been accustomed to teach and to repeat to each other for ages past; you see it break through the physiologism of Broussais; you also see it reduce to its proper value the doctrine of the *physiologic contagion* of a more recent date.

And first, can the theory of incubation sustain itself in presence of what inoculation produces, and of those results which you can repeat every day; for, remark, it is not a unique, exceptional fact that I relate to you, but masses of identical facts always giving place to the same phenomena, and of which every body has the proof in their hands.

The electric, expansive mode of Bru is done with; it is no longer possible to believe that the syphilitic virus penetrates the economy like lightning, that it is a shock from the individual infecting, to the individual infected. The chancre, the primary ulcer, is no more the result of a *shock in return*.

We cannot admit, at the present day, unless we are blind, that the virulent pus traverses our tissues by a solution of continuity or otherwise, for the purpose of first infecting the entire economy and of hiding itself at a distance, in order to return afterwards to *hatch* in the *nest* where it had been first placed.

Special grain, the syphilitic virus, grows where it has been sown; *particular ferment*, it is those parts which it immediately touches that enter first into fermentation. All this takes place, as we have already said, more or less quickly, according to the disposition of the soil, according to the fermentable aptitude — but all this takes place strictly, absolutely, in a point at first very circumscribed, and which we shall contrive perhaps to limit by and by.

The non-existence of a period of incubation, a fact so evident, so true and so logical, is not yet, however, accepted; the contrary prejudices have been of too long standing not to have the force of law, or to be easily overthrown.

Those who, notwithstanding, sustain the incubation, and who believe that the virulence of syphilis is compromised if it does not exist, have made to me this principal objection: they say to me — If you obtain instantaneous and uninterrupted effects by artificial inoculation; if you have observed only a local evolution; if you have been struck by an apparent silence of the organism, and if you have perceived nothing which explains a general participation in the syphilitic drama, it is because you operate upon an organism already impregnated, infected; you inoculate patients, and those patients are already inoculated.

This objection, you see, enters into the famous theory of *virulent bottles*. I have already refuted it; I have told you what we ought to think of this opinion as respects wounds, injuries and operations made upon syphilitic subjects. I cannot help returning to it; permit me to refer you to what I have already stated upon this subject. But I have another answer to make to this objection, besides the experiments practised upon the patients themselves. I shall answer this by the experiments made from sick individuals to healthy ones, and I shall invoke especially the recent inoculations practised upon man on the occasion of the inoculation of the monkeys. Well, in these cases, the results of the inoculation have been identical with those which I have just described to you; that is to say, an immediate action, an uninterrupted evolution, and production of the ecthymatous pustule.

But does artificial inoculation always give rise to this uninterrupted series of phenomena? Are there not circumstances, in which, between the inoculation and the manifestation of the symptoms, there will be a period of rest, of sluggishness, as in the inoculation of the vaccine virus? In the contagion by the ordinary way, does there not always seem to be a time sufficiently long between the action of the cause and the manifestation of the effects?

Yes, without doubt, and these are the cases which can justify, in a measure, the theory of incubation. But when we take the pains to examine these facts attentively, we see that they have been badly appreciated. I shall try to reduce them to their true value, and to bring them back to the laws previously established.

I have already said that these cases have never happened to me, in my numerous experiments, always publicly made. This arises evidently from the uniformity of the proceedings which I have employed. My honorable colleague, M. Puche, who has experimented as much as myself, and perhaps still more, has only once or twice seen these appearances manifest themselves, at the second or third day after the puncture. All those who have studied the inoculation of syphilis, know that when it does not succeed immediately, it is because it is negative.

However, we can understand that a too superficial puncture, that the virulent pus placed upon surfaces scarcely denuded, would require a longer time in order to affect the part, and in order that the effects should be produced. Here is what I have observed upon M. Robert de Welz. A first puncture, very superficial, was made, which produced no effects the first days, so that there was something which might resemble incubation. But the second puncture, which I made myself upon him, followed the regular course. The partisans of the influence of the general state would answer me, what of that? The first puncture had a slow development, because the organism was not yet impregnated. The effects of the second puncture have been rapid, on the contrary, because then the virus had invaded the entire economy. That is very well, I shall answer, but here is something which slightly deranges this beautiful theory; it is that M.

de Welz had a third puncture made, which being too superficial like the first, gave, like that, only tardy results.

Therein is to be found the key to incubation, my friend. We understand very well, without its help, how in the contagion by the ordinary methods, virulent pus placed upon surfaces more or less denuded, and consequently fitted to receive more or less quickly the virulent action, are affected more or less quickly, and give place to a morbid action more or less rapid. We know, and observation teaches us every day, and the experiments of M. Cullerier demonstrate it in an irrefragable manner, that the virulent pus can remain in contact with healthy surfaces without altering them, and without being altered itself; but we know also that surfaces constantly bathed by the virulent pus, acrid and irritating, excoriating before being specific — we know that these surfaces end by becoming eroded, and by being placed by this very pus in the conditions necessary for the action of the inoculation. This sort of vesication might require a longer or shorter time to be produced, before the special effects appear, and simulate incubation.

For example, some virulent pus is collected in a fold of the vulva, of the vagina, of the prepuce, in the interior of a follicle; it is not till a longer or shorter period after the pus shall have been thus placed, that, passing through the successive action that I have just shown, it arrives at the effects of incubation. There is nothing herein which is plausible; it is physical and material; it is what the observation *de visû* demonstrates every day to the eyes which know how to see. How many patients there are who think themselves at first only affected with a balano-posthitis, and in whom after more or less time we see chancres produce themselves. Add to this the carelessness of patients, the absence of all observations regarding them, a circumstance so common in practice, and which causes the time which has passed between the exposure to the cause, and its apparent manifestations, to be taken for the incubation. In this, you will find for the chancre, as for the blennorrhagia, the explanation of those pretended incubations with an elasticity of duration so considerable, that they vary between hours, weeks and even months.

You see that I enter more and more into the substance of these important and grave syphilographic questions. In my next letter I shall treat of the different forms which the chancre can assume.

May your good will, and that of your honored readers, still accompany me. This is for me the most valuable encouragement. Yours, &c. RICORD.

EIGHTEENTH LETTER.

MY DEAR FRIEND,—In the positive inoculations, things always occur as I have told you in my last letter.

When the inoculation fails, the puncture becomes a little irritated, but this soon disappears.

However, without depriving inoculation of anything which is fully established, we must recognize that there are *false pustules* for syphilis, as for the variola and for vaccina.

Their existence, if the examination is superficial, might lead to error. My learned colleague M. Puche confesses now, with a good grace, that he has been thus deceived by *false pustules*, when he formerly practised inoculations with muco-pus furnished by balano-posthitis. Thus, he does not attach to-day the same value as formerly, to the facts contained in the *mémoire* which he has published upon this subject; he has studied these facts better, and they have for him changed their signification. You ought to understand that I should not have committed the impropriety of speaking thus, if I was not formally authorized by M. Puche himself. My critics, then, who have made much ado about the inoculations of the muco-pus of the non-ulcerated balano-posthitis; who have made use of them as a weapon against my doctrines; who wish to prove by them that the chancre alone does not furnish inoculable pus, and that the blennorrhagia which is inoculated could not well be ulcerated—these critics can no longer make use of this argument without the new verification which its author believes indispensable.

These *false pustules* are but little developed; most commonly they are only simple bullous elevations, beneath which, we find a superficial vesication of the skin. Here, there is not that boring of the skin, as if done by a punch, as observed in true inoculation. In some very rare cases, deeper inflammation might appear and produce something analogous to the furuncle. But even in these cases, the progress is always very rapid, the duration slight, from three to five or six days at most, and the healing follows also very quickly without the intervention of any treatment.

However it may be, I have said, and I persist in saying, that when the inoculation has succeeded, it is always by a pustule that the chancre commences; this is what is incontestable, and what can be re-produced at will and with certainty.

However, the writers upon syphilis, who have ranged among the primary symptoms so many phenomena which ought not to hold place among them, might have here well placed this *ecthyma* developed under the conditions that I have already marked out to you.

It is true that our learned colleague M. Cazenave says that the ecthyma may be sometimes primary. He even cites, in his treatise upon syphilitic eruptions, a very beautiful example of primary ecthyma of the lip, the direct and immediate consequence of contagion. But what M. Cazenave says of this case, for me so frequent and common, proves to me exactly that neither Biett nor himself have understood the true nature of this symptom. Read again that passage of M. Cazenave, and you will be convinced that he does not consider, in this particular case, the ecthyma as being only a period of the chancre. For him, the ecthyma which he calls *primary* is always a *syphilitic eruption*; that is to say, the product of a general infection, constitutional—in a word, what I call a *secondary symptom.*

But in order to establish that the ecthyma is always the result of a previous general infection, although this may be the only isolated symptom by which the syphilis commences; in order to confound the chancre with ecthymatous commencement, the true primary ecthyma, *contagious, inoculable,* with the constitutional secondary ecthyma; M. Cazenave, after having so well said that this symptom may be the first and only result of the contagion which, "apart from the influence of the virus, has need, in order to be developed, of finding particular conditions," those conditions *which necessitate the inoculation of the primary symptoms*; M. Cazenave, I say, wishing, against his own principles, to place ecthyma among syphilitic eruptions, gives as examples of primary pustular eruptions, two observations where it was perfectly secondary, and regularly preceded by a primary symptom upon the fingers.

This error is very common among those individuals who do not know all the varieties of the chancre. Did not this happen to one of our unhappy colleagues to whom M. Cazenave makes allusion? Has he not been considered as having undergone a constitutional infection, *d'emblée*, and as having offered an example of primary pustular eruptions? And yet this unfortunate colleague had had a chancre upon one of the fingers of his right hand, a chancre followed later by an enlargement of the glands about the elbow, in the desired and regular order of secondary symptoms. All this I have myself established, and also my learned friend Nélaton. It is true that a person who had no very great experience with venereal maladies, although he had written much upon the subject, and who knew of the ulcerations upon the finger, pretended that there was nothing there but an *anatomical* tubercle, which had given passage to the virus without becoming inoculated. I much fear that the brain of this person has given passage to this fine story without becoming inoculated in passing, with a little probability and good sense.

I have not yet finished with the primary ecthyma. You who read all—sometimes from duty, often from taste, and always with profit to those who read you in their turn—you ought to be surprised to see in a *manual* upon syphilitic maladies, that the learned author, whom we both hold in high esteem, admitted the possibility of the production of a pustule by artificial inoculation, but not otherwise. In fact, M. Gibert absolutely denies that the chancre not inoculated artificially can commence by a pustule; he assures us that it is through an error of diagnosis that this period of chancre has been admitted. I believe that you already see upon what side the error ought to be. If you admit, I say to M. Gibert, that a pustule can be produced by the point of a lancet, agree that it does not require a great effort of imagination to find in the processes of ordinary contagion, something which may act in the same manner, such as a nail, a hair, &c., without taking into account other circumstances, the lewd and shameful confessions of which, you, in your quality of physician, must sometimes receive.

Nevertheless you see how subject to error the most distinguished observers are. Assuredly M. Cazenave and M. Gibert know as well as I what an ecthyma is, and yet how is it that they insist in referring it always to a general condition, and that they deny the existence of it as a product of chancre? Why? because that theory throws too often a deceitful gauze between the observer and the matter for observation; because that it does not suffice, as another observer has just told us, to pass ten years in a venereal hospital in order to see well all that takes place there; because, alas! there are eyes which always look and which never see.

I ask pardon, my friend, for having so long occupied myself with the pustular form of the chancre. Since I have done so, it is in my opinion time, at last, to come out from this *parrot's talk*, which invariably gives the same characters to the primary symptom as if it was immutable and eternal in its form. Nothing is more false, and more contrary to the observation of every day than this doctrine. The primary symptom, on the contrary, presents numerous varieties either at its commencement, during its course, or later. Permit me to recall here what observation and experience have taught me.

In the most common cases, chancre commences by an ulceration *d'emblée*, either superficial or deep. The primary ulcer does not always destroy the entire thickness of a mucous membrane or of the skin. Thus upon the semi-mucous membrane of the gland and of the prepuce, the ulceration may be sufficiently superficial to lead to the belief in an ulcerated balano-posthitis, and to justify certain success in cases of inoculation.

The ulcer *d'emblée* is produced when the virulent pus has been placed upon a surface recently denuded, upon a bleeding wound, or, what is more difficult and consequently more rare, upon a wound in suppuration.

Again, we see sometimes, and this has been disputed by those who are in the habit of disputing everything, the chancre commence under the form of an abscess. Thus, the bites of leeches which become inoculated, it is true, often offer an ecthymatous form; and it also happens that the virulent pus inoculates also the bottom of the bite without inoculating the borders of it; these could then become united, and enclose, so to speak, the virus which has inoculated the bottom, and this bottom then produces a little virulent abscess of the sub-cutaneous cellular tissue, which when it opens, or when it is opened, presents a chancrous collection. The fistulous tracks of the virulent pus in the sub-cutaneous or sub-mucous cellular tissue give rise to the same phenomenon.

All this is the true result of common practice and observation in my wards of the Venereal Hospital. I well know that in this theory so simple respecting abscess—as a form and as a primary period of chancre, an argument has been sought in favor of the existence of the bubo d'emblée, an existence which I do not admit, and which appears a contradiction in my doctrine. But I shall return, by-and-by, to these buboes d'emblée, and in such a way as, I hope, to satisfy my opponents.

However it may be as respects these different varieties in the commencement of chancre, they have no influence upon the ulterior form which these ulcerations will take.

This point has its importance; it becomes connected with the unity or the plurality of the syphilitic virus, a question yet quite obscure, or rather obscured by the vagueness and the want of precision in facts. Here is what I can say as regards myself:—

When the inoculation is made upon the patient himself, the commencement of the chancre being always similar, the ulceration which follows the inoculation finally takes the form, and offers the same varieties, as the first symptom which furnished the inoculable pus. Thus, if it is from a phagedenic chancre that the pus has been taken, the ulceration will take on the phagedenic character ; if from an indurated chancre, the ulceration will become indurated, &c. This is what my own experience has shown me. But in the inoculations which have been made from infected individuals to healthy ones, have things always passed thus? We know nothing, for in the inoculations which have thus been practised by other experimenters, they have taken note neither of the form of the symptom from which the pus was taken, nor of the form of the symptom which had been produced; they have been contented with saying, chancre on one side, chancre on the other, without any detailed description ; so that definitely these inoculations could not be of any great assistance in the elucidation of the question.

In common observation we find that one form in an individual can produce a different form in another. But as we are never strictly sure of the source from which the infection has been taken, we can dispute the results ; we can suppose that the individual who has a different form, could have taken it from another source, than that which he accuses. The results of the last inoculations which have just been made from individuals infected to those who are healthy, counterbalance, and cannot serve either for or against. In the observation of M. de Welz, the pus was furnished by a non-indurated chancre, and his chancres were not indurated, which circumstance in his case might depend upon a want of aptitude. In the case of the inoculation upon the interne of the Hospital du Midi, the chancre became indurated, and yet the pus with which he was inoculated ought to come from a primary non-indurated ulcer, if we take into considertion the conditions of the anterior constitutional syphilis under the influence of which the patient labored.

You see, my friend, that this question of the plurality of the virus, so clearly drawn by certain English physicians, is far from being solved. Until now, we were always entitled to the belief in the existence of one virus only; it appears always rational to admit that the chancre, under given circumstances, and such as can be determined in advance, commencing as it does always in the same manner, depends upon an identical cause, the ulterior effects of which are determined by conditions in which the individual is found, upon whom they are developed.

In fact, the great varieties which the primary ulcer presents at the period of progress, which are formed more or less quick, and which can be thus summed up:

Simple chancres;

Inflammatory chancres with decided gangrenous tendency;

Phagedenic chancres;

Indurated chancres; appear to find the reasons of their existence in secondary causes beyond the specific cause. I do not here give a lecture; I do not write a book upon special pathology; consequently I cannot enter into too long details. But, in order to justify my propositions, let me recall some of the assisting causes which give to the chancre such or such a physiognomy, such or such a course.

For example, observation shows what the abuse of alcoholic drinks produces, particularly in hot weather. The most simple chancres under their influence become rapidly inflammatory, and the inflammation in certain regions, as about the genital organs, in a cellular tissue which so easily becomes œdematous, arrives very quickly at gangrene. The action of alcohol in these cases, of which the English have given us such fine examples, is so pronounced, that we could call these ulcers "*œno-phagedeniques*."

As to the other varieties of phagedenic chancres—pultaceous, diphtheritic, serpigenous, &c.—we often find the cause of them in certain hygienic conditions, unhealthy habitations, bad nourishment, want of cleanliness; in the unseasonable employment and abuse of rancid mercurial ointment in the dressings; in certain diathetic conditions, tubercles, scrofula, herpetic condition, scurvy, and frequently in the different conditions which favor the production of hospital gangrene. Let us add to this, as we shall see later, the influence of a previous syphilitic diathesis.

However, the conditions most interesting to understand, those which in themselves almost constitute the verole, are those which preside over the *induration of the chancre.*

But the *indurated chancre* being one of the important points of the doctrine which I maintain, and which these letters are intended to defend, you will permit me to make it the subject of my next letter.

Yours, &c. RICORD.

NINETEENTH LETTER.

My Dear Friend,—If I made myself well understood in my last letter, you must see that although experiments have not yet demonstrated in an incontestable manner the unity of the syphilitic virus, I nevertheless admitted this unity; that I did not search for the difference of the primary effects of this virus in its greater or less activity and acrimony, as some writers on syphilis have done; that I sought for these effects, on the contrary, in the individual conditions of the persons who were to undergo the action of them; so that, in spite of some observations of Bell, and of some analogous cases that we still sometimes meet with in practice, and in which there is only a simple coincidence, we cannot conclude from the form and the gravity of the primary symptom of an individual, as to the form and gravity of the malady of the person who communicated it to him; and that, in fine, we can no longer at the present day, say to a patient, as we formerly said—if your malady is a serious one, it is because the person who communicated it to you was very much diseased; for very often it is the contrary which we observe.

This law of the unity of virus being laid down, I am going to occupy myself, as I promised you in my last letter, with the most important variety of chancre; the *indurated chancre.*

The knowledge of the induration, of that condition which certain primary ulcers take on, is not a new thing;

some pretend that we can even find the traces of it in Galen, which does not astonish me the least in the world, who believe in the antiquity of the verole. What is certain, is, that after the great epidemic of the fifteenth century, some writers upon syphilis observe upon this remarkable symptom; above all, it did not escape the observation of Jean de Vigo, who has other claims upon our esteem besides the invention of his famous plaster.

However, you know that it is to Hunter that we have given the honors of the discovery of the indurated chancre; this symptom has even received the name of the great physiologist. The Hunterian chancre, in fact, is no other than the indurated chancre. And yet Hunter scarcely glances at this subject; you recollect what he says—"the chancre has generally a thickened base, and although the common inflammation extends much beyond, yet the specific inflammation is limited to this base." But, as you see, Hunter does not make of this thickening of the base a constant condition; and he is right, for the greatest number of primary ulcers do not present this peculiarity. Neither does he make it a condition of the constitutional infection; a grave and inexplicable omission, for a man of the sagacity and of the divination of Hunter.

The writers upon syphilis who came after Hunter, even Bell, with his comparison of a *split pea*, did not recognize all the value of induration.

A greater portion of other syphilitic writers since Bell, have not paid attention to this symptom. M. Lagneau, in his treatise, does not appear to give to this any importance; however, to do him this justice, he has recognized, as Bell and others, that the chancre can have a pustular period; but apart from this, you will be struck like myself with that kind of confusion which characterizes his writings with regard to the chancre which he calls *primary*, and those which he calls *secondary*. In all cases, the induration is of no account for him.

As to M. Cazenave, "whose work, wholly circumstantial, and which we cannot take for serious," and whose courteous expressions which he recently makes use of towards me, I return, in order not to keep anything which belongs to him; as to M. Cazenave, you know his manner of appreciating the primary symptoms. It is truly beyond belief. Nevertheless, are there for M. Cazenave any primary symptoms, besides the *infecting act?* For him, in fact, the other symptoms are all either *primary secondary*, or *secondary primary*. Free yourself from this, if you can, in spite of all the talent of which you daily give proofs. In all cases, the induration, that capital phenomenon, does not appear to exist *upon the other side of the water*, as Lisfranc would have remarked.

And yet, who can to-day misconstrue the importance of this phenomenon? They have eyes which see not, who suffer this to pass as null and void, after all that I have done, after the judicious observations of the wise professor Thiry of Brussels, of my pupil and friend M. Diday of Lyons, of M. Marchal (de Calvi), of my learned friend and too benevolent partisan M. Venot of Bordeaux, of Messrs. Acton and Meric of London, of my learned colleagues MM. Puche and Cullerier; in fine, after the observations of my patients themselves in the hospital, whose education, made during twenty years, leaves few chances for the inattentive physician to commit errors.

The induration which can line the chancres and border them, meriting all the attention of the practitioner, permit me to study it with care.

All chancres do not become indurated; at the present day it is decidedly only the smallest number; and if my doctrines are true, this number will go on always diminishing.

But what is the individual cause—the necessary ulterior condition, in the insertion of the virus, which causes the chancre to become indurated?

Therein is one of the most interesting problems which the study of syphilis can present; and the solution of it is also one of the most difficult to obtain.

I have, however, the pretension to have discovered one of the unknown principles.

When we ask of age the cause of induration, age answers nothing.

Sex, temperament, hygienic habits, say nothing.

Nor do the anterior or accompanying maladies, foreign to syphilis, any more than the specific treatment submitted to by patients, come to enlighten you.

Until the present, then, we are obliged to confine ourselves to the common explanation that you know—that is to say, to aptitudes and idiosyncrasies.

In fine, we find that in certain individuals, a first chancre does not become indurated, while a second does, and those that they may contract afterwards do not become indurated.

Where, then, is the cause of this mysterious and singular condition?

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Let us search for one of the causes of these differences, which has passed until now unperceived, in the laws of venereal diseases, so general and so constant; let us search for it in the striking analogies which exist between the variola, vaccina and the verole.

We are upon the track.

The vaccina, for example, may fail at the first trial; this will be for the want of an aptitude of which we are ignorant; but if it succeeds, the ulterior failure in new vaccinations is explained; the diathetic effect of the first vaccination is not yet exhausted; a lapse of time is necessary, to render the organism fit for a new vaccinal impregnation, which modern observation tends the more and more to fix precisely.

Here is a capital fact in syphilogeny, a fact that a long experience demonstrated to me, a fact which has been equally observed by two men whom I love always to cite, MM. Puche and Diday. It is that

General rule, A PATIENT WHO HAS ONCE HAD AN INDURATED CHANCRE, WILL NOT HAVE ANOTHER.

It is probable that this law will present exceptions for the vaccina as well as for the variola. I should add that it is even desirable that it should present them, for this would prove that we can succeed in destroying the syphilitic diathesis.

Beyond all doubt these exceptions are more rare for syphilis, since MM. Puche, Diday and myself have yet to search for unexceptional proofs of them.

It is, that when there is an indurated chancre, there is necessarily *constitutional verole.*

With the induration, the syphilitic *disposition*, as Hunter called it, is acquired.

The syphilitic *temperament*, as I formerly called it, and as I have since repeated, is established.

Finally, there is a diathetic state, a special particular disposition, in virtue of which future manifestations will develop themselves.

Disposition, temperament, diathesis, which we do not double, or treble, any more than we treble the analogous disposition in the vaccina.

The indurated chancre is to the verole, what the *true* variolic pustule is to the variola; what the *true* vaccinal pustule is to the vaccina.

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

Herein, my friend, is an admirable law, a law which causes the verole to come under the general rules of virulent affections; a law which dominates over the study of syphilis, as variolic and vaccinal inoculations dominate over the history of variola; a law which satisfies the mind, and which safely tranquillizes it after a painful and fastidious voyage in the midst of deceiving hypotheses and of falsifying theories; a law, which arithmetic, so much outraged in its first rule by one of your old correspondents, will serve to establish in spite of him, since in order to have a real amount he will add up similar values.

But I am not now charged with the special education of the pupil *de province* of your honored correspondent; with teaching him to distinguish the difference which exists between a diathesis and the manifestations which this diathesis can produce; the difference between the diathesis properly called, and the cachexy, to all which things I shall without doubt have the opportunity to revert, and upon which I fear that this poor pupil has ideas a little clouded.

For the present let him know, and he will pardon me for this magisterial locution—that the diathesis acquired by the patient who has submitted himself to the infection, prevents a new chancre which he may contract from becoming indurated, and that this kind of immunity against this form of chancre—that is to say, against a new general infection—ought also to transmit itself by way of heredity. From this can be understood what I said just now; this disposition being transmitted may well have an influence upon the diminution of indurated chancres, and consequently upon the diminution of constitutional veroles. Therein is also a curious study as respects variola and vaccina. This idea, sprung from my school, has been well studied in a remarkable thesis sustained by a distinguished pupil of Val-de-Grace, whose name I do not for the moment call to mind.

Thus, then, the non-induration of the chancres which a patient may contract at different epochs, after having had an indurated chancre, is a primary proof, easy to verify by the statistics of the *unicité*—neologism of which I am not culpable—the *unicité* of the syphilitic diathesis; the *unicité* implicitly admitted by Hunter, since he has said that we could prevent the disposition from being established, but that it could not be destroyed once that it was established; *unicité* of diathesis which M. Cazenave did not suspect that he had proclaimed after us, when he wrote in his *Treatise upon syphilitic eruptions* "that he knows not if we have ever succeeded in destroying the syphilitic temperament." Truly, in good physiology M. Cazenave would not admit a double sanguine, bilious temperament, &c.; in good pathology, he would no more admit a double glanders, double variola, a single or triple hydrophobia. The *non bis in idem* is also in this connection a pathological law; in studying the evolution of constitutional symptoms, I hope to set it forth to the best advantage.

These points of doctrine upon the etiology of induration established, let us study this phenomenon at its time of appearance, its seat, its peculiar symptoms, its nature and its course, to arrive at last at the exposition of its consequences.

Such will be, dear friend, the important subject of my next letter. Yours, &c. RICORD.

TWENTIETH LETTER.

MY DEAR FRIEND,—The indurated chancre must still be the subject of our conversation. This subject is important, but a little dry, and I need all your kind attention.

In this variety of the primary ulcer, the form remains more regularly rounded, provided the ulcer is seated altogether upon homogeneous tissues. The borders are almost never separated from the neighboring parts. They are not always cut perpendicularly—slightly prominent, they are continuous with the base, which is hollowed out, as it were, *in the form of a little cup*. The surface of the ulceration, greyish, lardaceous, is sometimes variegated. Its centre is then of a darker tint, indented, brownish; we should say like that of a little cockade, which has caused the common name of partridge's eye *(œil de perdrix)*, to be sometimes given to this form.

But the induration which constitutes the principal character of this variety of chancre, at what period does it commence? What is the length of time which passes between the act by which the contagion is produced, and its first manifestation ?

The solution of this question is very important, for, from the moment that induration has taken place, the malady is no longer only local.

I have attempted to fix this period, but it is not always easy. The patients do not ordinarily present themselves till a long time after the contagion ; and not being aware of the importance of the pathological condition of which we are here speaking, they have not noticed the commencement.

What explains in the majority of cases this want of attention on the part of patients, is, that the indurated chancre is essentially indolent, of slow progress, suppurating little ; that they do not perceive it till very late, and often it even passes unnoticed by them. You recollect that I have already cited to you some examples. I again speak of them, in order that you may remind those who always believe in the miracle of constitutional veroles *d'emblée.*

We are not always sure of the date of the coitus, or of the contact to which we ought to refer the chancre itself; consequently it is very difficult to know when the induration commenced.

However, in the cases where it has been possible to arrive at anything precise, it is never before the third day that the induration manifests itself. In every case it is always in the course of the first or second week. It would appear even certain—at least until new observations more precise come to prove the contrary—that if a chancre exists for more than three weeks without induration, it will not become indurated. Induration is a precocious phenomenon. Certain conditions can deceive and make us believe in indurations at a later period. Let us examine these.

The specific induration is not always easy to discover. Either in consequence of the ordinary contagion, or after the artificial inoculation, the infected part often becomes the seat of an inflammatory process, which Hunter called the *common inflammation*, and in which the specific induration is enchased and masked during a certain time; so that it is only according as the simple, œdematous, subphlegmonous, or more plainly, inflammatory engorgement is absorbed, that the specific induration becomes marked, and is found as it were exhumed from the inflammatory atmosphere which surrounded it. Until then, the characteristics of the engorgement, whether œdematous or inflammatory, having prevailed, we cannot consider the specific induration as commencing until the moment when we begin to perceive it; and it is thus that we might be led to believe in tardy indurations, in chancres which have not commenced to indurate until after three weeks, a month, and even longer still after the contagion.

Certain local applications, as cauterizations for instance, give rise sometimes to factitious indurations, which we can produce at various epochs, and which might deceive.

These factitious indurations may even be complicated with specific indurations, and thus render them difficult to recognize. We know that the antagonists to the virus, formerly said that they could produce a chancre similar to the Hunterian chancre, with corrosive sublimate. Similar —yes, they are right; but identical, no. In fact, with the corrosive sublimate, the chromate of potash, the liquid acetate of lead, so often employed in common practice, the hot ashes from a pipe, and sometimes simply with the nitrate of silver, we give rise to appearances so analogous to the indurated chancre, that physicians who have not a large experience with this condition, are daily deceived. It is only in consequence of errors of this kind, that we can believe that the indurated chancre is not inevitably followed by constitutional symptoms.

There is another cause of error from which some writers upon syphilis have not escaped, and among others Mr. Babington, the annotator of Hunter. Some patients might retain from a first contagion an induration, and contract afterwards upon this induration a new chancre. If we did not know well the history of the antecedents, we might believe that this last chancre had commenced with induration, and that this might be the first phenomenon of the contagion. This is a great error; the induration comes on always consecutively to the ulceration.

The circumstances where we have not taken into consideration a previous induration, due to a former contagion, might make us suppose when the patients had contracted a new chancre upon this induration, that this new chancre had in its turn become indurated; an error which might cause us to admit more exceptions to the law of *unicité* than there really are.

You know that there are some writers upon syphilis who pretend that all the primary symptoms, whatever they may be, can be followed by secondary symptoms, and if there is any exception, it would be in favor of blennorrhagia. Well, these writers upon syphilis admit so much the more, that the non-indurated chancres, as well as the indurated ones, can be followed by constitutional symptoms. It is, then, quite important to understand how far this is true. You have already seen that commoni nflammation can mask the specific induration and cause us to believe in another form of chancre. It happens also, in some circumstances much more rare, that the ulceration, after having been indurated, becomes phagedenic. If, then, we have not seen the commencement of the disease, we might still be deceived, and believe in the possibility of constitutional symptoms after the non-indurated phagedenic chancre.

On the other hand, the induration, without losing its immense value, is not always well formed; it does not constantly assume the same development; it is sometimes superficial; we must know how to search for it in order to discover it in the thickness of the skin or of a mucous membrane. It gives sometimes to the touch only the sensation of a lining of parchment. I designate this form, at the Hospital du Midi, under the name of *parchment-like induration.* The indurated chancres, for this reason, are very often taken for simple excoriations, for simple cases of balano-posthitis, or they pass entirely unperceived; for they are superficial, on a level with the neighboring healthy parts, and sometimes even a little more prominent.

The induration ordinarily attacks all the base of the ulceration ; but in some rarer cases, it affects but the borders, and is then only annular. It is with this form of indurated chancre that we might preserve the denomination of *primary annular syphilis.*

When there exists no complication, the induration is abruptly circumscribed by the healthy tissues ; it is much more extended than the ulceration to which it serves as base. It is constituted by a hard nucleus, as if cartilaginous, resistant, elastic, indolent and perfectly rounded ; this nucleus lifts up the ulceration above the level of the neighboring healthy parts, and constitutes then a variety of the *ulcus elevatum.*

The induration presents itself sometimes under the form of a *crest* more or less prominent, when the plastic infiltration which constitutes it, does not take place in homogeneous tissues, and meets with resistance at some points, as happens at the reflection of the prepuce in the groove at the base of the gland—a seat where, after all, we find the best characterized indurations.

If we compress the skin or the mucous membrane upon an induration, these tissues grow pale, and we observe something analogous to what happens, when, in turning over the eyelid, we compress the conjunctiva upon the tarsal cartilage.

The induration is produced ordinarily in a slow and gradual manner. Sometimes it grows irregularly ; in some cases it remains a long time very slightly marked, in order to take on afterwards extensive proportions. The tissues often become indurated to a great extent; I have seen the whole of the base of the gland, which appeared to have taken on a cartilaginous transformation, and which might be taken for a cancerous degeneration. One of the most curious observations in this respect, has been offered by a patient sent to me by Prof. Andral.

The induration, after having diminished or even disappeared, is very subject to return. It is not rare to see it then take on dimensions more considerable than it had at first.

The term of the induration is not limited. In those cases which are superficial, parchment-like, I have seen complete resolution take place, so as not to leave any traces after less than a month's duration. At other times, on the contrary, it persists during some months, and even years. The groove about the base of the gland—a region, where, as I have said, it is the best marked—is also that, where it remains the longest. Upon the gland, upon the neck of the uterus, at the vulvar circle, where it is often little marked and very difficult to appreciate, the induration disappears very quickly. It is sometimes very ephemeral in the urethra, especially in females; also in the anus, and in the vagina. We must pay much attention in order not to be deceived. Upon the tongue, and especially upon the lips, it remains sometimes quite a long time. In all cases, when the induration commences to disappear, the ulceration has been already a long time cured.

When resolution takes place, the induration undergoes some modifications ; it loses its resistance, its elasticity, it becomes, as it were, gelatinous, and finishes by leaving in the place that it occupied, a wrinkled spot, of a brown copper-colored tint.

The indurated chancre, which is less often multiple than the other varieties, and of which the specific ulcerated period is soon limited, either *sua sponte* or by the effect of art, takes on, however, under certain cirumstances, quite extensive dimensions. It extends itself and deepens. We might believe that it was going to produce great loss of substance ; well, when the cicatrization is complete, we do not often find any further traces of it, for it is the plastic exudation which has alone served as food to the phagedenism, at the same time guarding the neighboring tissues from the progress of the ulceration. This condition, so common to the indurated chancre, is important to understand as regards the etiology of constitutional syphilis, for it is not the cicatrices which are the most numerous, nor the deepest, which prove that infection has taken place.

The specific induration is the certain, absolute proof that the constitutional infection has taken place. It is the passage of the primary symptom to the secondary symptom. In fact, the indurated chancre is the variety which loses the quickest the principal characteristic of the primary symptom, viz., the possibility of furnishing the inoculable pus. But if it always demonstrates the infection, and if its increase is in ratio with the gravity of the symptoms which are to follow ; if we can consider it, pardon me the expression, as a *syphilomètre*, we can also regard it as an excellent measure of treatment ; for it is one of the symptoms which ordinarily best obeys a mercurial treatment. There are, however, circumstances under which the induration resists; it is then no longer with the specific induration that we have to deal, but the most often with an organized tissue, which has succeeded it; that is to say, with a tissue of a fibrous nature. It is thus that I have been able to explain an induration which a patient presented to me in my wards in the Hospital du Midi, who was affected with a caries of the frontal bone, which came on thirty years after a chancre at the base of the gland, and in whom this induration persisted under the form of a well-pronounced nucleus. In a great number of cases the difference between the tissue of a fibrous character and the specific induration is very difficult to make out.

The specific induration has for its anatomical seat, the thickness of the skin and the mucous membranes, the subcutaneous cellular tissue as well as the sub-mucous, but it would seem that the lymphatic capillary vessels are the seat of predilection. It is in fact where the lymphatic net works are the best designated and most abundant, that the induration takes on its best form and its largest dimensions. What still comes to the support of this opinion, is the manner in which the induration extends, and is propagated; we see that it is in following the lymphatic vessels, which, according as they become more voluminous, are designated in the form of cords.

As to the intimate nature of the induration, as to its essence, as to what constitutes it, organic chemistry, which has afforded us so many marvels of late years, which has, perhaps, given us too many, has as yet discovered nothing; and the microscope, which always promises, and which sometimes keeps its promises, has thus far only recognized in the specific induration, the fibro-plastic tissue, proportionately very abundant, but which does not differ from what we find elsewhere and in other non-specific conditions. This is, at least up to the present time, the result of the researches undertaken by one of my very distinguished disciples, Mr. Acton, of England, and of those which have been since made by MM. Robin and Marchal (de Calvi) at Paris. The same results have been obtained by our learned and industrious colleague and micrographist, Dr. Lebert, to whom we are indebted for such beautiful works.

Such are, my friend, the results of my researches and of my observations upon the indurated chancre. I simply indicate them to you here, for, as I am obliged often to repeat, I do not write a didactic work upon syphilis. I recall only the principal points of my doctrine, on account of the objections which are still from time to time, and more or less directly, addressed to me. The developments form the subject of my oral teaching; they are, morever, the subject of an extended work which I am preparing, and of which these *letters*, so to speak, are only the *summary*. From this work, I select the general principles, the general points of the doctrine, indicating the principal motives upon which they are based; and this work, imperfect though it may be, has no other merit than that which the position of your readers gives it, who are no longer pupils, but learned and enlightened practitioners, and to whom these indications ought only to recall the studies and researches previously and completely made.

Yours, &c. RICORD.

TWENTY-FIRST LETTER.

MY DEAR FRIEND,—How do the chancres heal, how do they cicatrize? Let me say a few words to you upon this subject, which is an important one.

The period of healing is announced by the disappearance of the areola of the ulcer. Its borders become disgorged, sink, or they shelve towards the bottom, and the separation from the surrounding tissues, if it has taken place, ceases. The margin becomes of a pale tint, of a greyish-pearl color, and finishes by taking on again the normal color of the neighboring tissues. The bottom becomes cleansed; the grey, diphtheritic, lardaceous layer is at first as if transpierced with granulations, which at a later period everywhere fill it up, and give to the ulceration a granulated aspect, and a healthy, rosy tint. The pus then becomes less abundant, creamy—*laudable*, as we can say here with justice, for it ceases to have the power of inoculating. As the parts fill up, the epidermis spreads from the circumference to the centre, and cicatrization is completed, as takes place in every wound which has suppurated, or as after every other ulcer which has no longer any cause for continuing.

The *cicatrix of chancres* may be more prominent than the neighboring parts; sometimes it is upon a level, and more frequently depressed, according to the thickness of the tissues attacked; in a great number of cases it is indelible, while in others it disappears completely, as often happens after an indurated chancre, or when the chancre is seated upon a mucous membrane.

But, as those who have had much experience know, the period of the reparation may have its irregularities. In the *serpiginous chancre*, one extremity often becomes cicatrized, while the other continues to increase; sometimes one side heals, and the other side ulcerates again; frequently, in fine, the healing takes place in one or several points at the centre, while the circumference enlarges its unhealthy circle without ceasing.

Finally, as you well know, in certain individuals, excepting when a well-directed treatment has intervened, when the granulations have not been repressed by cauterizations, or when foolish prejudices have prevented us from doing this, these granulations become, as they say, luxuriant, vegetating, and give to the ulceration certain aspects, which have gained for it the names of *granulated*, *fungous*, *vegetating* chancres. Veritable vegetations, varied in their form, may then be produced; of an adventitious epigenic tissue, they have not, on this account, a syphilitic nature, as we shall see by-and-by.

At this period, as I have already told you, when the chancre has infected the economy, it may itself undergo a local transformation, and finish by presenting the characters of mucous tubercles, and thus justify the opinion of those who, for want of analysis, have not understood these changes, and who have admitted that these symptoms may be either primary or secondary, and that in all cases they were contagious; an opinion which I have already combated.

But here is a point of doctrine upon which I insist, and which I ought to recall to you; it is that the chancre which may increase at different periods, *never relapses when once cicatrized.* If a new inoculable chancre shows itself at a later period, after complete cicatrization, we may affirm that it is the result of a new contagion.

After all that I have just told you, it is very certain that, taking into account the morality of patients, so far as we can weigh and measure it, by knowing the conditions in which they have been placed, by recalling the seat of preference of chancres, their number most frequently limited, by knowing also how to appreciate well the different varieties of the period of progress and of the specific *statu quo*, the progress, the duration and the different aspects which they may present at the period of reparation and even after cicatrization, as also the influence more or less pronounced of the mercurial treatment, in certain cases we can arrive at an almost absolute rational diagnosis.

However, the physiognomy of the primary ulcer is ordinarily so expressive (excuse the word), at the specific period, that in seeing we recognize it. We must, however, not trust this first impression; it may cause us to commit indiscretions that will cost us something to repair. You have allowed me a pathological anecdote; I make use of your permission, happy if I can distract you a little from the dryness of my preceding descriptions.

One day, one of our very grave *savans* enters my office, and without any other preamble shows me a diseased organ, saying to me, what is that?—I answer at once, it is a chancre—Well! sir, it was my wife who gave it to me.—Then, sir, it is not a chancre.—And why not, if you please?—Because, I replied, what distinguishes simple ulcerations which resemble chancres, from the true chancres, is the source from which we believe that we have taken them. My patient was not duped by an argument, which would have sufficed for certain physicians whom you know, and contented himself with saying, with much dignity and resignation: cure me.

But is the diagnosis always as easy as is believed, and as some of our classical authors profess? I appeal to M. Lagneau, who has in our day so worthily represented them. Observe whether, in spite of all the care which he takes, he succeeds in distinguishing the primary chancre from what he calls, with so many others, the secondary chancre. Look again at the synoptical and comparative table which he has made, of ulcers which might be confounded with those which are caused by the syphilitic virus, and tell me if this enables you to establish this difference with certainty.

Mercury, that touch-stone so infallible in the eyes of the faithful, and which has been the foundation of the division of the *true* and the *false* syphilis in England, is a deceptive agent. It often cures non-syphilitic symptoms, while it aggravates those which are so, and which are sometimes cured without any treatment.

How many chancres there are which are overlooked by experienced practitioners! How many errors committed especially in regard to the different varieties of the indurated chancre, the most dangerous of all! Sometimes we believe them simple excoriations, sometimes we can be deceived to such a degree as to consider them as true cancerous degeneracies. My colleague and friend, Dr. Vitry, of Versailles, must recollect a patient to whom a physician of Paris called me, not to judge of the nature of his complaint, but to amputate his penis. I recognized the existence of an indurated chancre, attended with considerable increase of the plastic exudation, and the pills of the iodide of mercury replaced the knife.

One of our learned professors of the Faculty of Paris, who is as cognizant with syphilis as with other diseases, in the diagnosis of which he excels, must remember the history of a great Russian lord whom we saw together at the house of our honored and regretted master, M. Marjolin, and in whose case he would not recognize a primary symptom, because there remained nothing but the specific induration, and because this lord could not account for nor explain to us, how he could have contracted this affection, which shortly afterwards, as I had predicted, gave the most convincing proofs of being constitutional.

If you will let me, I will relate to you another little story. The nephew of Cullerier one day sent me a well-known writer, to ask my advice respecting an ulceration which he had upon the corona glandis; an ulceration with an indurated base, and which did not then present the characteristics of the borders and of the base which are classically required in order to constitute a chancre. I did not the less recognize an ulceration with the specific characteristics of the induration which I have lately described, and with the glandular radiation which we shall have to study presently. Cullerier was not of my opinion, inasmuch as he had examined the only two women accused, and whom he had found healthy. The nephew did not admit the mediate contagion, nor spontaneous syphilis, and as he had faith in what the patient said, he could not admit the existence of a primary ulcer. I, who often doubt, even with the most certain proof, and who admit all the rational ways of contagion, remained convinced that the patient had been deceived, that he was mistaken, or that he deceived us. In fact, six weeks had scarcely passed before constitutional symptoms well characterized — too well, for they were very difficult to cure — manifested themselves. But while Cullerier was still asking himself how and why the patient had syphilis, I was called to the house of a distinguished lady.

I arrived, knowing neither the end nor the motive for my visit. This great lady was mysteriously seated in her boudoir, and, in spite of the twilight that reigned in the place, I could perceive upon her face quite satisfactory evidences of a secondary affection. Doctor, says she to me, what I have to say to you is of a very delicate nature. Wishing to cut short a painful confession, I said to her, I see what it is, madam, and your face explains to me sufficiently why I have the honor of being in your presence. What do you mean ? she asked with astonishment.—That you are sick, madam, and that doubtless you require my services.—Not in the least. I have requested your visit in order that you should aid us in preserving M. X—— (the writer who had been sent to me by Cullerier), not only from his malady, but also from his dangerous intrigues. And here was this lady, who took upon herself to draw me a portrait, not very flattering, of the two women whom Cullerier had examined, whom he had found healthy, and who were, according to her account, the cause of all the evil. I had great trouble, as you may suppose, to make this lady understand that the source from whence our poor writer had taken his disease, was situated much nearer to me, and to obtain from her the confession, that the pressing interest which she had for our patient had other motives than a pure Platonic affection.

Thus it is with all of them; and the moral of this anecdote is, that the men of the world never make you full confessions; that in having relations with great ladies, or with others in whom they have confidence, their ideas are a thousand leagues from the truth; their suspicions do not rest upon the veritable source of their malady, and they search for it where it does not exist.

You see, then, how difficult the diagnosis of chancre often is; and how wrong we are to deny its existence, when the patients do not aid us in discovering the source from whence they have taken it.

It is, then, because I know all the difficulties of the diagnosis in quite a large number of cases; it is because I have seen the most skilful men commit frequent errors, that I have said, and do still say, in spite of the contrary opinions, that the only positive, unequivocal pathognomonic characteristic of chancre at the period of progress or of specific *statu quo, is found in the pus which it secretes,* and which can be inoculated; whence I conclude that,

Inoculation gives the most certain evidence of the specificity of the ulcer.

I said in the work that I published in 1838, that if we ought to give mercury in all the cases where a primary virulent symptom exists, we should always be certain of this virulence by practising artificial inoculation in time. But be assured, this operation, to which some persons might object, and which they have the right to consider as dangerous, when one does not know how to make use of it, is not necessary in practice, and I have never advised it as a general rule.

The prognosis and treatment of chancre are based upon other indications, than its virulence; for it is the induration and its accessories, which inoculation is unfitted to make us distinguish, which foretells the future fate of the constitution, and requires the specific treatment.

This is what I hope to be able to demonstrate.

Yours, &c. RICORD.

TWENTY-SECOND LETTER.

MY DEAR FRIEND,—I was very desirous of saying a word to you upon the treatment of chancre, but according to the plan which we have adopted, I cannot in this connection enter into any great detail.

Perhaps you will first permit me to say to you something upon the prophylaxis, and upon the medical police, which has become better established within a few years, and especially since I instituted the examination with the speculum in the special hospitals, and in the dispensary for the public health, and which has been adopted after my example.

It is very certain that since this mode of investigation has been generally employed, we can observe a great improvement in the health of the public women. Thus, according to Parent-Duchâtelet, in 1800, one diseased woman was met with in nine; now we do not encounter, since 1834, but one in sixty. Consequently, the speculum has played its great part in this amelioration.

But if we wish to do the business satisfactorily, we must, as I have always professed, visit the women every three days, without distinction of rank, whether they are in a house or "*en carte*," whether they inhabit Paris or the Barrieres. You remember that from the second day of an artificial inoculation, we may already have inoculable pus. Swediaur admitted that the chancre could be developed in twelve hours; it is necessary then that the visits should be frequent, and the examination always made with the speculum, in order that the inspection of public women should offer a certain guarantee.

I write designedly this word *guarantee*, for there are some who, after their adventurous amours, think that they have a right to reclaim indemnity from the administration. Perhaps you think that I am not serious; here is a fact which goes to prove to you my assertion: — A few years ago a merchant of Lyons came to me in a state of very great exasperation against the prefect of the police. He came to get a certificate, stating that he had contracted a chancre in a public house that he believed *guaranteed* by the authorities. His intention was to follow it up with damages and interest. He did not know that the *tolerance* is a sort of brevet, which, like all brevets, is without guarantee from the government.

I hasten to say that the ameliorations which are introduced every day in the inspection of prostitution, and the zeal of our colleagues charged with the painful business of the dispensary of health, and of the hospital of Saint Lazare, will give better and better results.

That public women are a necessary evil, is generally agreed upon at the present day. I wish neither to combat nor to support this sad proposition; it is not the place to examine this here; but if the evil is necessary, it should not be extended, as regards number, as a learned colleague of Belgium recently appeared to desire, but special attention should be paid to its quality.

In requiring that public women should not communicate disease, it ought to be so arranged that those who frequent them, should not expose them to it. How shall this be done? Must we institute an examination of the individuals who frequent them, and prevent them from having intercourse if they are diseased? But in addition to all the difficulties of such an institution, the danger which we might wish to prevent by this institution, would be rendered greater, for the filth, in place of falling into a sink which the police can clean out, would go elsewhere.

We cannot certainly think, at the present day, of establishing lazarettos, quarantines, of demanding together with a certificate of vaccination, a clean bill of health from syphilis, as my friend Diday, of Lyons, wrote in a moment of praiseworthy philanthropy, a bill which should be *exigible*, and as indispensable as the passport, a bill without which one could not be admitted to any public function. Whatever the ingenious author of this proposition may have said, the difficulties of its execution appear insurmountable.

There was a time, as you know, when the infected, banished from Paris, were condemned to the cord if they reëntered the city; an epoch when in the insane asylum of Bicêtre they whipped the patients at their entrance and at their exit. All this did not diminish their number; on the contrary, the whippers merited in their turn to be whipped; these barbarous measures have fallen into disuse.

It is doubtless necessary to submit to a rigorous inspection all those that we can reach, soldiers for example, to sequester all the patients over whom we can have any authority; but a certain tolerance, the pardon of a fault quite often involuntary, and good hospitals with the succor which can be found in them at the present day, and which we can still ameliorate, herein consist the best means for a general prophylaxis, or those at least which shall tend to render the disease less and less grave.

Besides, all those who are acquainted with the sad conditions of the work and remuneration which is made to women in our present condition of society, have for a long time understood and proclaimed that herein was one of the most abundant sources of prostitution, and consequently of the propagation of syphilis. To ameliorate the condition of woman's labor, is then to perform at the same time a work of humanity, of morality, and of public health.

You remember what I said to you of the manner in which chancres are produced. It is necessary to remember it in order to avoid them. What science possesses most certain as regards the prophylactic treatment, is not to expose one's self to chancres. This appears a little *naïf*, but let the debauched remember that it is the truth. I am going to touch here upon a delicate subject, and one filled with dangers. It is still a question of morality and of medical deontology not yet solved, to know whether the physician can and ought to give advice to preserve from an evil those who expose themselves to take it from a degraded source. I do not pretend to be more rigorous than the austere Parent-Duchâtelet, who commenced this subject with the purity of intention which you recognize in him. On the other hand, am I not re-assured by the very nature of the Journal which gives such liberal hospitality to my letters? I address myself to the learned, to physicians; and was it not you, my friend, who said, that science is chaste, even in a state of nudity? After all, be re-assured, I shall not do more than touch upon this ticklish subject.

There does not exist any sure and absolute preservative from the chancre; this is my declaration.

If, in spite of this, one wishes to run the chance of it, some precautions can be taken. One must first bear in mind the precept of Nicolas Massa, so forcibly translated by the elder Cullerier—the relations ought not to be voluntarily prolonged; at this time one must be egotistical, as the grave Hunter remarked, but not egotistical after the manner of Madame de Staël, who called love the egotism of two.

Attention to the most minute cleanliness on the part of suspicious persons, ought to be exacted in public houses. What we have known for a long time past respecting the deposit of the virulent pus which may be retained in the genital organs of women, shows the necessity of this. It is a means of always preventing mediate contagion. I have told you that numerous experiments have shown that it sufficed to decompose the virulent pus in order to neutralize it. Alcohol in water, water mixed with a fifth part of Labarraque's disinfecting fluid, all the acids diluted with water so as not to be caustic, wine, the solution of zinc and the acetate of lead, suffice to prevent the virulent pus from being inoculable; while, if this same pus is not altered, it suffices that the quantity should be excessively small, homœopathic, if you please, in order to act. M. Puche has told us, at the Hospital du Midi, that he had obtained results from the inoculation of a drop of pus mixed with half a glass of water.

The use of fatty substances is very useful, especially for medical men who practise touching upon dangerous parts. Astringent lotions which tan the tissues a little, have often served to ward off the contagion.

But if the precautions of neatness are necessary before connection in the person who might infect, they ought to be minute after the act, in the individual who exposes himself.

There is a method which morality repudiates, and in which debauchees put much confidence, which doubtless often guarantees, but which, as a woman of much *esprit* has remarked, is a cuirass against pleasure, and a cobweb against danger. This mediate agent is often rotten, or has already been made use of; it is frequently displaced; it performs the office of a bad umbrella which the storm may tear, and which under all circumstances, while it guarantees badly against the storm, does not prevent the feet from getting wet. In fact, I have seen ulcerations quite often upon the root of the penis, upon the peno-scrotal angle, upon the scrotum, &c., in persons who had taken these useless precautions.

Many patients believe themselves safe from contagion if they do not terminate the venereal act. A lady who consulted me about herself, was much astonished in having communicated disease to her lover, inasmuch as *he did not finish*, she said.

Some medical writers upon syphilis believe that the urethral infection in particular, is produced after the emission, which causes a vacuum, and from the horror which nature has of a vacuum. But numerous facts have taught me the contrary. The emission in fact ought to be considered as a powerful injection from behind forwards, and which thus cleanses the urethra; and if the urethral affections already so common are not more frequent, it is perhaps to this condition that it is to be attributed. Thus an old and excellent precept is that which recommends a speedy micturition after every suspicious connection. At one time, fortunately remote from us, they made use of jugglers.

The circumcision of the prepuce, the excision of the nymphæ which are too long, ought also to constitute an hygienic law as regards the genital organs, for these appendages greatly favor contagion.

I ask your pardon for this digression—but science must attempt to deprive charlatanism of the dangerous cultivation of a deceitful prophylaxis. We ought to be always able to point out everything which can favor the avoidance of contagion, and prevent the propagation of syphilis — not in order to protect or to favor libertinism, but to thereby guarantee virtue and chastity, which become too often the victims of it.

There remains for me now to speak to you upon cauterization as an abortive means, and as curative of chancre. But in order not to divide the subject, I shall make it the topic of my next letter. Yours, &c. RICORD.

TWENTY-THIRD LETTER.

MY DEAR FRIEND,—I promised to speak to you today upon the cauterization of the chancre.

This practice, which I have attempted to support in therapeutics, has not yet, however, been generally adopted ; it has even been the subject of a very severe reproach on the part of some practitioners; and I add with humility, that it was very near undergoing a severe ordeal on the part of the Academy of Medicine, before I had the honor of being a member of that society.

Do you remember, in fact, that one of the members of that learned society treated cauterization very severely, and that he sent this method with proud disdain to the *lock-up*, from which he remarked it ought never to come out. The author of this apostrophe, in his quality of an old military surgeon, ought at least to tell us whether this method cured or not in the "*corps de garde;*" for the important point is to be certain of its efficacy; and if the method is good, its place of origin is very unimportant. This is said without bad intentions.

I am not the inventor of the cauterization of the chancre, but I am an adept in the doctrine which extols it, and with this, you know that they have not failed to attack me. I have here, then, to defend the principles which I profess.

Let us first invoke analogy.

We cauterize the bite of the snake, the bite of a mad dog, dissecting wounds, the malignant carbuncle, the malignant pustule, &c., and we obtain numerous successful cases when we arrive in season. Nobody would suffer a wound made by an instrument soiled with the matter of glanders or of farcy, to remain. The surgeon who did not cauterize in these cases would be very culpable and much blamed. And yet these same men who are armed with iron and fire for all these cases, come to a stand-still when it is a question of chancre! Why? It is because they cease reasoning, or because that their reasoning is bad.

Let us prove it.

Does the chancre, whatever may be its variety, always produce secondary symptoms? Does it always infect the constitution?

There exists, as you know, three very distinct opinions upon this subject.

Those who believe only in incredible things, and the number of those is yet considerable, are convinced that the chancre is not even a primary symptom in the extreme sense of the word; but that it constitutes simply a primary manifestation of the general infection, or, as I have already remarked to you, a primary secondary symptom, or a secondary primary one!!!

Others, who already commence to penetrate the truth, —and we must range Hunter's school in this category—admit evidently that the chancre is at first a local affection; but they think that it ought inevitably to affect the economy, if a specific medication is not at once made to intervene.

Finally, the most reasonable, those who have on their side, observation, experiments, and the evidence of facts, affirm that the chancre is, at the commencement, always a local affection, which art can arrest, and which, even without the intervention of art, may remain local under certain well-determined circumstances, whatever may be its duration, or its extent in surface and in depth. These last observers maintain, and therein is one of the consoling points of the doctrine which I profess, that even when the chancre is to infect the economy, this result is not immediate and instantaneous, but that this unfortunate condition never arrives until after an interval which gives us time to destroy it.

I shall not speak to you of the physiologists whom I formerly had to combat, and who admitted infection neither before, during, nor after—this doctrine is duly and thoroughly buried. And what is more singular, some of these adepts have since been more virulent than myself. I could cite to you those, who, though incredulous as regards virulent creeds, have finished by believing in all, even in homœopathy.

I do not wish here to enter into the discussion of how and when buboes are produced, of the time when the constitutional infection and its mechanism takes place; we shall return to this by-and-by. I wish only to recall to you the reasons which have caused the cauterization of the chancre as an abortive or curative measure to be rejected, and those which have made me adopt it.

What object have we in cauterizing chancres?

1.—To prevent the constitutional infection.

2.—To prevent the production of buboes.

3.—To resist the progress of the primary symptom, the consequences of which are greater or less deformity, and sometimes the loss of precious organs.

4.—Finally, to destroy a *foyer* of contagion.

Those who believe that the constitutional infection always precedes the chancre, say that it is not only useless to cauterize it, inasmuch as the evil which we wish to prevent already exists, but they even add that it would be dangerous to do it; for the chancre is *an emunctory* through which the economy is disembarrassed of the virus.

If this opinion had foundation, it would follow that it would not only be improvident to destroy the chancre, but that, on the contrary, we should preserve it, extend and multiply it, in order to open to the virus the most numerous and easy means of exit. This would be logic indeed! But you know, my friend, that it is not thus that these logicians act, and, let us acknowledge it, it is very fortunate for their patients that they do not act up to their principles.

The difference, nevertheless, is not great between this school and that one, which, as I have told you, is of the opinion that the chancre, although local, inevitably produces the general infection. Its maintainers profess that the chancre is the source of the infection, the activity of which is in ratio with the number, extent and duration of the primary symptoms.

But, alas, with these fine principles, follow immediately a contradictory application, and a nonsensical practice. What in fact, do they prescribe? Listen to them, and they will tell you.

Be careful of destroying the chancre; do not seek a rapid cure; you will drive back the virus, and cause it to strike into the economy; you will shut the wolf into the sheep-fold, and finally render the infection more active.

Do you not admire how all this is deduced and is linked together! We drive back, we cause the virus to strike inwards, in drying up the virulent virus! The wolf shut up in the sheep-fold is more dangerous, because he is dead!

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The infection becomes more active, when we have destroyed the elements which ought to increase it!

My intelligence cannot elevate itself to the sublime heights of this reasoning; are you more fortunate than myself, my friend?

This is not all; the partisans of this doctrine again tell you. respect the chancre, it teaches you what the patient actually presents, and what he will present at a later period. They add, do not cure the primary ulcer too soon, it serves in guiding you in the general treatment, and obliges the patient to follow it. What do you think of these precepts? What satisfaction is there to know each day, beyond a doubt, that your patient certainly carries a chancre, and to be afterwards assured, that it is this which has determined the other symptoms which you will have to combat by-and-by.

The primary symptom enables you, they say, to direct the depurative treatment; but you know, as well as I, that there is not one of those who profess these doctrines, who suspends the general treatment until the chancre has been cured. even by their own method. Their treatment is about the same in all cases; it is a fixed dose of mercury administered during a determined time, whatever may be the nature of the primary symptom, whatever may have been its duration. And then what say you of this precaution of letting a chancre progress, so as to amputate the penis, in order to insure the patient's following his treatment; it is truly admirable, and we could not be more prudent!

Cauterization has been reproached for being a frequent cause of bubo ; and for the support of this assertion, the meagre statistics of Bell have been often cited, which a single visit to the Venereal Hospital of Paris suffices to reduce to nought.

The law is this, and you shall verify it when you please: there are more buboes without previous cauterization of chancres, than otherwise.—Cauterization does not always prevent buboes from being produced ; it never produces specific ones ; it may often prevent them. It may prevent the constitutional infection ; it never favors it.

I well know that many observations have been cited for the support of the heresy which I combat ; but they are not all of the force of the observation which we find somewhere in Van-Swieten. In this observation, there is question of a patient affected with a chancre, *for more than a month, and who in consequence of a cauterization, had been affected with secondary ulcerations of the throat, in consequence of the pretended repercussion!* Oh, Syphilis, when will you be understood ?

M. Lagneau, who pronounces himself against cauterization, because that among other inconveniences, it gets rid of the primary symptom too quickly, cites, in order to blame this process, an example in which it had an admirable result. But that we may the better judge, suffer M. Lagneau to speak. Here is his observation.

" In 1807, a superior officer temporarily called to the imperial head quarters at Warsaw, exposed himself to venereal contagion. Shortly afterwards, two chancres made their appearance at the base of the gland. He was proceeding in their treatment, when unexpectedly the army marched. This patient could not think of dispensing with following his regiment, at a time when everything announced some great events, in which he wished to take part. Being attached to a corps of cavalry of the vanguard, his duty was much more difficult, inasmuch as the cold was extreme; added to this, the diet which one always follows in similar circumstances, and many other very pressing causes, allowed him to count but little upon remedies irregularly administered, in order to arrest symptoms which could not fail of developing themselves rapidly under the influence of so many causes capable of producing them. *I yielded, then, to the reiterated demands of this officer, and touched his ulcer with the nitrate of silver*, forewarning him, however, what he had to fear for the future. *The chancre cicatrized very promptly, and the patient made the campaign without feeling the least inconvenience.* Shortly after the battle of Eylau, the army having gone into quarters upon the Passarge, he told me of his condition, and I advised him to prevent by methodical treatment the consequences of a general infection. He followed this advice, and has never felt the slightest venereal symptom since that time."

After an observation so conclusive in favor of cauterization, you will not expect, I hope, that I should give you millions of facts, which I have collected in twenty years of practice. The above appears to me sufficient.

But now, in order to tell you how I understand cauterization, you will permit me, in the next letter, to recall to your mind some important propositions.

Yours, &c. RICORD.

TWENTY-FOURTH LETTER.

My Dear Friend,—In terminating my last letter, I asked of you the permission to recall to your mind some important propositions, before making known to you how I understand and how I practise the cauterization of the chancre. Here, then, are these propositions!

The chancre, as I have endeavored to demonstrate to you, is at the commencement, an absolutely local affection, and which may remain definitely local.

The chancre may heal spontaneously or by a local treatment.

It is not until after a certain duration that the chancres take on such or such a form more or less grave, and that they can produce symptoms in the neighborhood or at a distance.

If we destroy chancres early, if we apply to them an abortive treatment in the first moment of their existence, from the first to the fourth or fifth day of their appearance, we are almost certainly safe from those symptoms.—In all cases, if we arrive too late, and when we cannot count upon the abortive treatment, cauterization may still shorten the duration of the primary ulcer.

These principles laid down,—and I have yet to wait for a truly serious objection to them, either experimental or clinical—we immediately understand all the value of cauterization as an abortive method; it is so important, it is so efficacious, it gives such good results, that I would wish, like M. Ratier, that it was a precept advertised everywhere, where persons are exposed, never to let an erosion or a suspicious discharge remain an instant, without having it destroyed by this means.

But in order to conclude upon the good effects of cauterization as an abortive and preventive treatment of every future symptom, several conditions are necessary :—

First, we must not reckon the *age* of the chancre from the moment when the patient perceived its existence, but from the contagious contact which must have produced it. In taking this precaution, we shall see, as I have said, that a chancre destroyed before the fifth day of its existence is decidedly dead, and will produce no more consecutive symptoms.

In order to reckon upon the abortive cauterization, we must not be content with having touched an ulceration with any kind of caustic ; when the scab falls, we should find in the place of the virulent ulcer, a simple wound, otherwise the caustic will have been of no avail. It is in consequence of imperfect cauterizations, or of those practised at too late a period, that we may see supervene symptoms which we have no right to impute to them, in fact. If buboes already exist, if the chancre is indurated, if, consequently, the diathesis is established, and from this same cause secondary symptoms already exist, it can only serve to modify the primary affection, to hasten the period of reparation, to repress the granulations, to promote the cicatrix, and finally to shorten the duration of the ulcer.

It is in the artificial inoculation that we can study well the cauterization as an abortive method, as a neutralizing means.

And here it is important that I tell you, that as soon as a puncture has been made with an instrument charged with virulent matter, or as soon as by any other method the morbid poison has penetrated into the tissues, not only do simple lotions no longer suffice to prevent the contagion, but we cannot even arrest its effects in applying upon the contaminated part the different agents susceptible of neutralizing the virus, as I lately told you, when we mix them with it before inoculation. These mixtures may assuredly destroy the *syphilitic germ* in the state of seed, and out of the soil where it ought to be sown; but immediately that it is sown, they are powerless in preventing it from germinating: cauterization or excision, made in time, enjoy alone this privilege.

I have made upon this subject numerous experiments: I have placed upon the *punctures of artificial inoculation*, at the moment when I made them, either *mercurial plasters*, as has been advised for the abortive treatment of variola, or some pledgets of charpie besmeared with the double mercurial ointment, and the inoculation has progressed notwithstanding.

I have been able to prevent the chancre from developing itself, only by destroying the part contaminated.

It must be borne in mind, when the pustule is already formed, or if the ulcer exists, that the virulence is not entirely in the pus secreted, that it is not even limited to the diptheritic layer which covers the chancre; for if we cleanse the ulcer, if we take away the pus which it furnishes, if we destroy its false pyogenic membrane, it is reproduced with its specific power. There is, then, to a certain extent, *a sphere of virulent activity*, the radii of which are proportionate to the extent of the ulceration and of its duration. Consequently it is necessary, and this is very important in practice, that the caustic extend beyond the field of the specific inflammation, if we wish that it should be efficacious.

I have told you that every chancre, no matter what may be its extent, is limited by tissues which are not in the condition *of virulence*, and in which we may make a simple wound, the cicatrization of which we may afterwards easily obtain.

This limit which the caustic ought to attain, is not easy to determine. What I can say is, that I have always succeeded when I have practised the cauterization in an extent double that of the ulceration, and in traversing all the thickness of the tissues. We may conceive that the extent of certain ulcerations, their particular seat, do not always permit us to put this precept into practice, so that we very often fail. This is, therefore, what happens almost always when we make use of nitrate of silver. This caustic, the action of which is very superficial, is not applicable excepting to the most recent and the slightest affections.

Vienna paste is the caustic which has succeeded the best.

I have never failed when I have wished to destroy a pustule of inoculation up to the fifth or sixth day. A single application suffices, in these cases, and almost always a dry eschar is formed which is detached little by little by a cicatrix which is formed beneath. If the eschar falls off too quick, or if it is thrown off by suppuration, it is a simple wound that is left open.

Arsenical paste has also given me very good results, but employed in a positive manner, that is to say, allopathically; for you know that this therapeutical agent has failed to have an homœopathic success in the hands of a learned colleague.

The actual cautery is also an excellent method—the best, perhaps, if it was not so frightful to many patients, and if we were justified in abusing the use of chloroform every time that we had a cauterization of this kind to practise.

I am experimenting at this moment, from the good results given out in Belgium and in England, with the solidified nitric acid, not only in the treatment of phagedenic chancres, but also in the more simple kinds, and as an abortive method. According to what I have seen, in a great number of cases where I have perfectly succeeded, it would seem that we can neutralize the ulcerations without the necessity of destroying as much of the tissues as with the other caustics. We must say, however, that its action is very painful, that the pain endures a longer time than with the Vienna paste, and that we are ordinarily obliged to make several applications at two or three days of interval, if the ulcer is already a little extended.

As to the rest, whatever may be the caustic employed, we must repeat the applications as often as at the fall of the eschar, we find the lardaceous base of the period of progress. We ought later to have recourse to a less powerful caustic only in order to promote the cicatrization.

Hunter, who as you know is a partisan of the cauterization of the chancre, has also advised the excision of it. When we can excise nymphæ which are too prominent, which serve as a seat for primary ulcers; when we can take off a prepuce which is too long, the border of which is contaminated; and when we can cut sufficiently far from the diseased parts, we succeed, and this operation ought also to be preferred in all cases, because at the same time that we cure the disease, we cause a deformity to disappear. But if the seat of the chancre does not permit us to cut at a sufficient distance, as is most commonly the case, we must renounce this proceeding.

Like cauterization, excision is useless against the indurated chancre.—The earliest excisions of *the specific induration*, have never prevented the symptoms of the constitutional infection from manifesting themselves.

In all cases, whatever method is employed for destroying more or less rapidly a chancre, whether excision or cauterization, we should never neglect to fulfil all the other indications which might present themselves.

But let me terminate this letter, or, if you like it better, this *postscript* to my last letter, in repeating to you that the cauterization of the chancre is an admirable method, and that it is, as regards society, the most powerful prophylaxis, inasmuch as in destroying the contagious symptoms the most surely and the most promptly, it extinguishes the foyers of contagion.

All that I have just told you, results from the observation of several thousand facts, and from experiments as rigid as long sustained.

Permit me still to add, as regards the prophylaxis of chancre, that it would be a very great error to believe that, according as chancres develope themselves, or as successive contagions are effected, that the new symptoms which arise are less active than those which preceded them, and that chancres go on losing their severity in ratio with their number, and that they finish by no longer having the power to reproduce themselves.

We observe very often the contrary; the last chancres contracted may be *more active* than the *first*, they may even take the *phagedenic form*, which happens perhaps oftener when there is a *syphilitic diathesis*, or *syphilisation* (as those say who do not like to make use of the customary language). This is even so true, that I have considered the *syphilitic diathesis as a cause of the phagedenic condition.* The proof of all this I engage to furnish to you when you wish, at the Venereal Hospital. I shall return later, to all these points of doctrine; in the mean time, the laws which we strive to deduce from experiments made upon animals, prove that the inoculation of the syphilitic virus gives results not *identical*, but essentially *different*, according as it is practised upon man or upon animals. These laws, if they are truly laws, do not in any way invalidate, even up to this time, all that I have said to you upon this subject. Then let us await something better.

You perhaps remember that Frike, of Hamburg, who has also made some experiments upon inoculation, believed that he had observed that successive inoculations lost more and more of their intensity, and that their effect became *null* at the *sixth* inoculation, when we practised them upon the same individual. I have pursued the inoculation of the chancre even as far as the eighth generation, and I have never established the least difference between them. Frike, to whom I showed these results, verified them like myself, and had to confess that he was deceived.

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

Yours, &c. RICORD.

TWENTY-FIFTH LETTER.

MY DEAR FRIEND,—In the first place, excuses and regrets for my too long silence—I dare not recall the date of my last letter. It is better to confess one's faults than give a poor excuse. I acknowledge, then, that it is a very long time since I promised to speak to you upon buboes. At least, admire how logical I am; for, as you know, I do not admit the bubo *d'emblée*.

The buboes, as old as man, were not unacceptable to Astruc, unless the first man was deprived of the lymphatic glands; the buboes well known to the Jews, who, according to Apion, were already subject to them in their journey into Judea, and of which the good king David also appears to me to have had many to complain of, constitute an important condition to well understand, and a very interesting one to study.

You comprehend, my friend, that it would no longer be the epistolary form which I should assume, but rather the magisterial and didactic, if I were to tell you all; but no, I confine myself to the limits that I have imposed upon myself and which you have accepted.

It is so long a time since I have proposed what I am going to expound to you, that it has for the most part become public property; and yet there are still some behind hand, and there are some who have not yet forgotten what they learned at the School in 1828, in the last edition of M. Lagneau.

However it may be, can the bubo, viewed as a venereal symptom, be developed without the precedence of any other symptom? Can it supervene, as they say, as a primary symptom *(d'emblée)*? This opinion, which dates from the period of mystical rites, upon what is it based? What proves the truth of it? Analyze what has been said in all times, consider well the observations published in its support, and you will see every where false analogies, errors of diagnosis, ignorance of the laws of evolution, and of their possible consequences from want of due appreciation.

For the causes, a contact, a connection is sufficient, provided *that it be a suspicious one*, no matter whether a longer or shorter time elapsed before the appearance of the bubo. There is always the same facility, the same elasticity, for that period called the period of incubation. It is always to the preceding sexual connections with the individual who inspires the least confidence, that we have recourse, in order to explain an enlargement of the glands, the cause of which we know not how to discover, and generally without knowing how the individual who is accused, was affected. With this manner of reasoning, there is no enlargement of the glands which might not be considered as of a venereal nature. But if simple contacts, the deposition of the virulent pus upon surfaces not denuded, are sufficient to give rise to buboes without producing previously other conditions, the buboes *d'emblée*, the least frequent of all, according even to those who admit them, would be the most frequent; for the circumstances under which contagious parts come in contact without excoriation, are by far the most numerous.

In the great number of patients whom we have under observation in the large hospitals, as at the Venereal Hospital of Paris, and in whom often exist numerous chancres, furnishing an abundance of pus at the specific period, and which soils the neighboring parts, do we ever see buboes supervene beyond the course of the lymphatics which terminate directly in these ulcerations? In observations of this kind, we should be cautious about being led away by the illusions of M. Schals of Strasbourg, and by the *naïveté* of those who have cited him.

To those who have rejected the idea of the bubo *d'emblée* as I have done, and many before me, it is asked — but why will you not grant that the venereal virus traverses the skin and the mucous membranes in order to pass to the glands, without inflaming the former, since we see many other bodies, other matters, absorbed without the necessity of a previous lesion?

First, it would be useless for me not to grant this. If it was the case, it would be necessary to accept it; but it is not.

From the fact that we can cause mercury to penetrate into the economy by simple frictions, without solution of continuity, can we conclude that we can make caustic potash penetrate equally as well? Does the poisonous matter of a dead body ever act without an excoriation, the saliva of a mad dog without a bite, the venom of a viper without the puncture? Would our excellent colleague and learned vaccinator, M. Bousquet, count much upon the application of vaccine virus without the production of vaccinal pustules? Are vaccinal enlargements of the glands ever seen without vaccinal pustules? From the time when we inoculated the variola, and at the present day, are there variolic glandular enlargements without the variola? Certainly not. Do not invoke, then, false analogies. If certain causes have a process of action, it is not meant that all act in the same manner; it is this which distinguishes them, and in this respect syphilis has its *specificité*; it does not penetrate without solution of continuity, and the surface which it first injures, preserves its marks for a longer or shorter time, before it goes any farther.

Those authors who admit the bubo *d'emblée*, all tell you that they have met with patients affected with an engorgement of the inguinal glands, who have had neither blennorrhagia nor chancres; they have all observed cases of this kind. Bell has seen perhaps twenty of them, when he should have seen some hundreds of them if their existence had been real. M. Lagneau, in imitation of those who have preceded him, gives some similar observations, and adds that we can always find examples of these cases at the Venereal Hospital.

Yes, it is because there are always at the Venereal Hospital, a considerable number of these pretended buboes *d'emblée*, that I can understand how they have been so long time deceived.

I shall here make quite a curious observation : it is, that in the history of buboes *d'emblée*, their partisans have never cited examples of them in other regions than in those of the *inguino-crural*, with the exception of the observation of M. Schals, where an enlargement of the axillary glands, the consequence of a paronychia, was taken for a bubo produced by the absorption of blennorrhagic vapors through a recent cicatrix upon the finger; they do not say, that buboes *d'emblée* have been observed below the jaws, where, however, so many doubtful kisses tend.

In order to admit that a glandular enlargement is of a venereal nature; in order to have the right to consider it as being the consequence of a contact more or less recent, the result of the passage *of the virulent pus in substance* through cutaneous or mucous surfaces remaining healthy; in order to admit that this engorgement is the first syphilitic manifestation, that it is in fact a primary bubo, and that it is not a secondary one, for the authors of this doctrine admit secondary buboes — we must give some differential signs between these two kinds. Now, you know how they are distinguished; if the patient has already had some trouble anteriorly, the bubo is reported constitutional; when there has not been other antecedents, they attribute it to the last connection, and it is then ranged in the category of primary symptoms; for as respects the seat, the form, the symptoms, the progress and the terminations, they do not offer anything absolutely different.

But do the lymphatic glands obey only venereal causes in general, and the syphilitic virus in particular? Certainly not. I need not here speak to you of everything which can affect them; it is a subject too well understood; but what I need to recall to your mind is, that when syphilis is entirely foreign to them, and we do not always find the cause which has acted upon them, as it happens in many other diseases the causes of which escape us — we say then that the enlargements are essential, idiopathic. But may not these same enlargements present themselves with their hidden cause, and with their same nature in individuals who have undergone suspicious contacts? Incontestably, yes. Well, have they succeeded in establishing a difference by any signs that you know? Certainly not. They have not given a single incontestable pathognomonic sign. Most frequently it is the particular seat, considered as specific, which has decided the question. They have done for the inguino-crural regions, what M. Charles Dupin has done for the departments of France, as regards instruction; what Parent-Duchâtelet has done for the quarters of Paris, as regards prostitution.

Such a glandular enlargement which is in that region considered as a venereal bubo, would be considered innocent in the arm-pit, and especially upon the sides of the neck; as if all the lymphatic glands were not equal in the human constitution; as if the same causes could not attack them every where, with only the difference of frequency.

Not only they do not distinguish by the ordinary way these simple glandular enlargements, arising from known or appreciated causes, from the buboes called venereal, but they have not even succeeded in establishing a marked difference between the strumous and venereal buboes. What think you, in fact, of those characteristics, which consist in "the knowledge of the temperament of the patient, the particular aspect of the strumous buboes which are commonly *soft*, *œdematous*, and of a violet red!" Add to this, the specific *elasticity* of scrofula, of my learned colleague and friend M. Boyer, who has the good sense not to admit the bubo *d'emblée*, and you will understand that with such means of establishing differences, it is not astonishing that they have confounded every thing, and that they have established as fact the primary bubo! But those who admit this, constitute all that is truly primary here.

We shall see, by and by, what venereal buboes are, taken together, and what are syphilitic buboes, taken by themselves. At present, let us content ourselves with closing this letter, by saying that neither by experiments, nor by incontestable observations, have the existence or even the possibility of primary buboes been demonstrated; that this way of stating facts in pathology has also passed away; that in consequence they have for us fallen from the nosological tablet, and that in order to proclaim their fall, it is sufficient for me to cite here the condemnation uttered against them, in a moment of abandon, by one of their most glorious supporters, Hunter, who says, in speaking of the bubo *d'emblée*, "If the parts were explored with much more care, if the patients were minutely interrogated, it is probable that we should often discover that a little chancre is a cause of the infection; this is what I have seen more than once. In fact, when one considers how rare the absorption is in gonorrhœa, where the mode of the absorption is the same, one can scarcely admit that the infection could be the result of the simple contact of venereal pus, when the application of this pus has so short a duration. We might suppose, it is true, that the repetition of the contact takes the place of its duration; but we cannot admit such an opinion, for this same repetition would expose to the development of a local affection."

After Hunter, I have nothing more to say to you to-day.

Yours, &c. RICORD.

TWENTY-SIXTH LETTER.

MY DEAR FRIEND,—This letter will perhaps appear to you a duplicate of the discussions at the Surgical Society of which l'Union Medicale has given a report; but you know that it is not my fault if they oblige me to repeat often the same thing. This applies to those who do not wish to understand, for I will not say that it is for their interest not to understand. I suppose that my adversaries are governed by one influence only, viz., that of science and of truth. I have the right also to exact that they should suppose me influenced by no other. I shall continue, then, to speak to you upon buboes.

After having denied, in the most absolute manner, by means of reasoning, experiments and observation, the existence of the necessary venereal bubo of some writers upon syphilis, or of the bubo called primary, I ought to tell you to-day what are venereal buboes, as I understand them. It is certainly one of the clearest points in pathology for those who have yet a transparent pupil, a sensitive retina and a brain without prejudices. We must first take the part of the patient, and afterwards that of the disease; we must know what glands are, and in what condition they are found in the *delinquent*, before the offence, so that we may distinguish them from those which have not become diseased till after an affection supposed to be venereal. This established, and according to the law that the venereal maladies are not the only causes of glandular affections, which they may complicate, or which often complicate them, let us see what really takes place in those subjects who have no other pathological pretext.

In the largest acceptation of the word, venereal symptoms, whether virulent or not, blennorrhagia and the chancre, may give place to sympathetic buboes: the word is here well placed as regards diseases which are themselves the result of unfortunate sympathies. These sympathetic buboes, of a nature essentially inflammatory, are ordinarily seated only in a single superficial gland; they yield quite easily to antiphlogistics and to resolvents, and in the rare cases where they suppurate, they never yield *an inoculable pus.* They are the only ones which can accompany blennorrhagia when it is not symptomatic of an urethral chancre. So, that we can say, *that a blennorrhagia* *which, during all its course, has never furnished inoculable pus, will never give rise to a virulent bubo.* This is again one of those laws against which the anarchists can do nothing, and to which the power of the lancet, which they have just recognized, will make them submit at will.

But these sympathetic buboes, these inflammatory glandular enlargements, that so many other causes may produce, such as cauterizations badly or inopportunely made, or any other irritant, do not of course constitute a specific affection; the venereal diseases are for them only as common causes, and they belong to them only indirectly, or as a simple complication.

The specific buboes, which we have here to study distinct from the other diseases of the lymphatic glands, can only be the consequence of virulent venereal affections; that is to say, of syphilis. They are either the mediate product, successive, if you will, of the contagion, or the result of the constitutional infection: this constitutes two kinds, perfectly different, and very important to understand.

The first kind of syphilitic bubo contains two varieties, almost always confounded by the greater portion of writers upon syphilis. You can convince yourself of this deplorable confusion in certain recent treatises.

The first variety of the bubo, mediate or successive, is that which follows the *non*-indurated chancre, and its different phagedenic varieties. This bubo of *absorption* is not *inevitable.* Every non-indurated chancre does not strictly give rise to it; we may even say that there are more non-indurated chancres without buboes than otherwise. These buboes are the necessary terminations of the *direct* lymphatics, the extremities of which bathe in the chancre, either of the same side, or of the opposite one, when the vessels cross the median line. This connection is necessary, and when this does not happen, the buboes do not follow. I can thus explain their frequency, as the consequence of chancres of the frænum for example, and understand why I have never seen them follow numerous inoculations which I have made upon the upper part of the thigh.

The bubo that we observe with the non-indurated chancre, not only never precedes this latter, *which ought to occur often or at least sometimes, if it may happen without it*, but it ordinarily does not show itself till after the first week, in the course of the second, and under certain circumstances not till later—after some months of duration, of years even, provided that the primary ulcer still persists in the specific period. In a patient of my colleague M. Puche, it was after three years duration of a serpiginous chancre, that a virulent bubo manifested itself. This is always the law; that it is not until the ulceration happens, whether it be sooner or later, to meet with the necessary connections, or that these have not destroyed them by its progress, that it allows its virulent pus to pass into the lymphatic vessels, which convey it directly to the glands, without being themselves infected, or causing infection in its transit.

With the non-indurated chancre, be it open, or concealed in the urethra, in the anus, in the vagina, in the mouth, the bubo affects most frequently only one gland when the chancre is solitary; *it affects only the superficial glands*, so that this division of buboes into superficial and deep can be in no wise applied to the virulent buboes. The bubo, from virulent absorption, symptomatic of the non-indurated chancre, is inflammatory and ordinarily very acute; it inevitably tends towards suppuration.—Whether the virulent pus furnished by the chancre at the specific period is arrested in a lymphatic, or whether it has arrived at a gland, it is a sort of inoculation which it produces, and which by reason of individual dispositions gives place to symptoms analogous to those from which it emanates; that is to say, to chancres of the lymphatics or the glands, with a tendency to increase and to suppurate. But in this *intra-lymphatic* inoculation and by absorption, if I may thus express myself, there supervenes, as in the inoculations upon the skin and upon the mucous membranes, a common inflammation of the neighboring parts. And while that the lymphatics and the infected glands proceed to suppurate specifically, their phlegmonous atmosphere will furnish only simple pus. These two layers, so distinct, so independent at first, so easy to understand, have not always been known. You will remember that even one of your recent correspondents, he who loves to confound everything, has found it surprising that we could distinguish them. Well, these two concentric layers have different properties, which you already foresee, and which explain to you how some experimenters, like Cullerier, uncle and nephew, have been able to support the opinion that the pus of buboes is never inoculable. In fact, if upon the day of the opening of a bubo in which the pus has not remained too long a time, we inoculate with the pus which escapes, that is to say, with the pus of the phlegmonous layer, the result is negative! while that if we happen to take the pus from the deep layers, that is to say, the virulent pus furnished by the glands, the result is positive!

I have met with some cases where the infected glands, a sort of virulent cysts, were dissected out and exposed by the surrounding phlegmonous ulceration. I could then inoculate the pus from the neighboring parts without result, open afterwards the gland, and obtain a pus of specific action. When we have long delayed in opening a virulent bubo, so that the glandular pus is effused amongst the phlegmonous pus, and has had the time to become mixed with it, as also when it has already been opened for a certain time, all the pus which it furnishes is inoculable.

Hunter, that prophet upon syphilis, had already determined that the virulent pus of the bubo of absorption is identical to the pus of the chancre, and like it is inoculable, the bubo in this case being a *glandular chancre*, contagious like other chancres. It is even the pus of a virulent bubo, which he has compared to the pus of a reputed secondary symptom, in the observation cited before the College of Surgeons.

But, remarkable fact, the virulent *primary* pus is never met with beyond the first glands, in direct connection with the chancres which have been the source of this contagion. We never find inoculable pus in the deep glands, in the lymphatics which emanate from them, or at their terminations; there is a barrier which the *primary* pus has never broken through. It is experiment, my friend, it is artificial inoculation, which has taught all this, not now displeasing to those who have heretofore so calumniated it. Again, if it happened that we were in doubt; if what the pus from the bottom of an abscess produced upon the borders of the spontaneous or artificial openings of a bubo, did not suffice to establish a certain diagnosis in the great majority of cases, the negative effect of inoculation for the inflammatory and scrofulous buboes, and *positive in the sole case of a virulent bubo*, would furnish the incontestable pathognomonic sign.

The remainder as soon as possible.

Yours, &c. RICORD.

TWENTY-SEVENTH LETTER.

MY DEAR FRIEND,—The second variety of the *mediate*, successive bubo, is that which succeeds the indurated chancre. This form of the symptomatic bubo merits the greatest attention, and ought to be studied with care. It differs as much from the preceding variety, as the indurated chancre itself differs from the other varieties of the primary ulcer.

The enlargement of the glands is in this case perhaps more precocious than that which succeeds to the non-indurated chancre. It is rare that the first week is passed without its manifestation, and we can say that it is almost never deferred beyond the second. If we do not meet with it sooner, it is because that we do not know how to search for it. With the indurated chancre, bubo is inevitable from the commencement. We never see it arrive at a very late period, as I have said might take place as a consequence of other forms of the primary symptom.

I have not observed the chancre specifically indurated without the symptomatic enlargement of the neighboring glands. This is so regular, this enlargement is so characteristic, that it may serve to indicate the nature of the chancre, which has preceded it, when it has already disappeared, when it is concealed in certain deep-seated regions, or when its base is less decidedly formed.

For those who well understand this form of bubo, the seat of the primary symptoms, a sort of forced entrance of constitutional syphilis, is always easy to discover, provided that we arrive in time; for the chancre is alone the absolute cause of all the symptoms of syphilis. We may easily be convinced of this truth in those patients who are laboring at the same time under secondary symptoms, and who have this variety of glandular enlargement in a regular manner, only in the neighborhood of the primary symptom. By its evidence, we may even recognize certain transformations *in situ*, unravel in some sort certain secondary symptoms, and discover their veritable point of departure, as happens in certain cases of papules or mucous tubercles termed primary, and which have succeeded to chancres *sur place*. I can now then affirm, that it is for want of a strict appreciation, of a precise analysis, and for not having seen the disease at the commencement, or because that one has suffered himself to be deceived by simple coincidences, that he can believe that the mucous tubercle (secondary symptom) can always give place to bubo. We can easily be assured every time that this, like all other secondary symptoms, shall be developed upon several regions at the same time, that it is there only where the chancre has existed, that we shall inevitably find the glandular enlargement such as I am about to describe.

As in the acute virulent bubo, symptomatic of the non-indurated chancre, a lymphangitis may precede and accompany the glandular enlargement which we are here discussing. In this case the lymphatic cord is hard, indolent, sometimes knotted upon the course of the valves; we can easily raise it up and circumscribe it when it is seated upon the dorsal surface of the penis. At the corona glandis, under the preputial conjunctiva, we find flexible winding cords, and if we extend upon them the semi-mucous membrane, this latter is discolored, and the cords remain white, which does not take place in the case of inflammatory lymphangitis. This condition of the lymphatic vessels, the consequence of the indurated chancre, might be confounded with other lesions of these same vessels if we had not the indurated chancre from which the diseased vessels emanate, and the affection of the glands in which they terminate, to distinguish them. Besides, in this affection of the lymphatics, the neighboring skin, without changing color, is frequently œdematous; but it is a variety of œdema in some sort gelatiniform, and upon which the finger does not make an impression. The glands, as in the other varieties, become much more tumefied on the side corresponding to the chanere, where there exists only one; this side may remain alone affected, but often the opposite side is equally attacked.

Whether one side alone or two at onec are affected, the infection is very rarely eonfined to one single gland. In the large majority of cases, the buboes are *multiple.* It is a rule, if not absolute, at least very general, that we see formed within the lymphatics, proceeding from indurated ehancres, what we may eall the glandular *pleïades.*

It is at first a simple indolent tension, which passes almost unperceived by patients and even by physicians, as we have the proof in the observation of M. Boudeville, of which mention has been made in the Society of Surgery. It is rare, excepting in a well pronounced lymphatic temperament, or with a strumous eomplication, that the bubo takes on a great volume, and exceeds that of a small nut. With the exeeption, also, of aeeessory eauscs of inflammation, entirely foreign to the nature of the indurated chancre, the glands remain indolent, hard, elastic, giving to the touch a sensation as analogous as possible to that of the specific induration of the ehancre; they do not run together to form a single mass, as takes place in the strumous bubo, for the neighboring cellular tissue does not ordinarily become engorged; they are habitually moveable upon their base, moveable under the skin, which does not adhere to them, and which neither changes eolor nor temperature. In fat persons, in females especially, they are, as it were, drowned in the fatty eellular tissue, and we must seareh for them carefully in order to recognize them. These buboes terminate almost always by slow but complete resolution, and this, quite often, a long time after the disappearance of the chancre which has given birth to them. Sometimes the glands, as well as the lymphatic vessels, remain in an indefinite hypertrophied condition. They are very rarely the seat of an acute inflammatory process, and when this takes place it is always the consequence of common causes, not of a specific one. If the successive buboes of the indurated chancre suppurate, which is still more rare, they never furnish *specific pus*, as our learned colleague from Brussels, Dr. Thiry, has so well stated, and as I myself have established; it is simple pus that they afford, if it is not the pus of a secondary symptom, but in all cases it does not inoculate.

It is well understood that we must not suffer ourselves to be deceived by recent chancres which the patient may contract upon old indurations, and which following then the law of non-indurated chancres may give place to virulent buboes with inoculable pus. These new chancres, with an indurated false base, *borrowed*, as it were, are quite frequent.

The indolent bubo which I describe here as the base of the *specific* induration of the indurated chancre, is already a condition of secondary transition, of which we shall find the more complete continuation in the constitutional buboes properly so called, or the enlargement of the posterior cervical glands, constituting the second kind of syphilitic glandular enlargement, upon which I shall have to speak to you at a future time.

According to what precedes, permit me, my friend, to offer the two following propositions, the entire bearing of which you will understand, as regards the prognosis, and which an experience of twenty years authorizes me to advance with certainty.

First,—Every bubo which suppurates specifically, that is to say, which furnishes inoculable pus, is never followed by the constitutional infection. This is a sign more important than the absence of the induration of the chancre which has preceded, and which may deceive.

Second,—The numerous indolent buboes, the consequence of an indurated chancre, is a further and sometimes the only proof, when we have not been able to establish the induration of the chancre, that the constitutional infection is certainly effected.

Now will you again permit me some therapeutical reflections which follow from the principles which we have laid down and admitted.

And, first, we can no longer admit at the present day more than one method of treatment for the venereal bubo; for, as we have just seen, the venereal bubo does not constitute a pathological individuality, it is far from being always the same, and its differences consist principally only in its greater or less depth and acuteness.

We cannot, as in the time of Bell, without taking into consideration their point of departure, their intimate nature, have the pretension to prevent, with certainty, the suppuration of buboes, or to determine it at pleasure. These ingenuous day-dreams of syphilitic writers of former days have vanished. No one believes at the present day that we can make pass through the same vessel which has given passage to the virus a sufficient quantity of mercurial ointment to destroy this virus in the gland where it is arrested. We know too well that mercurial preparations placed in direct contact with virulent pus, upon primary venereal ulcers, or upon the chancrous ulcerations of buboes, not only do not always neutralize the specific morbid secretion, but that very often, on the contrary, they render it much more active.

If we can, in the great majority of cases, prevent the suppüration of sympathetic buboes by the methodical use of antiphlogistics, and of resolutives, we fail in the bubo of absorption, which follows the non-indurated chancre. Whatever may be the means we employ, we cannot determine a *specific* virulent suppuration in the bubo, symptomatic of the indurated chancre. It is for want of the knowledge to determined the species, that we have been so often deceived, and believe in certain results.

You know that it is understood that I shall not lose myself in too many details, but you will permit me to put on a few leeches. Well, when the acute buboes succeed to non-virulent venereal symptoms, to blennorrhagia for example, we can apply leeches at quite advanced periods, without disquieting ourselves much whether the bites are at a greater or less distance from the centre of the inflammatory foyer. In these cases, on the contrary, when the point of departure of the bubo is virulent, when it is a non-indurated chancre which has preceded, and when the rational diagnosis permits us to admit the existence of a virulent bubo, if we can still combat the inflammation by leeches, we must concentrate them upon the very point inflamed; for if suppuration supervenes, and the bubo opens, or is opened, every leech bite which is not cicatrized will become inoculated by the pus which this foyer will furnish.

I have seen very grave accidents happen in similar cases from not having understood the laws of inoculation; numerous leech bites become successively infected, and give rise to as many chancres, the succession of which has by no means diminished the intensity. The most remarkable example was furnished me some years since, in the case of a financier, in whom thirty leech bites became as many chancres, which afterwards took on the serpiginous form. The primary affection had cost ten thousand francs; the cure was not as dear, although the treatment lasted more than six months.

A young woman who had witnessed a similar accident in her lover's case, came one day to consult me for acute sympathetic bubo. I advised leeches; she began immediately to cry. I asked her if it was the fear of the pain the bites would occasion her, that troubled her? She replied no; but that it was on account of her profession, which consisted in standing as a model for painters. Suddenly she consoled herself, in saying to me — After all, it can be done, since I stand at this moment for a dressed saint! In fact, at the next *salon*, I recognized my patient as a repentant Madeleine!

This, my friend, is historical, and you have given me the liberty to tell the story.

In the opening of the suppurating buboes, when they are not virulent, whether you make one or many openings, you will succeed most frequently in obtaining a prompt cure, the result of which depends much more upon the nature of the disease than upon the operation.

But for the buboes of a specific nature, whether you make one or more openings, the pus which traverses these openings inoculates the borders of them, and transforms them soon into chancres which most ordinarily, in increasing, unite and bring about in a great number of cases, whatever we may do, the destruction of all the skin that covered the abscess. Those who believe in the constant efficacy of numerous punctures, have not seen everything or have not said all. When the abscess is of but little extent, we must make only one puncture or an incision; when the skin is still thick and the foyer too large, we may have recourse to numerous punctures; but if the separation is considerable, the skin rendered thin and altered, the Vienna paste, wisely and properly employed, affords a more rapid cure, by destroying quicker, within proper limits, what diseased nature, who is at this time less intelligent, takes a longer time to *gnaw* irregularly. When we understand how to do it, the traces of these artistical cicatrices are much less conspicuous and deformed than those which are otherwise obtained.

In all cases, when we think we have to do with a virulent bubo, we must rather open it too soon than too late.

Don't be impatient, my friend; I have hardly anything more to say to you, for I come to the sympathetic buboes of the indurated chancre, in regard to which a great number of persons have given themselves much useless trouble, and which, except the complications which require a particular treatment, antiphlogistic if inflammation supervenes, or antistrumous if scrofula accompanies them, leave almost nothing to do locally; the antisyphilitic, mercurial, general treatment being the essential, we may say the only method to bring about a cure.

Whether the mercury penetrates by the digestive passages or by the skin, it acts efficaciously against this kind of bubo, without the necessity of running through such or such a vessel, and without following strictly such or such a passage. This does not exclude the utility of mercurial frictions, the use of resolvent plasters, and the good effects of compression. Yours, &c.

RICORD.

TWENTY-EIGHTH LETTER.

MY DEAR FRIEND,—I come now to a question, as they say, all palpitating with actuality; it concerns the constitution! But do not fear, gentlemen of the parquet. Let it be well understood, it concerns only the syphilitic constitution. Alas! they are no more agreed upon this than upon the other, and all the efforts which I have made to come to an agreement, have only served to bring the antagonists to a denial even of the principles which they have always professed.

Yes, my friend, the pretended *conservatives*, the classical, those who will believe only in the dogmas laid down by the *Fathers* of syphilis, have become heretical; they deny to-day what they wrote yesterday; they retract tomorrow what they write to-day. Veritable revolutionary retrogrades, levelling the immortal works of Fernel, Hunter, &c., they plunge us again into the darkness, the disorder and confusion of the fifteenth century; they wish to carry us back to that epoch in which syphilis, rendered active by an epidemic influence until then unknown, struck patients, physicians, and the entire world, with a profound stupor, and made them believe in the greatest marvels. Proteus-like, with indefinite and intangible forms; cameleon-like, with colors continually changing and deceptive; the last plague which quitted the box of Pandora, or fell from the stars, according to the political and practical Fracastor, syphilis propagated itself, acted, infected, destroyed without bounds, without measure, without rules, without limits of time or space, and drew in its course the grievous cortege and the innumerable theories of all human infirmities. But, my friend, are we to-day in 1851? Allow that I remain within my time and my generation, and that I study syphilis by other methods and processes than those which the historians of the epidemic at the end of the fifteenth century made use of. Now, what do we see at the present day? If the painting of Alexander Benedicti is not effaced, it has at least lost, thanks to the progress of hygiene and of therapeutics, its vivacity, and the eye, less troubled, can seize upon all its shades.

If I have been one of those who have most strongly rejected the impulses of the physiological school in order to save the syphilitic virus from the storm of *inflammation* which threatened to carry away all, I shall combat with the same energy these retrograde revolutionists who wish for laws in pathology no longer, and who striving to deliver up every thing to the caprices of chance, bring into this part of the medical domain, an inexpressible love for that anarchy which they have borrowed from other strange dogmas.

Although I am often forced to leave a long interval between these letters, that I have so much pleasure in addressing you, you have not forgotten or lost sight of the logical order, which is the clinical order, in which, so far, the primary venereal symptoms which we have had to examine are produced; I have insisted much upon their different nature which constitutes two orders of them—*the non-virulent and the virulent*, and the varieties of these last, which alone belong to syphilis.

I have already said to you, and it is here especially that it must again be repeated, the general syphilitic poisoning of the constitutional syphilis, or the diathesis, as you like, can be established only as a consequence of the chancre, whatever may be its seat, or by way of inheritance. Be assured that I am not going to bring up again, all the arguments upon which I have rested in order to establish this important proposition, and to separate definitely the blennorrhagia properly called, from the ulcer which constitutes the necessary primary symptom of constitutional syphilis, and which proposition only fails in that form of syphilis which inheritance produces.

No constitutional syphilis without the chancre, or without father or mother constitutionally affected with syphilis. Herein is a truth which I can call more consoling than the doctrine which I combat; a doctrine which makes syphilis an invincible enemy of the human kind, present everywhere and everywhere invisible—which like the lion of the Scriptures is without ceasing on the watch, *quærens quem devoret*. Yes, it is my hope that, in a near future, this phantasmagoric doctrine will be appreciated by all as it should be, and that it will only frighten those who will not approach it. What increases my hope is the fact that efforts have been very recently made to restore it to an honorable place; and if you did not afford us also frequent examples of polemic courtesy, I should add that they were the last convulsions of an expiring doctrine.

But does the chancre always produce the general infection? If it does not always produce it, what are the circumstances in which it is established, and what takes place after this? These are questions which I should wish to be able to answer *in extenso*, but which the epistolary form forcibly restricts.

First, you have seen that the chancre was the only symptom which we can produce with inoculable pus, which all experimenters have produced, and the same as M. Vidal himself produced when he inoculated M. Boudeville. You have also been able to assure yourself, that nature does no otherwise than art, when we know how to imitate her. The chancre is, then, the primary symptom which follows contagion, and consequently the primary symptom in spite of experimenters, *who inoculate secondary symptoms of all kinds*, and who by consequence do not consider the chancre as a primary symptom. There are for them primary syphilitic eruptions, primary buboes; but there are no more primary ulcers. Read their books, read their journals! I do not know but that one day the infecting coitus will become a consecutive symptom! That would be slightly primitive.

But in admitting *the autocrasy* of the chancre, I have already told you that daily observation proves that all chancres do not give rise more certainly to buboes than to the constitutional syphilis. I have already told you that the *indurated chancre* alone inevitably determined the bubo, and especially the syphilitic infection; that the induration was the proof of general poisoning, and, in a measure, the first secondary manifestation. They have made me say that there was not constitutional syphilis without an indurated chancre, when I have only said that there was no indurated chancre which was not followed by constitutional symptoms, which is not exactly the same thing. In fact, we sometimes see, but very rarely, constitutional manifestations supervene in cases which appeared exceptional, but which are not so in reality.

I have told you all which may deceive in the research for the specific induration of the chancre, and how we may complete the diagnosis by the knowledge of the symptomatic bubo. The true non-indurated chancre, without glandular affection or with glands which have *specifically suppurated*, never infects the economy. These propositions are absolute; but in order to establish them, a strict diagnosis is necessary, and we must not do what my learned colleague and old pupil, M. Diday, of Lyons, did, when he wished to find non-indurated chancres which could give rise to constitutional syphilis; we must not content ourselves with statistics made up of morsels like those which some very honorable colleagues have furnished him from memory, without a *direct or an accessory* knowledge of the symptoms, and which necessity alone compelled him to accept; we must do much better than this.

There are chancres, then, and they form perhaps the largest number, which do not affect the economy, and which we may most generally recognize. I shall not enter again upon the details of this question, which I have already partially treated of in my preceding letters. I wish only to refute here an objection which has been regarded as peremptory, to the consoling doctrine that the chancre may be only a local symptom.

It has been said: how can it be that a poison, a virus, should be placed in contact with the circulation without the latter becoming affected? Do we not see, on the contrary, this poisoning take place as soon as a part of the economy is infected? But those who utter this language forget the numerous cases in which the inoculations of variola have failed, those in which it is no longer possible to vaccinate, the numerous observations of malignant pustules, of malignant carbuncle, which have been only local or destroyed when they were formed. Why should not the syphilitic virus, already less active, enjoy the same privilege? But let us not insist any longer upon this, since they will not be convinced, and let us enter upon other questions.

You already know that the constitutional infection is neither in reason of the seat, of the number, of the *extent*, or of the absolute duration of the chancre, and that it supervenes only under certain circumstances which I have endeavored to specify. Therefore it is not upon this which I wish to converse with you; it is upon the time which separates the constitutional manifestations from the planting of the virus, or of the production of the primary symptoms. What interval is there between the chancre and the first secondary symptom?

Whatever may be the mechanism by which the infection is produced in traversing first the lymphatics, or in acting immediately upon the blood; whether the virus is a ferment which finds in our humors a fermentable matter from which results a new poison, which has lost the property of being inoculated; or whether the poison is otherwise brought about, is it impossible to determine the time of incubation, as Jaques Catanie expected? Here, again, my friend, we find the famous doctrine of caoutchouc, which permits the secondary symptoms to show themselves some years after the contagion, or an undetermined number of years later, from fifteen days to thirty years and more! Is there here clinical truth? Is it what observation shows, when we know really from whence we set out, and when we earnestly care to know where we ought to arrive?

It is very certain that if we do not recognize the reputed primary symptoms; if we do not succeed in discerning that which alone ought to produce the infection, and if we consider the constitutional syphilis in all cases, as the consequence of all which might precede it, as the sum or the result of all the blennorrhagias, of all the ulcerations, of all the buboes which shall have previously existed, at no matter what distance the one from the other, we shall arrive at the results at which the author of the "Treatise upon Syphilitic Eruptions" has arrived, who, rejecting every primary symptom, admits finally more than is necessary. In some patients we shall have, as the origin of a constitutional syphilis, five or six blennorrhagias, as many chancres and buboes, at years of interval, so that the infection might have commenced thirty years before in order to manifest itself thirty years later, when the successive additions shall have produced the necessary quantity to act.

If you believe that I exaggerate, read the titles of most of the observations in the book to which I have just made allusion, and you will be astonished at what you will see there. It is absolutely, as I have already told you, as if observations upon cases of variola were given to you, due to contagions, to successive infections, in traversing different epidemics, at years of interval, and at last only manifesting themselves after a sufficient accumulation of variolic pus. It is as if they had just told you that the vaccination which succeeds the last time in an individual who has been vaccinated several times without success, was not the result of the last attempt, but the production of all those which had been made previously. You will answer that those who sustain equal errors do not know the laws of virulent affections, and that it is probably on this account that they deny them; and I must confess that I am entirely of your opinion.

But let us return to what clinical observation teaches so regularly every day, to what I will undertake to verify to the *mécréants* whenever they shall wish it. Let us see what happens after the chancre *duly diagnosed* and flanked, pardon the word, with its glandular pleïades. Well, when no treatment called specific has been made, when the disease has been left to itself,

SIX MONTHS NEVER PASS, WITHOUT MANIFESTATIONS OF THE SYPHILITIC INTOXICATION SUPERVENING.

There is here again an inevitable law which there is no means of eluding, but by the aid of a treatment. Ask rather my conscientious and persevering colleague, M. Puche, who has verified it by hundreds of observations collected by himself, and without ever having found an exception. Six months, yes, six months, and this is a very long time, for most frequently it is from the fourth to the sixth week that the secondary symptoms supervene, frequently from the second to the third month, and much more rarely from the fifth to the sixth. This is a truth, my friend, which we cannot repeat too often, which has important consequences, and of which I am as well convinced as of that maintained by Galileo.

This established, and before going farther, permit me to say a word to you upon the syphilitic *disposition*, as Hunter called it, upon that state which the primary affection establishes, and which gives place to other symptoms. It is very certainly an intoxication, a poisoning which cannot take place, as in the case of variola, vaccinia, typhoid fever, &c., but in virtue of a predisposition which does not always exist, and which the first infection prevents from being produced a second time; but it is by this very means, that a persisting poisoning is established which imprints upon the economy a serious modification, whence results a morbid temperament, that is to say, a diathesis. However, you know that in certain treatises upon general pathology, the constitutional syphilis is not considered as a diathesis, and yet is there any other diathesis which is more characterized? Is there any other general condition where symptoms more specific are produced, repeated and transmitted by way of inheritance? But what fact has not been contested?

They have moreover contested the order of the evolution in the different constitutional manifestations. More backward than Thierry de Hery, forgetful of the precepts of the judicious Fernel, and deaf to the ingenuous voice of Hunter, they wish to sustain at the present day, as I told you in commencing this letter, that syphilis is vagabond and without order; syphilis, so systematic, so symmetrical, and so regular (as we understand it), that an illustrious professor of general pathology, M. Andral, said to me one day, that it ought in a measure to serve as a key to all pathology.

It is here again, to be well understood, that in order to know how to appreciate this order, we must observe the disease in a state of *nature*, and without artificial influence, without therapeutical modifications. In this case, and the physiological school has lately furnished us a vast harvest, we see symptoms which succeed, and which differ according to the time of their appearance, by their seat, their number, often by their arrangement, their form, their duration, their termination, their influence upon generation and inheritance, and in fine, by their greater or less obedience to such a medicine, to such or such a specific, if you will.

Syphilis may be compared to a ribbon which is unrolled more or less quickly, but the colors of which change after a certain number of turns, and the *free end* which is held by the person who communicated the disease no longer resembles the other extremity adherent to the bobbin, or if you like better, the skeleton of the individual affected.

These shades, so decided, so well placed, so exact in their succession, you can never represent, or express by the acute and chronic stage, for each may be acute or chronic without this changing in any degree the other characters upon which my classification is based. No, the difference between the acute and chronic states is not the only one which exists between the primary, secondary and tertiary symptoms. Syphilis, taken as a whole, is so much the more chronic the longer it has lasted; this is evident; it is one of those great truths which need no demonstration; but the absolute duration of the disease is not the only cause of the differences of seat, and of the form of the symptom which it determines; thus the roseola, which for certain persons is an acute symptom, may be reproduced several times in the course of the first and second year of the infection, and perhaps sometimes later; while the affections of the bones, which the same persons ought to rank among the chronic symptoms, show themselves in some cases in the first five or six months of the constitutional poisoning.

You will permit me the next time to return to this subject, and to give you the distinctive characters of these different symptoms.

Still a little patience, and if some subjects foreign to these letters do not come up, we shall finish, though syphilis appears infinite in its nature.

Yours, &c. RICORD.

TWENTY-NINTH LETTER.

My Dear Friend,—I must be slightly unfaithful to my programme. You will excuse me for this, on the ground of necessity. You know that at the present time there is a discussion about the inoculation of the secondary symptoms of syphilis. A large German treatise has just been published upon this subject. I never better understood, what one of our most witty Prussian colleagues, who inhabits Paris, said to me one day, viz., that he thanked Heaven every morning that he was born a German. In rendering all possible justice to learned Germany, I observed to him, that one might be almost as well content at being born French, English, or American, and that I did not well understand the object of his thanksgiving. If I am thankful to the Supreme Being, said he, it is because I understand German, and that I have no need of learning it. This reason appeared sufficient to me, who never knew that admirable language, and who yet know all the difficulties of acquiring it.

In my ignorance, then, of the Teutonic language, I have waited until the astonishing work of M. Waller, of Prague, upon the contagion and inoculation of secondary symptoms, should be translated, in order to speak to you upon it. The translation has been given by two journals, both friendly to each other: the "*Gazette des Hopitaux*" and the "*Annales*," upon certain diseases of the skin, and upon a particular form of syphilis. These two journals have shown much courtesy towards me, and I thank them for it. The

"*Gazette des Hopitaux*" greatly blames M. Waller for having imitated M. Vidal, and for having communicated syphilis to healthy individuals; the "*Annales*," but partially satisfied with M. Waller, does not publish his work, *except with every restriction*, and certainly it is right.

However it may be, thanks to these translations, I have been able to read the work of M. Waller, which is divided into two parts; one part clinical, the other experimental, with a preamble upon generalities.

Must I tell you, my friend, that I believed, from one end to the other of the work, that I was reading German: that is to say, a tongue which I do not understand.

I have not understood, in fact, how M. Waller, who seeks to prove the contagion of secondary symptoms, the possibility of transmitting them by way of inoculation, and even the transfusion of secondary syphilis by the inoculation of *syphilitic* blood, can reproach M. Cazenave for admitting, without proofs, primary syphilitic eruptions, and should dare to say to him that parallel assertions were scarcely anything but opinions; and as experience shows in no wise the exactness of them, that they could prove nothing against the arguments of adversaries. In fact, M. Waller proves, as I have already said, that the pretended primary syphilitic eruptions of M. Cazenave are all consecutive to *chancres* well and duly verified.

But the physician of Prague, who has just succeeded in showing the possible transmission of secondary symptoms, by the contagion called physiological, and by artificial inoculation, pretends that if I have not succeeded in my experiments, it is because that, first, I have wished to produce primary ulcerations by the inoculation of secondary forms; and that, second, with one exception, I have only inoculated venereal subjects, that is to say, the same patients already affected with secondary syphilis.

My friend, I perceive that M. Waller has not understood my experiments, especially if he does not understand French better than I understand German. When I said, and again remarked, to all those who repeated my researches, that the secondary symptoms *strictly diagnosed*, did not inoculate, I not only proved that they did not produce chancre, but also that they did not give rise to any other result. As to the inoculation practised upon the patients themselves, I cannot yet understand how people who admit that mucous tubercles of the scrotum or of the nymphæ can be transmitted by way of contagion to the skin of the neighboring thigh, should not admit that if the secretion of these was truly contagious, we could artificially produce this contagion under the same conditions, and that it should not be possible excepting when transmitted from a diseased to a healthy individual. I have, however, believed until now, that logic at Prague was the same as at Paris, and that the difference in the languages was of no account. M. Waller says that in the numerous experiments that I have made, one healthy subject only has been inoculated with pus from a secondary ecthyma, and that after having stated that no result had supervened upon the third day, the patient had been sent off. The person inoculated was neither diseased nor sent off, for the inoculation failed completely, and this person was Dr. Rattier, who has drawn up all the observations of my *Treatise upon the In-*

oculation of Syphilis, and who has remained ten years near me, a period of incubation more than sufficient perhaps to hatch out something, if indeed there had been anything to hatch.

But let us come to the clinical facts upon which M. Waller lays great value; a value so great, that he has believed them insufficient, and he claims for them that faith which strict science fortunately does not compel us to give. To believe and to be certain, have never appeared to me synonymous, and so long as one fact shall remain not demonstrated to me, I shall continue among the doubters.

It is certain that it is not rare to see individuals having mucous tubercles (whatever may be the synonyme) reclaim the services of physicians in affirming that they have never had a primary ulceration nor gonorrhœa; in these individuals we may not be able to discover any trace of a chancre. But for him who knows how to search for the primary symptom and to recognize it—who knows that the patient may be interested in concealing it, or that he himself has not perceived it—who knows that it may be situated in any region, and very often concealed; for the experienced physicians, and for those who know that the chancre which infects is often that which *in the great majority of cases leaves no cicatrix*, the story of the patient, or the inability to discover the primary sore, does not justify a hasty conclusion, as M. Waller would wish.

How, when ninety-nine times in a hundred, and this is a small proportion in order that I may favor my adversaries, you find a chancre or inheritance to afford you a reason for a constitutional syphilis, and that once you are deceived, or mistaken, instead of remaining at least in doubt, you will take this apparent exception for a general rule! As to myself, the profession of faith that I have always made, and which I still make, is this: The clinical facts which I have collected in a considerable number, and perhaps in a greater number than my opponents, have not afforded me the absolute, incontestable proof of the contagious property of secondary symptoms; my experiments have proved to me, up to this time, that we cannot inoculate them.

In the clinical observations cited, have they ever established, as can so often be done, when there is question of the contagion of the chancre, the condition of the patient who had transmitted it, at the moment of this contagion, and followed the diseased patient from the first day of the suspected connection, after being strictly assured upon his anterior sanitary condition? No, never! In all these histories, in all these stories of the thousand and one nights of syphilis, what do we see? Patients who arrive several weeks, several months after the contagion, and just at that period of time when they and those whom they have infected should be in the secondary stage. Let us see, rather, my friend, the observations of M. Waller himself, who I believe to be of very good faith, and tell me if they differ in any point whatever, from those that I have already had occasion so often to comment upon in my preceding letters.

There is question first, about "a respectable private family of Prague," such as without boasting, we have many of at Paris. In this family a daughter, a child of two years, presents some mucous tubercles upon the nymphæ, upon the perinæum and about the anus. The father and mother assert that they have never had venereal disease; the other children, *eight in number, are well, and have always enjoyed good health.** In searching the cause of this symptom, we discover that the nurse, admitted into the family only within three months, has some mucous tubercles at the corner of the mouth and upon the internal surface of the lips, upon the tongue, tonsils, and upon the velum palati. She has upon her some isolated spots, covered with a solid exudation; † we find mucous tubercles upon the nymphæ, and (here we are) *the distinct cicatrix of a chancre* upon the fourchette! Ah, Monsieur Waller, France has never accused the learned and conscientious Germany of fickleness; far from it—and yet what shall we think of your distraction in citing a similar observation, when you were not compelled to it.

Three cases which follow, are perfectly analogous—dispense with my citing them; for you will always be convinced, like myself, that you are reading a foreign language, and that you do not understand German.

In fine, in order not to fatigue the reader, and as a moral to the preceding fables, M. Waller cites the observation of three sodomites who had some mucous tubercles about the anus, and who had affirmed to him that the malady had commenced there and in this manner; that one of them had communicated it to his brother in sleep-

* How fortunate that all the family were not infected, as the villages of Portal and Vercelloni were.

† What was this?

ing with him! Fortunately, the history of the case finishes these.

After these admirable proofs of the contagion of the mucous tubercle, M. Waller, not always appearing to understand French, any more than I do German, takes as mine, the opinions that I combat and comment upon, relative to the mucous tubercle, in the work that I published in 1838. This error is a difficult one to surmount, unless we explain it by the same reason, viz., that he has not understood my propositions which he cites, and which I ask permission of you to reproduce, inasmuch as since 1838 I have only the more and more confirmed them.

1st,—The mucous tubercle is never inoculable; this is also the opinion of M. Vidal.

2d,—It ought to be placed among the secondary symptoms; it is a proof of constitutional syphilis.

3d,—The secretion which it produces can, in acting as an irritating matter, determine inflammation of the tissues with which it is brought in contact.

4th,—When the mucous tubercles, or the mucous pustules, have transmitted syphilis to another individual, it is because at the moment of the contagion, there were other symptoms *specifically contagious, as in the observations of M. Waller.*

5th,—Like the other secondary symptoms, the *true* mucous tubercle can be transmitted only by way of inheritance.

The efforts that I have made to arrive at these conclusions, are not great, as M. Waller would suppose, and they have in no way fatigued me. I have only taken the pains to study the chancre, as you know, to follow it in all its phases, and I have thus learned not to confound it with the mucous tubercle, which it not only resembles at a certain period, but which finishes by taking on not only its aspect, but even its nature; that is to say, it passes from the condition of a primary inoculable symptom to that of the secondary which is not inoculable. It is not my fault, if nature does this, and if the chancre is not the same at its commencement and at its end; I obey Nature, and this is all. As to the rest, this does not trouble me; for I do not believe, as M. Waller, that it is *very fortunate* that there are only primary and secondary symptoms, and that it would be a great misfortune if science succeeded in discovering the process of *fusion* between the oldest and the youngest branch of syphilis.

Here we are, again, with the nurses. It is the person named Watzka, No. 2950, who furnishes an overwhelming proof in favor of the transmission of secondary syphilis from the child to the nurse, and *vice versâ*.

This woman, at the moment of her admission, presents at the base of each nipple an oblong mucous tubercle, having upon the right breast the volume of a bean, upon the left that of a pea, *reposing upon a large base* and covered with a plastic exudation; there exists a deep ulceration upon each of the tonsils and a catarrhal inflammation of the throat. The 9th of March, a spotted and papulous exanthema, extremely abundant upon the entire cutaneous surface, is added to the preceding symptoms. The genital parts, excepting some cicatrices in consequence of parturition, present nothing abnormal. The husband of the patient is very healthy.* She pretends to have been infected by the child, which had been confided to her by the establishment of the Enfans Trouvés, three months before (Dec. 1847). At the end of the third month, towards the middle of February, she remarked first upon the left breast, and seven days later upon the right, a red spot, a little excoriated, which gradually became elevated, and which later acquired the tuberculous form already described; as to the affection of the throat, the absence of subjective symptoms did not permit the patient to state precisely the commencement of it. Besides, at the end of four weeks, she is cured by the employment of the proto-iodide of mercury, and by warm baths. The foundling which had been confided to her was a daughter, which at that period was perfectly healthy, and had consequently neither primary nor secondary symptoms; but she soon after had upon the visage and especially upon the lips, a pustulous eruption, to judge of it from the description given by the nurse. It is not until the end of three months that she restored the child to the establishment of the "Enfans Trouvés," where it died a little while after, at the age of four months. *I was not able, it is true, to procure any information upon the manner in which the syphilis acted during the life time of the infant;* only, upon the register of the hospital, I saw that it had been treated in the department for sick children for a syphilitic pemphigus. In the account of the autopsy, among the signs upon external inspection, are mentioned some scales, eschars, and cicatrices of a bluish and deep red,

* It appears that the secondary symptoms are not contagious for him.

especially about the mouth and neck. General anæmia, with catarrh of the bronchia and colon, were given as the cause of death.

At the same time that she suckled this foundling, Watzka also nursed her own child, a little daughter, strong and robust. "This child, aged nine years, had, according to the report of the mother, some days before its entrance into the hospital, an eruption upon the right thigh, an eruption which we recognized to be formed by *syphilitic tubercles* of the skin. They were scattered upon the external part of the thigh, had the volume of a pea, were almost circular, of a dirty reddish tinge; some were dry, others covered with scales, others, in fact, had commenced to ulcerate. Upon the rest of the body there was a spotted and papular exanthema, similar to that of the mother. A few doses of calomel; afterwards lotions with the corrosive sublimate, and warm baths, cured this infant in the space of three weeks.

"Already the progress of the disease in the mother and in the child had struck me by its singularity, and made me think of a contagion communicated from the foundling; but what confirmed me still more in this supposition, was to see present herself upon the first of April, in my wards, the mother of Watzka, an old woman of seventy years, thin and haggard. With the exception of some mucous tubercles upon the nipples, she presented the same syphilitic manifestations as her daughter, viz., deep ulcerations upon the two tonsils, a spotted and papular exanthema upon the entire body. The syphilitic eruptions were excessively numerous, and were at first developed upon the left cheek and upon the left side of the neck, where this woman who took care of the children suckled by her daughter, had the habit of carrying the sick child when she wished to appease it or put it to sleep. The genital organs offered no trace of antecedent syphilitic disease. This patient was cured by the use of corrosive sublimate internally."

Ah, M. Waller, you who find others fickle and some times obscure, are you sincere and clear here? Have you brought your clinical knowledge and experience to bear upon this observation? How, without hesitation, nor taking into account the time since Watzka was infected, can you call the ulcerations of the breasts which you so well describe, with *a large base*, mucous tubercles? I do not know how mucous tubercles are made at Prague; but at Paris your mucous tubercles would be well-marked *indurated chancres with a large base*, and at the period of exuberant reparation (*ulcus elevatum*). You say nothing about the neighboring glands; we see that you have not the habit of analyzing your patients carefully, and that you always content yourself with a superficial examination. However it may be, I can assure you, that if you had inoculated the pus of these pretended mucous tubercles, although they evidently proceeded from a chancre, they would have given you no result.

Let us go on. It is very evident that Watzka has had a well-marked syphilis in consequence of the two indurated chancres of the nipples. But who communicated these chancres of the nipples? Was it the foundling? This child had nothing at the time that it was taken to nurse;

they at least told you so; you never saw it; they knew nothing of the history of its patients, and the commencement of the disease upon it was not seen. It became diseased during its relations with the woman who nourished it, they say; it died later of syphilis, it is possible, nay, even probable; but what is there which proves that this woman did not infect it, as she infected her own child? How can you affirm that these chancres upon the breasts of Watzka were not communicated to her by one of those processes that I have already explained, or by some other one more ingenious still? Prove to me the contrary, otherwise than by the assertion of the patient. Are you going to invoke, in favor of your hypothesis, what happened to the mother of Watzka, to that woman of 70 years (not exempt on that account from the primary symptom, as could have been formerly seen in my wards), who, having the habit of leaning the children which her daughter suckled, upon her left cheek, had contracted upon that cheek a syphilitic eruption, as a first manifestation; consequently a primary syphilitic eruption. But this proof you do not desire — neither can you invoke it, you, who with reason do not admit the primary syphilitic eruptions of M. Cazenave.

Be facetious, M. Waller, I permit you that, for my taste does not allow me to like dull people; but be logical.

Again, you found no traces of a syphilitic disease upon the genital parts. Did you examine with the speculum? And even if you did so, you know as well as myself, that the chancre, ninety-nine times in a hundred, leaves no traces upon the vagina nor upon the neck of the uterus. Stop, let us say no more upon this observation.

Let us pass to the second observation, to Nowak. Who was it that established the diagnosis of the infant's disease, and who, the first symptoms in the nurse? It was the patient herself! And you accept this diagnosis without contestation, and without even seeing the patient for the first time until three months after the commencement. How, when I dispute your diagnosis, you, physician of the Venereal Hospital, when I call *indurated characteristic* chancre, what, from system, you please to call mucous tubercle, you do not even doubt the science, and, the true estimation in which Nowak should be held? This woman who might have syphilis in spite of her *knotty* ecthyma of the size of a hen's egg, which syphilis does not prevent, but does not produce in France, had, say you, only some cicatrices upon the genital parts in consequence of parturition! I should be very thankful to you, if in your next work you will make me understand how, in all cases, you distinguish cicatrices following chancre, from those which follow parturition, especially when they exist together upon the same regions. As for myself, I confess my profound ignorance; I often confound them. What shall I say to you, also, of the youngest child of this woman, that you received at the same time with her, that is to say, three months after the commencement of the disease, and in whom the mother had at first *diagnosed* some mucous tubercles of the vulva, which no longer existed at the moment when she was submitted to your observation? I shall tell you that I no more accept this diagnosis than that one of which you furnished me the elements in your first observation. And the son of the husband of this woman, *aged* 14 *years*, who had *a syphilis* of the bones and of the periosteum seated upon the two tibias, with superficial ulcerations of the tonsils and with mucous tubercles about the anus! Whence, and how commenced this disease? Was it by the anus? Was it by suckling? The two daughters of Rosalie Nowak, who live together with the son of the husband, in the paternal mansion, complain equally for a long time of pains in the bones! Oh, Voltaire, they rob you; for there is a story which you have given of your unfortunate colleague, Lidrac, who contracted syphilis from his wife upon their wedding night, and to whom this chaste better half gave as an excuse, that it was a family affliction! Judging from the good nature of Lidrac, we understand how the fables of Portal and of Vercelloni gained ground; but with the knowledge and *esprit* of our colleague and friend, M. Bouchut, we give the facts for what they are worth; and where there remain any doubts, we do what I have thought we ought to do, remain among the *doubters*.

But, dear friend, for some time past, I have written to you from Prague instead of from Paris. Excuse me, I return to you.

It is upon the subject of blood that we have now to treat. M. Waller does not attack too severely the chloro-anæmia; we will return to this by and by. The question is only about a difference of a few globules, more or less, in the blood of the infected person. But the important point, is the *clinical* contagion of syphilis through the blood, as a prelude to the inoculation, or of the experimental transfusion of syphilis through the blood! This, my friend, has strongly excited me. First, I know that we live in the world of possibility, even in that of the exclusively impossible. I have therefore read attentively the two observations in support of this doctrine — always mistrusting the idiom which I do not understand — and I have found that a young man, who had never had sexual intercourse, never had chancre or blennorrhagia, connected himself with a woman, and lived with her a long time. It sometime happened, after repeated coitus, that this act was accompanied, in both individuals, by a flow of a few drops of blood. Now, some months after the commencement of this intercourse, the young man perceived upon the corona glandis some condylomata *accuminés** which, in spite of repeated excisions and cauterizations, re-appeared several times within two months; finally, a syphilitic psoriasis over all the body was added to this.

The translation in the *Annales*, upon the particular form of syphilis of M. Cazenave, terminates here. I do not think, however, that the young and learned translator, M. Axenfeld, received as little satisfaction as myself, and that he did not understand the German of the last passage, which has been given by the intelligent translator of the *Gazette des Hopitaux*, M. Marc Sée. Here is this remarkable termination. "*The patient could never find the least syphilitic disease in his mistress, and a minute inspec-*

* Condylomata, vegetations, mucous tubercles, all these are the same to those who do not regard attentively.

tion did not enable me to discover the least trace"!!! Thanks, M. Sée, for this is truly prodigious. Here are two individuals, who had absolutely nothing at first, who suffer from excoriation, and who bleed, and of whom one contracts *the contagious property of the syphilitic blood* from the other who has nothing!

Here I am again perplexed by the German; I do not in the least understand this observation.*

I have seen, somewhere in a French work, by M. Richond, representative of the people, an observation which appeared to me the same; and if M. Richond had been at Prague, I should have supposed that we had made an importation from Bohemia. But M. Richond has given his observation so candidly, as to succeed in proving that the syphilis could physiologically spring up spontaneously between two healthy individuals; it has never occurred to him to cite it in favor of transmission by way of inheritance.

Now, my friend, I do not dare to speak to you upon the second observation, which has for guarantee Dr. Cejka. I am like Confucius; I respect in others the opinions that I have not, when these opinions proceed from an honest heart, and when they cannot injure any one; so that if it was concerning a fact in private practice and in a consultation, I should never have said anything, and I should have contented myself with giving my advice as regards the treatment; but since it is a scientific fact, I ask pardon

* M. Waller does not understand mediate contagions. I advise him to read the ancient authors a little, what I have written upon this subject, and the experiments of Cullerier.

of my honorable colleague of Bohemia, there are fathers, brothers and husbands, who believe themselves as sure of their children and of their wives as he was of his client, and who have been as much deceived as he was. Here is this observation, which has no need of comment, and which bears witness to the loyalty of M. Cejka.

"A man, aged 30, in other respects healthy and vigorous, had in the month of December, 1848, a chancre, which was treated by the pills of Dzondi, and which cicatrized towards the middle of the month 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 treated by a physician during three weeks, and cured at the end of that time. Fifteen days after, the other eye was equally attacked, but at the end of seven weeks the disease was cured in both eyes, and disappeared without leaving any trace. Some weeks later, this man was married to a young girl whom Dr. Cejka saw nearly every day, whose relations in the house of her parents he knew perfectly, and who never had had any sexual intercourse. At the commencement of the marriage, coitus was practised with great caution; but in December, 1849, the married couple had, during coitus, a small discharge of blood. In January, 1850, the wife had a syphilitic psoriasis upon the scalp and face, and a spotted eruption upon the entire body. In March, two little ulcerations made their appearance upon the lips, and later, some condylomata upon the nymphæ. As to the husband, he had no manifestation, either primary or secondary; he is still in perfect health."

Thus, in him, this same coitus had developed no morbid symptom; his wife, who was a virgin, was *not excoriated the first night of her nuptials*, but only some months after! Perhaps things may be thus done at Prague?

And thus the blood of the syphilitic may transmit syphilis by inoculation!

All these stories from Bohemia have, however, found great credit with some persons at Paris. Would you believe it, my friend? Would you believe that men whose mouths are full of the words, *observation*, *scientific exactness*, *rigid analysis*, would zealously receive facts of this nature, which are imperfect by all the laws of observation, and do not allow examination and analysis for an instant! Ah! if I had the impudence or the ignorance to sustain my doctrines by facts of this sort, would there be sufficient recrimination against me? It would be just, and I should not complain of it. But these facts come from abroad; they appear to come in support of an opposition so poor, that it must be that it is put to its last extremity; should they be directed against any other pathological doctrine, they would be left in obscurity and ignored; but against the syphilopathic doctrine which I defend, they seek to polish them, to cut and make them appear like precious diamonds. Let them do or say what they may, they are only false stones, and without value; the refined taste and the sure tact of your readers will not allow them to be deceived.

Ask me nothing to-day upon vaccination as a means of the propagation of syphilis. Vaccination has its enemies, like everything else. It has been accused already, correctly or otherwise, of being the cause of typhoid fever, of having prevented children who would die at a later period, from this last disease, of dying sooner from variola. We may also well accuse it of propagating syphilis. But the accusation of MM. Viari and Wegelar has not yet led to its condemnation.

I terminate, my friend, since there is nothing more discussed in the first part of the remarkable work of M. Waller. I should say even extraordinary, except hereditary syphilis, upon which all the world is nearly agreed, and the transmission by the milk against which I protest, and which M. Waller wrongly believes that the old inoculators admit.

The continuation of our programme as soon as possible.

Yours, &c. RICORD.

THIRTIETH LETTER.

MY DEAR FRIEND,—I have not yet finished with M. Waller, of Prague, and I cannot quit this good Bohemian confrère without saying to you something upon the second part of his work, that is to say, upon the artificial inoculation of the secondary symptoms.

I have told you, that, "in spite of *the probability* of the contagious nature of secondary syphilis," M. Waller, *was* neither *able* nor did he *desire* to confine himself to this. It was to the secretions, to the morbid products of the secondary symptoms, that he especially gave his attention, in order to practise their inoculation. Up to this time M. Waller has failed, like myself, and like all those who have experimented with the products of different secondary symptoms. His experiments, like those of others, have been made upon the patients themselves; and although these patients must have been under his observation for several months, he has never seen either primary or secondary symptoms appear at any time in the patients inoculated, any more than other experimenters have. Did this happen because the patients, already under the influence of secondary syphilis, were no longer fitted to undergo a new secondary contagion? But the successive manifestations, the so frequent relapses ought to permit us, on the contrary, according to the ideas of my adversaries, to consider the individual, already under the influence of the diathesis, as constituting a soil ready prepared to receive the seed of the constitutional syphilis, and to produce the secondary symptoms. You are aware that upon this subject they have paraphrased the celebrated Napoleonic expression. They tell you that when we wish to prove that the inoculation of the chancre in individuals already infected, was only the result of their syphilitic constitution, it sufficed *to scratch a syphilitic person in order to bring out the disease*. But when it was asked why in these same patients when we inoculated, when we scratch with the secretion coming from secondary symptoms, we obtained nothing, either they were silent, or they answered that the inoculation was uncertain, and that the symptoms which were not inoculable, *were by this very fact contagious*. Singular and convenient answer, which recalls to mind that which Pascal has so well castigated in his *Provinciales*.

Permit me here, dear friend, to recall an argument which has often been advanced. They tell me, if the pus of the chancre alone is inoculable, it is because it is in all its rigor, in all its force, in all its virulence; while that the morbid secretions of the secondary symptoms are perhaps modified, weakened in such a manner as to be no longer inoculable, but only physiologically contagious. Figure to yourself, two assassins, and the syphilitic virus well merits this title, the one very strong, the other very feeble, who wish to gain entrance into a house. The strongest waits until a passage is opened to him; this is the chancrous pus, which the lancet introduces. The feeblest, the muco-purulent secretion of the mucous tubercles, on the contrary, breaks through the doors, and traverses everywhere, provided a way is not open for it! The product of the secondary symptoms has its physiological passport; and, behold, it penetrates without being seen. When the school of Broussais formerly gave the special orgasm and the functions of the genital organs, as an explanation for the production of venereal symptoms, it said something almost physiological; but in the physiological act of drinking a glass of water, of swallowing a soup, where is the orgasm on the part of the glass or of the spoon which has been used by a person infected with *secondary* symptoms, in infecting the healthy individual who has made use of it after him? What are the particular physiological conditions, which have then taken place in the lips, in the tongue, and which we should not meet with, if we searched for them by the aid of inoculation? We have seen a very great number of these physiological contagions, we have already spoken of them, and when we have known how to search for them we have found the *inoculable* chancre upon the border, or at the bottom of the empoisoned cup. "Seek and ye shall find."

But let us return to our colleague of Prague. He has been desirous of giving all the exactness and all the precision possible, to his experiments; he has desired that the facts which he presented should be free from all controversy. Let us see if he has succeeded.

And, first, why has not M. Waller inoculated the patients who furnished the matter supposed to be inoculable, at the same time that he inoculated the individuals reputed healthy? He has not told us that he believed them secure at the time of the secondary inoculations, although he has never succeeded in producing anything upon them; but only he has not wished to do it, from fear, he says, that in case of success, the results would be contested. This reasoning is not good; when we have something very contestable to prove, an additional proof can never do harm.

I then engage our colleague, in his next experiments, not to neglect this, if it is only to prove that the pus which does not inoculate in the patient himself, does not prevent the healthy individual upon whom we inoculate, from having at a later period symptoms, the true source of which it then remains to prove.

However, the Bohemian experimenter in an early experiment inoculated a child of twelve years, very healthy, but affected with a tinea capitis, and placed in a hospital where syphilis is admitted, and therefore endemic, easily met with in one ward as well as in another, and even in the same ward, and thus adding its influence to all the inoculations, and to all the accidental contagions.

A scarificator was applied upon the anterior portion of the right thigh of this child, and in the wound yet bleeding made by this instrument, the pus of mucous tubercles is insinuated, which is afterwards fixed on by the aid of charpie which is impregnated with it. But whence has this matter inoculated been taken? It is the woman named Nemée who furnished it. This woman certainly presented, at the moment of the experiment, "*the cicatrix of a chancre;* she had upon the nymphæ some mucous tubercles covered with an exudation half decomposed and *half purulent;* besides, some exudations of a similar nature existed throughout all the throat, and accompanied by a commencing ulceration upon the tonsils. An eruption of spots was spread over the entire body. This woman had, at the same time, a vaginal blennorrhagia.

"The next day (7th of August), and the following days, the wounds of the scarifications, and the skin situated between them, are very slightly inflamed; but at the end of four days, all the wounds are dressed, there is no trace of inflammation, the entire surface has no longer any other aspect than that of a scarification that has healed."

"The 15th of August, I remarked at the place where the inoculation had been made, some red spots; and the 30th of August, consequently twenty-five days after the inoculation, I discovered already there fourteen cutaneous tubercles, the greater part of which had arisen in the very cicatrices of the wounds of the scarificator. These tubercles were almost all confluent, four only, situated upon the borders, were isolated; their base was large, their volume that of a bean, and many of them the size of a pea; hard to the touch, they were for the most part of a dirty red, some of them of a dirty yellow; their form was almost exactly round, and upon some was perceived a slight desquamation! Nothing morbid in the other regions of the body. (No treatment.)

"The following days the tubercles still augment in volume, and become mixed all together; they represent then a surface of the size of a thaler, knotted, projecting half a line above the level of the skin, and covered with greyish scales, which thicken and finish by forming a large crust common to all tubercles. In cleaning this surface with some tepid water, the crust is detached, and the tubercles appear under the form of flat elevations, slightly excoriated, but which are quickly covered with new, thin, dry and greyish scales.

"The 27th of September, twenty-seven days after the appearance of the tubercles and fifty-two after the inoculation, a spotted syphilitic eruption is manifested upon the skin of the abdomen, of the chest and back. This is made up of spots for the most part united, some a little prominent, isolated, of the size of a millet-seed, or of a bean, oval and elongated, some of a pale-yellow color, others of a greyish-red, without an areola, without itching or pain, completely dry, without crusts or scales. The next day, and the following days, the number of these spots augments prodigiously, and all the body is covered with them; there exists neither febrile movement, nor symptoms of catarrh, &c. In the first days of October, some of these spots are raised in papules, others in tubercles, and the whole takes on a physiognomy so characteristic, that without taking cognizance of the antecedents, every physician might immediately recognize syphilis. There was not yet any sore throat; but as this spotted, papular and tubercular syphilis proves sufficiently the success of the inoculation, I may even now give publicity to this case.

Let us first analyze the patient from whom the pus for inoculation has been taken. She had *a cicatrix of a chancre*. But because a chancre is already cicatrized, would this prevent other chancres from still remaining, and from being inoculable? The so-called mucous tubercles of the nymphæ, with their dirty exudation, were they not still primary ulcers with their *diphtheritic layer*, with their special and specific surface? Where is the differential diagnosis made by M. Waller? Is it sufficient that he tells us authoritatively, that they were mucous tubercles, when we know that he did not recognize the different varieties of form which the primary symptom may take on according to its seat, its time of duration, and the transformations which it may undergo. For M. Waller, as you know, the chancre is one and always the same, perhaps also before, during and after its existence; all which is not contained in the descriptive formula which the *parrots* of all times and of all climates have repeated, and repeat still, is no longer chancre, and ought therefore to be something else; mucous tubercles, for convenience sake! I am exacting, am I not? But how can I take seriously the diagnosis of people who confound, at every moment, as I told you in my preceding letter, the mucous tubercles themselves with the raspberry-like vegetations, under the erroneous name of *condylomata*. After so gross a fault, it may be permitted to confound sometimes the chancre with mucous tubercles; but independently of the possible error of the diagnosis of mucous tubercles, produced, they do not say how long after the chancre, of which there is yet traces, we ask what was the vaginal blennorrhagia of Neméc? What was the state of the vagina, of the neck of the uterus at the moment of the experiment? And consequently what was the nature of the vaginal secretion, which soiled the ulcerated surfaces of the vulva, from which perhaps was taken a matter that was foreign to them? You say nothing of this, M. Waller, you who always aim at precision. How is it, that in experiments of such importance, and after which, you proceed rapidly to conclude upon a truth, which you have thought until then overlooked, you neglect the most common conditions; you do not tell us that you examined this woman in the strictest manner, and that the speculum left nothing doubtful at the bottom of the well! Believe me, these are experiments to make over again, for they are defective in the most elementary conditions. I do not at all know, in spite of your good faith, which I in no way doubt, what the matter was that you collected upon the genital organs of Neméc.

There was one means of getting out of this; it was by taking the matter for inoculation from the exudation upon the tonsils.* I advise you, another time, to make this experiment, and you will give me the result of it. You know, like myself, that the difference in seat, is of no importance, and that if the secondary symptoms upon the genital organs are inoculable, those of the throat ought also to be; for the chancre of the mouth is inoculable like that of all other parts of the body.

Now for the child. You inoculated it in making upon it some deep scarifications. At the end of four days, all is finished; there are not even any traces of inflammation. But what becomes of the injured parts? How are they protected from every future contamination, so easy, so frequent in a venereal hospital? Have you placed them under a *cover;* under your beautiful Bohemian glass, as I do here? Have you isolated them, protected them in any manner whatsoever? It appears not, and you desire me not to manifest any doubt! Let it be so; for eight days after, the evolution of primary symptoms commenced, which by their slowness and progress, modified by the artificial conditions given by the tissues upon which they are seated, answer perfectly well to the *indurated*, *crusted*, *echthymatous* chancres, as happens with the cutaneous chancres, and are regularly followed like them, and in the desired classical time *(forty-seven days after* the *first* manifestation of the primary symptoms) by well characterized secondary symptoms.

What say you, my friend, of this syphilographic observation translated into French? Does it not appear to

* If it had been of the same nature as that of the genital organs, you ought to have succeeded.

you, apart from the little inaccuracies and the trifling neglects in observation which I have been obliged to notice in the original text, that it was a very common case of inoculation of primary symptoms, giving rise to all the sequelæ of the constitutional symptoms, as happened in the famous observation of M. Boudeville? Is there anything wanting here? Tell me, I will complete it. I will tell you how the virulent pus behaves when placed in the cellular tissue, and above which, the wounds, the lips of which are not inoculated, could momentarily be closed; I will recall to you, how certain leech-bites were contaminated by neighboring chancres; I will again explain to you, as I have already done in the notes which I have added to Hunter, how M. Babington could be deceived, and believe that the chancre sometimes commenced by induration, or, if you will, in the language of M. Waller, by *tubercle.*

I still believe, that the experimenter of Prague would have done well not to have cited this observation which compromises his doctrine.

Second experiment, "*with the blood of an individual affected with constitutional syphilis.*"

"Frederick, a young man of 15 years, inscribed under the number 15,676, had been rickety in his infancy, and has had, since the age of 7 years, an exfoliated lupus upon the right cheek and below the chin.* This lupus, of the size of a thaler, was cured, with the exception of a little point upon the cheek, by means of a prolonged treatment

* He ought to have been well convinced that the inoculation would fail, in experimenting upon such a subject, in whom, in case of success, there was every thing to fear from a constitutional syphilis.

by cauterizations and the iodide of potassium. This child had never had syphilis, and therefore he was fitted for inoculation, which was undertaken the 27th of July, 1850, upon the left thigh. For this experiment I took the blood of a woman (Preund), in whom secondary syphilis was developed under our observation. This young woman, formerly fine-looking, had lately contracted five or six primary ulcerations, without, however, ever having had secondary syphilis. But during the treatment of the two last chancres, which had followed in fourteen days interval, she commenced to lose flesh, to grow pale, and when the last chancre was cured and there remained nothing but a catarrh of the urethra, some tubercles made their appearance upon the skin of the face and some spots upon the entire 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 taken from her. In spite of the rapidity with which this last operation was made, the blood was already mostly coagulated before it could be transported into the chamber of the patient where the inoculation was to be made. The wounds of the scarifications (made upon the child as in the former experiment), were cleaned thoroughly, and disembarrassed of the bloody clots by washing with a tampon dipped into warm water; then the blood for inoculating was insinuated into these wounds, partly by the aid of a stick, partly by means of lint saturated with this liquid, then applied and fixed upon the scarified part. Neither suppuration nor inflammation followed; at the end of three days, the wounds were completely closed. The patient continued well.

"The 31st of August, thirty-four days after the inoculation, I remarked upon the left thigh, where the inoculation had been made, two distinct tubercles, having the size of a pea, of a pale reddish tinge, externally, without itching or pain. The following days they increased, became united at their base, covered with scales, and an indistinct red areola surrounded them both. The base of the tubercles, that is to say, the subjacent skin, and the sub-cutaneous cellular tissue, became firm, resisting (indurated), and upon the surface of the tubercles an ulceration was formed, which gave rise to the production of a thin brown crust. It is thus that an ulcer was formed towards the 15th of September, whose base had the dimensions of a pigeon's egg, and whose borders were surrounded by a red copper-colored areola, and covered by the crust in question. This crust being raised up, the bottom of the ulceration became visible; it was excavated, lardaceous, and bled easily about its borders. Some days after, *an isolated tubercle* was also formed upon the right shoulder, large as a pea, reddish, and covered with thin scales; *although the patient could not fix the day of the first appearance of this symptom.* The general health is maintained.

"The 26th of September, and the following days, Friedrick complained of loss of appetite and sleep.

"Oct. 1st, sixty-five days after the inoculation, and thirty-two after the appearance of the first tubercles, an exanthema made its appearance upon the skin of the abdomen, back, chest and thighs; an exanthema which we recognized to be *a well-marked syphilitic roseola.* There were some spots exactly like those described before (in the first experiment), only in certain points they were a little more elevated. The ulceration upon the thigh had acquired the size of a thaler, still preserving its excavated appearance, lardaceous bottom, and its copper-colored border.

" In the following days, the eruption became so abundant that the entire body, without excepting the face, was covered with it, and seemed to be spotted like a tiger. Besides this, there was neither itching, pain, nor symptom of catarrh or of fever.

" On Oct. 6th, several spots, principally upon the internal part of the thighs and belly, are raised up like papulæ and tubercles, and from this the diagnosis of the syphilitic infection, even without knowledge of the antecedents, became as easy as in the preceding case."

In this experiment, the blood which was made use of, appears certainly to have been taken from a woman affected with constitutional syphilis ; but was it the blood of this woman which gave the syphilis to the unhappy child, the subject of the experiment ? A child, scrofulous, affected with lupus, with a skin such as you are acquainted with in such patients, living after the experiment among venereal patients, always without any precautions, without guarantees, without having protected the cicatrices so subject to become irritated, to become excoriated in certain subjects, and to furnish afterwards an easy entrance to the contagion, in almost constant circulation in venereal hospitals! Thus, as it is not to the patient who furnished the blood that must be attributed all the symptoms which developed themselves in succession, the two tubercles developing themselves not until thirty-four days after the experiment, are for us, *due to another mode of contagion* from which they did not guarantee this little patient! For while the evolution of the chancres *with indurated base* is made upon the thigh in the most regular manner, in proportions but little exaggerated, since the base of these chancres was the size *of a pigeon's egg*, which depended probably upon the accompanying pathological condition of this little patient, we see another tubercle of the same form, of more regular proportions, upon the right shoulder, *of which they neither knew the origin nor the time of its first appearance*, and which is not probably the direct result of the inoculation, unless a blade of the scarificator slipped. But what produced this tubercle of the shoulder? Whence did it come? What matter is it—they do not take it upon themselves to explain; it is sufficient to explain the development of those upon the thigh, from the fact of the inoculation of the blood, inasmuch as there is nothing more to ask. However, this tubercle of the shoulder is not yet a symptom consecutive to the first *secondary symptoms of inoculation;* for it appears at the same time with them, while that the true secondary manifestations, very regular, *very classical*, did not appear until *thirty-two days after the primary symptoms.*

These last symptoms have been established by numerous and honorable colleagues, whose knowledge I in no wise question; who have faithfully told what they have seen and perfectly recognized. But in spite of their number and the authority of their name, before which I am ready to bow, if they had united and offered their testimony in order to certify that the infection ought and could be brought about only according to the theory of M. Waller, I should remain convinced that M. Waller was not the only one deceived.

But M. Waller is not fortunate; I thought that Wallace was dead; I have even pretended to have added some words to his funeral oration. It appears that I am mistaken.

However it may be, if I had commenced the reading of this Bohemian work at the end, instead of the beginning, I should have perhaps dispensed with commenting upon this last and astonishing observation, for the violent attack of its author against my friend Diday of Lyons, would have made me think that he did not believe in the possibility of inoculating constitutional syphilis, unless his pretensions stopped at the secondary symptoms, and that the blood of the tertiary was not more mischievous, in spite of the influence of the syphilitic subjects of that stage upon inheritance, the analogy of which M. Waller invokes when it is necessary for him. M. Waller is here right, in spite of himself, in affirming that my friend Diday produced nothing in inoculating the blood of patients suffering from tertiary symptoms; but M. Diday can say, in his turn, to M. Waller, that he has done no more than he, in this respect, with the blood of those affected with secondary symptoms; for if one does not acquit him of the syphilis that he communicated to the patient in his first experiment, he ought to give him the most complete absolution in that of the second.

I make a proposition to the promulgators of the opinions of M. Waller among us; that they make bold to present the facts that I have just cited to the *Anatomical Society*, and to the *Medical Society for Observation.*

But they will not dare to do it!

After this, my friend, you will permit me to say to you, that I have not made a step farther in the knowledge of the German language, and that I shall not understand the new propositions of M. Waller, and his conclusions as regards the *sanitary police, and legal medicine,* until he shall have given us some observations which I shall not be able to translate by common sense, without the aid of German, as I have been able to translate those which he has just given us with so much pretension.

It is for you, and especially for your numerous and impartial readers, to decide whether I have gained my battle of Prague. Yours, &c. RICORD.

THIRTY-FIRST LETTER.

MY DEAR FRIEND,—Before our excursion to Prague, we were upon the manifestations of constitutional syphilis.

I told you that when no treatment had been directed against the chancre, we saw its symptoms appear in a given time, and follow a certain order, which allowed us to classify them.

In fact, in spite of all efforts to obscure it, as soon as the constitutional infection has taken place in consequence of the primary symptom, the patient has acquired what Hunter rightly called the syphilitic disposition! that is to say, the *diathesis*, and from this moment, symptoms proceed sooner or later to show themselves, and progress more or less quickly in different places, and upon different tissues.

And, first, in what may be considered up to a certain point as a period of incubation, the primary effects which we often observe, are some disturbances of hematosis and of innervation more or less pronounced.

Prior to any other symptom, in a great number of the analyses of the blood made with the greatest care by M. Grassi, and recorded in the inaugural thesis of my pupil and friend Mr. Macarthy, I have been able to establish the fact of the diminution of the globules of the blood, the cholero-anæmia, which accompanies the secondary symptoms, properly so called, and which is often very pronounced.

At this period, also, and frequently before the appearance of any other symptom, and as a primary consequence, troubles of vision, weakness of the muscular powers, neuralgic pains of the head, *rheumatismal* pains of the extremities, sometimes supervene. These *precocious secondary pains*, which may also manifest themselves a little later, at the same time with other secondary symptoms, to relapse, either alone, or with these, are not found at another period when one knows how to recognize them, and when one does not confound them systematically with another order of pains.

It does not enter into my plan to make a detailed history of these varieties of precocious neuroses, or of the secondary period of syphilis, neuroses, which are not necessary, which even often fail, but which have certain common characteristics, which it is sufficient for me to allude to.

They consist in intermittent nocturnal pains, which manifest themselves particularly under the influence of heat, especially that of the bed; so that in those patients who turn night into day, and *vice versâ*, these attacks are inverted. The pains of this period do not regularly return each time in the same seat, and during the intermissions, pressure does not bring them back.

Some patients often even experience relief at the moment of the greatest sufferings, not only in exposing the painful parts to the action of the cold, but also in compressing them. Movement of the limbs where the rheumatismal pains are seated, rather relieves than increases these pains, which the patients complain of only in the region of the articulations, and sometimes in the dorso-lumbar region. In these cases there is no change in the color of the skin, no change of temperature, no tumefaction. Under some circumstances, there is lassitude merely, which ceases most generally when the other symptoms and the cutaneous eruptions manifest themselves.

At this period especially of precocious symptoms, we find, as one of the most constant manifestations, some glandular swellings, to which we may strictly give the name of secondary buboes.

The affection of the lymphatic glands at the secondary period merits a very particular attention; it is in a measure characteristic of this period.

This variety of glandular enlargement rarely fails, and often constitutes one of the first proofs of the infection, if we know how to recognize it. It succeeds, sometimes at the third week, but more frequently at the commencement of the sixth, to the numerous indolent glandular enlargements, *necessarily symptomatic* of the indurated chancre.

Its seat of preference is the posterior cervical, or cervico-cephalic region. We find it much more rarely elsewhere. However, I have seen upon a few subjects other tumefied glands; but we should be very careful not to suffer ourselves to be deceived by other causes of glandular tumefaction, and especially by primary symptoms in an *unaccustomed seat*, or by strumous dispositions, which everywhere favor the tumefaction of the lymphatic glands, but certainly less in the posterior cervical region than elsewhere.

The veritable secondary glandular enlargements never acquire a large size; they are indolent and most generally numerous; they never suppurate, or at least they *never suppurate specifically*. They never furnish inoculable pus.

Without doubt, and as the most part of observers have established, we only observe this variety of bubo when the skin is already the seat of an eruption, and ordinarily of a superficial kind; but I can affirm that I have found the engorgement of the posterior cervical, occipital mastoidean glands in patients who did not present the least trace of an eruption upon the scalp. My colleague, M. Puche, at the Hospital du Midi, informs me that he has made the same observation. What is certain is, that if this variety of glandular tumefaction is connected with certain forms of secondary symptoms, with which they are alone met with, these same secondary symptoms do not inevitably and always produce it in all regions, as the indurated chancre produces its glandular satellites, which, apart from this *necessary co-existence*, are very analogous, even identical in other respects. In all cases, if these two varieties of syphilitic bubo may be sometimes confounded, we should always distinguish them from that which the non-indurated and *non-infecting* chancre determines, which suppurates and *which furnishes inoculable pus*.

You will not find these secondary buboes after a certain period; you will not see them produced *for the first time* at the late secondary period, and much less at the tertiary period of syphilis. If, with late symptoms, you meet with glands which are affected, search, and you will find other reasons to explain them, and their manner of growth will be different, or the patients will tell you that these enlargements have supervened upon the first symptoms.

At the commencement of the constitutional syphilis, at the moment of that first explosion, we meet often also with a symptom which observers, who collect their observations only from books, have considered as a proof of a long-standing, grave and inveterate malady; I mean alopecia, one of the most precocious symptoms of constitutional syphilis, the first which manifests itself in some patients, and which we no longer find at an advanced period of the disease with *the same characteristics*, provided we do not confound it with calvities and with those other causes which may produce the fall of the hair.

But if we pass now to what happens to the skin, to the mucous membranes and to their dependencies, we find with the avowal even of those who do not believe in marked phases in syphilis, that the nearer we are to the moment of contagion, the more superficial and generally disseminated are the forms, or more or less confluent. You know, my friend, that with these forms have been made secondary symptoms *d'emblée*, or *secondary primary* symptoms, or primary secondary; but it has never occurred to them to regard as such the deep-seated tubercles, the gummy tumors, the affections of the periosteum and of the bone; which, after all, would not have astonished me much, inasmuch as they were so fairly on the road to this.

Follow, my friend, the syphilitic evolution, a thing unhappily still so easy to do in our days, and you will see with what regularity and with what constancy, in a given time, of which I have already spoken to you, the exanthematous eruptions, of a rubeolic or erythematous form, manifest themselves. This constancy is such that some observers, and I shall again cite my friends M. Puche and Cullerier (son), think that it never fails. What is certain, is, that they manifest themselves almost always when we know how to look for them in season, and do not suffer them to pass unperceived, inasmuch as nothing reveals their existence but the sight.

But these primary symptoms, to which succeed, sooner or later, tubercles more or less prominent, the dry forms of squamæ, vesicles, vesico-pustules, and pustules more or less superficial of the suppurative form, do not present themselves with the same characteristics at all the periods of syphilis, when we know how to refer them to their true source, to their true point of departure; to the infecting chancre or to inheritance.

We observe the same thing as regards the mucous surfaces, and the regions of the skin which are continuous to these surfaces, and easily susceptible of undergoing transformations; at first, there are simple alterations of color, but here, on account of the structure of the particular seat and functions, and the papular condition, the tubercles are delineated sooner, and progress more rapidly in order to give rise to mucous tubercles upon which so many hypotheses have been built, and upon which so much discussion is yet going on. But these symptoms, so little understood, and the particular physiognomy of which is due to accessory circumstances, as I have just said, to texture, seat and functions, do not manifest themselves at all periods of syphilis, any more than roseola does.

If you should take the pains to make a differential diagnosis, unless by a deplorable confusion of language you should confound the *tubercular syphilitic eruptions* with the papules more or less *tuberculiform*, you will not find these symptoms as the first manifestations of a syphilis contracted ten or twenty years before and not treated.

But in proportion as syphilis grows old, as it runs through its orbit, the symptoms which it produces and which tend to become more and more grave, more and more deep-seated, appear by a sort of compensation to become also less numerous, more discreet, if we can employ this word in similar cases. It is the thickness of the skin which is attacked, it is the cellular tissue which lines it which is affected, and this again has a predilection for certain regions! All things being equal in other respects, it is there where the cellular tissue is the densest that the affection shows itself. In the mouth, it is also the thickness of the mucous membrane, and the sub-mucous cellular tissue, which is invaded, and while the precocious secondary symptoms occupy the internal and superficial surfaces of the lips, of the cheeks, the borders of the tongue or the tonsils; the late symptoms burrow more deeply in the tongue itself, in the palatine region, upon the velum palati, or behind the posterior pillars, upon the pharynx, where they determine, most frequently, grave alterations and frightful ulcerations.

All this, my friend, except some rare cases of galloping syphilis, which you will again permit me to call syphilis of the *Renaissance*, and which like many worm-eaten and inconvenient pieces of furniture of that period, are fortunately disappearing more and more; all this, I say, does not show itself generally till a long time after the contagion. All this, be assured, is perfectly known to dermatologists who have done so much for the study of syphilitic eruptions, and to whom no one more than myself knows how to render justice when it is merited; but all this is also denied, if need be, when *the system of confusion exacts it.* In order to recognize the truth of what I advance, a diagnosis is always necessary a little more precise than that to which a certain antagonist has confined himself. At one time all syphilitic eruptions were bullous, or *nées bulleuses* (excuse the pun, it was unavoidable) ; at the present day we are incrusted with the mystical ecthyma that our colleague M. Baude thinks he understands.

But if a certain time is necessary, in order to arrive at the manifestations of which we have just spoken, according to the opinion of all observers, at whatever epoch you may take them, since the epidemic of the 15th century, a much greater time is necessary in order that the disease should attack the testicles, the fibrous system, the osseous tissue, the muscles and other deep-seated organs, the heart, brain, lungs, liver, &c. Follow the patients, start always from the true source, do not let go of the end of the ribbon of which I spoke to you in a preceding letter, and you will see that it is very rarely before the first six months, and often much later, that these symptoms manifest themselves, necessarily preceded by some of those of which I have already spoken.

When the periosteum and the bones become affected, pains precede or accompany this condition. These pains, true osteoscopic pains, so easily confounded by inattentive observers with those of the second period, and increasing the liability to errors into which we love so much to fall, are as distinct from them as it is possible that they could be. As regards the seat, it is upon the superficial bones and in the compact regions that we meet with them ; they are fixed, and have not the moveable *rheumatic* character ; they are nocturnal and are exasperated by the heat, especially of the bed ; they are always increased by the touch, either during the paroxysm or during the intermittence or the diurnal remission. Finally, where the pain is seated, a swelling, a tumor of the periosteum or of the bone, may and commonly does supervene.

All this, my friend, is the fruit of observation; it is not copied from the books, it is not the fruit of the imagination; for, if I have known how to study syphilis, I did not invent it; if I had, I should much regret it in a social point of view.

From observations, then, made within twenty years, upon hundreds of patients, whom hundreds of medical men who have followed my cliniques have seen with me, it results, that if syphilis abandoned to itself tends to produce more or less frequently, for a longer or shorter time, certain manifestations, these manifestations are produced at a certain time and in certain determined seats, from which result certain forms, certain lesions, constituting in some respects as many distinct maladies, connected together by a common cause, and succeeding often by gradual transitions, but also sometimes by sudden and well-decided leaps.

We may, then, admit with Thiery de Hery, Hunter and others, three well characterized periods:—

1st.—Primary symptom, the chancre:

Immediate result of contagion;

Necessary source of the reproductive virus;

Remaining in the condition of a local symptom, upon the skin or upon the mucous membrane, within certain limits;

Having the power to extend to the neighboring glands only, and to give rise to buboes ;

Finally, infecting the economy.

2nd.—Secondary symptoms, or constitutional poisoning resulting from this infection, and showing itself in the course of the first six months :

Having for seat the skin, the mucous membranes and their neighboring tissues ;

Symptoms supposed contagious, without strict demonstration ;

That we cannot yet reproduce by artificial inoculation ;

Transmissible by way of heredity by the father and the mother, separately or by means of both at once.

3d.—Tertiary symptoms, rarely showing themselves for the first time before the sixth month :

Having for seat the sub-cutaneous cellular tissue or the sub-mucous, the fibrous, osseous and muscular tissue ; certain organs, such as the testicles, heart, brain, lungs, liver, &c. ;

Not only none of their morbid secretions is contagious by ordinary contact, but their *specific* influence upon inheritance appears to go on always decreasing, only to become subsequently one of the hereditary causes of scrofula.

These periods, with all deference to those who dread the precision and the language which the exact sciences give to medicine, are easy to verify, and there is no disorder in this perfect arrangement, excepting when therapeutics intervene, so that we may say here, as I shall prove to you bye-and-bye—

Souveut un beau desordre est un effet de l'art.

Yours, &c. RICORD.

THIRTY-SECOND LETTER.

My Dear Friend,—"Order is the will of God," said one of the most amiable women of the 17th century. It appears to me that Madame de Sevigné would find that I have conformed to the supreme will, and that she would appreciate the order that I have re-established—the more assuming would say created—in this poor syphilis, which many writers have treated worse than it treats humanity.

I have told you how syphilis in its free and normal development was arranged, proportioned, and how symmetrical it was, how regular was its progress, its steps counted and measured, with what art, according to the region and its duration it knew how to remove the hair, to grow pale, or to cover itself with its copper-colored paint; finally, you have seen it, superficial, light and diffused at its commencement, become more serious, more profound and more grave in growing old.

Well, all this, as the existence of the person whom it affects, is subject to perturbations which are not always inherent in the nature of the disease, but most frequently, on the contrary, the result of accidental causes, and more particularly the result of treatment.

Syphilis, without doubt, is one of the diseases over which art has the most power! Many credulous and inexperienced physicians believe even, with the vulgar, that medicine ought always to be all powerful, and that where the disease has been able to resist it, to increase or re-appear when we have combated it, it was always the physicians and not the remedies that must be blamed. You may have seen some time since, in a medical journal, that one of our colleagues assured us, with an admirable coolness, that no syphilis could resist 110 of Dupuytren's pills. A hundred and ten! I know those who would not resist the pleasure of making here an equivocal boast.

I do not wish to, neither can I give a treatise upon anti-syphilitic therapeutics. I only wish to speak to you of the treatment in the most general manner, as I have done as regards the other questions to which I have alluded in these letters, and as regards the doctrines which I profess.

Constitutional syphilis is certainly one of the great calamities of the human species; fortunately, in spite of its frequency, it is still comparatively rare, and does not attack all those who are exposed to it. "He who wishes it, does not have syphilis," as one of our old masters, the venerable professor Dubois, said. This *inaptitude* we have found in certain idiosyncrasies, and does not the observation which has taught me that generally one did not have the constitutional syphilis twice, that one was not apt to contract indurated chancres twice, followed each time by the syphilitic evolution, at the present day well understood, permit us to believe, since syphilis is hereditary, that in some cases the disposition acquired by the parents, and which renders them indemnified, could be transmitted to the children? It is according to these ideas that I have professed, and that I still profess, the justice of which I each day verify, that we have endeavored and that we shall still endeavor to give to the economy a general disposition equivalent to that which vaccinia or a first variola ordinarily affords, in preventing the variolic virus, not only from acting locally, but especially in preventing the infection and its consequences.

In the researches of this kind, in the attempts which we may make in order to arrive at this result so desirable, a certain reserve is necessary, a great prudence; we must guard against eccentricities, and in view of the good that we seek for, we must also consider the evil that we produce.

Surely it is not my place to blame the experimental researches, after having so often invoked them in order to sustain my doctrines, and thanked them for the bright light that they have spread upon so many obscure questions, and which it would have been impossible to clear up without their aid. No, this is a part to leave to those who after having stigmatized and slandered that which is most perfect in science; to those who after having calumniated experiment, come to ask of this experiment no more than we have the right to expect from it, but which duty commands it to refuse.

M. Diday, of Lyons, brought up in our school, and convinced as we are, that he has no right to compromise the health of any one by communicating to a healthy individual a disease as grave as syphilis, in searching a prophylactic means against constitutional syphilis in the syphilitic virus itself, has experimented only upon individuals already affected, but in different conditions.

He has started from those principles which I profess, and which I recall to you.

1st.—The chancre is at first a local symptom.

2d.—The constitutional infection does not take place till later.

3d.—When the syphilitic diathesis already exists, a new chancre remains definitely local.

4th.—One may be under the influence of a syphilitic diathesis, or have acquired the immunity against a new syphilis, without the necessity of having, under given conditions, syphilitic manifestations, or symptoms.

5th.—Finally, syphilis is transmitted from parents to children, from the mother to the fœtus, through the circulation, but the older it grows, the more it encroaches upon its last tertiary phase, and the less it tends to be reproduced by generation, with the marks of its other periods; then perhaps it otherwise modifies the constitution of children.

To modify, then, the general condition before that an existing chancre had had the time to infect the economy, and to obtain this result with the syphilitic virus itself introduced directly into the blood, but enfeebled and arrived at that epoch when it could only produce a general *disposition* without syphilitic manifestations; such was the laudable pretensions of the learned surgeon of Lyons. In order to arrive at this result, M. Diday took some blood from an individual affected with tertiary syphilis, and presenting, as a characteristic symptom of this period, an exostosis. This blood has been inoculated upon patients actually having *non-indurated* chancres; and these patients, to whom no anti-syphilitic treatment had been administered, and in whom no *direct* result of the inoculation had been observed, have offered no constitutional symptom at a later period, and after the desired time which I have elsewhere determined ; one alone, in whom the chancre was indurated at the moment of the inoculation of the tertiary blood, presented the classical and regular march of the syphilitic evolution.

You know, my friend, that when M. Diday published his experiments at Paris, they criticized them strictly, they especially blamed him for having said that we could allow, momentarily doubtless, tertiary symptoms to persist, in order to furnish the prophylactic vaccinia, which ought to prevent individuals affected with primary symptoms, from having at a later period constitutional symptoms. They would have willingly brought M. Diday up to the bar *de conseil* of the hospitals of Lyons, although M. Diday only operated by transmitting the virus *from one diseased individual to another, from a tertiary to a primary.* Such were, as you know, the inoffensive attempts of M. Diday, who became the innocent cause of the attacks directed against me, and the origin of my letters which you have so graciously received. I do not know whether I ought to thank my friend of Lyons ; you will tell me this later.

However it may be, for my part I ought to combat the ideas of M. Diday, for the two following reasons :

1st, The local effect of the inoculation of the *tertiary blood* being null, you do not know if it has taken effect.

2d, The absence of constitutional symptoms upon the individuals inoculated, proves nothing more ; for the chancre in the conditions in which you have experimented, is not followed by general symptoms in the patients whom I do not treat at all.

Brought up in the seraglio, M. Diday well knew my opinion as regards this last; thus he has endeavored to render his *non-indurated* chancres as infecting as possible, in trusting to contradictory authorities which have furnished him the statistics which you know, and which, he is too serious a man to have taken for true. We do not owe less praise to M. Diday 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 does, proofs of a profound knowledge, and he merits to be read with attention.

But M. Diday has only pretensions against constitutional symptoms; he has remained convinced up to the present time, that nothing can oppose itself to the contagion, or to the inoculation of the primary symptom, the chancre. M. Auzias Turenne has gone farther; he thinks that we may render individuals refractory to the direct and immediate action of the virulent pus, and may resist the contagion of the chancre. He has arrived at this belief, from his inoculations of animals. He says that he has observed that in making successive inoculations, the last became gradually less and less intense, of shorter duration, and that finally inoculation had no effect. M. Auzias Turenne has explained this by a modification given to the economy; by a sort of infiltration of the syphilitic virus, producing what he calls syphilism or syphilization, which would be to syphilis what vaccinia is to variola; that is to say, that in order to prevent or avoid new primary symptoms, we should not even have the chance to determine the syphilitic diathesis such as we understand it, and the possibility of seeing constitutional symptoms develope themselves.

What say you of this, my friend? You do not dare to answer, even as regards the monkeys, who appear however to have a certain nosological importance. But the experimenter that I have just cited, naturally striving to apply this law to the human species, thinks that he has established that certain persons have become refractory to the chancre after having undergone a certain number of contagions. How many of these does he suppose necessary in order to arrive at this immunity? He does not say, so far as I am aware. His cases have been taken from public women, who have given themselves up to debauch for a long time, and who have chancres less often than those who commence. You know too well that all those who expose themselves to contract chancres, do not catch them, or, what is better, are not caught; that something else is necessary for contagion besides the *physiology* of one of our colleagues, and this something consists in the conditions of the tissue which we meet with as much less frequently as the parts have served a longer time, in proportion as they are more spacious, better tanned, lined, like the hands of a laborer, with a thicker and more resistant epidermis; and finally, if my physiologist wishes it, which are blunted, and incapable of excitement, of orgasm, of emotion, and of that virulent temperature which M. Cazenave exacts.

I have very often seen — too often, alas! and others have seen as I have — patients who have had chancres several times, at various periods; in whom the last were no less grave than the first, in whom numerous non-indurated chancres, having at first existed at different periods, did not prevent a last chancre from becoming indurated, and infecting the economy, and in whom this infection did not prevent them from contracting a new ulcer which did not indurate, and which was often more intense than all those which had preceded it.

I have seen chancres, and one will always find them at the Hospital du Midi, extend gradually without ceasing, by the process of the phagedenic condition, by veritable successive inoculations, especially as regards the serpiginous chancre, run over and plough through surfaces to a frightful extent, amputate the penis, hollow out all the inguinal region, cut up and furrow the skin of the abdomen from one iliac region to the other, descend upon the thighs, and if I dared so to say, *unbreech* (déculotter) the patients.

Well, these chancres have occupied months, years in attaining these limits, which are not even the extent that they may attain, furnishing still inoculable pus with results just as grave as those at the commencement; and yet here, the number of the accidental and successive ulcerations, their surface and duration, agree well, as it appears to me, with what is done in the inoculations called preventive, which are repeated at short intervals, and *in the same region.* It is true that here nature or disease does this without *preventive intention*, which establishes a difference as to intentional art. Animal magnetism, if you were a believer, would perhaps give you the explanation of this mystery.

But what shall we say in presence of what has just come to us from Italy, from Turin? Bohemia is surpassed, and the name of M. Waller ought to pale before that of M. Sperino, the most hardy and most fortunate of experimenters. Since I have seen the balloons of Paris, and all which Messieurs Poitevin and Godard can transport into the clouds, I have become more credulous, and I am no longer astonished at anything, unless it would be to see fifty public women upon whose abdomens three or four inoculations had been made during two months, once or twice a week, which gives a total of twenty-four, and in some, forty-eight and sixty-four inoculations, without there having been question of much phagedenic ulceration, or without ever one chancre becoming indurated, before that others could prevent this result, when we know with what rapidity the chancre infects and becomes indurated; though some even maintain that it infects before its manifestation, and M. Sperino tells us that it was not until he had arrived at the numbers above indicated that he could no longer inoculate! Yes, I am still astonished, and I await the report of the Commission, which I hope will give us all the details which are not supplied in the facts of M. Sperino. I await especially until they present to me an individual *syphilized*, and refractory, who shall come before the cliniciens of the Hospital du Midi, or before the National Academy of Medicine, to defy me to close combat with the weapons of my choice.

In the mean time, here are the results of the analyses which I have made, from observations known at Paris and in Italy; it is that the pus coming from *non-indurated chancres* has always been inoculated to produce analogous symptoms, and that the only time at Paris when pus coming from a primary symptom which has determined constitutional syphilis has been inoculated, the healthy individual, the student upon whom the inoculation was practised, had an *indurated chancre* and a general poisoning. If it was always thus, as I have already said, we should be obliged to arrive at this conclusion, that there may be differences in the disease, which do not depend alone upon the conditions of the individual upon whom the cause acts, but upon differences in the causes of the disease.

However, be it as it may, and in view of all the circumstances with which you are familiar, what would you think of a method, which, in order to prevent you from contracting a chancre, the risks of which you have not inevitably to run, as happens for variola, exacts that one should first communicate to you from twenty-four to sixty-four inoculations, and still without your knowing how long a time this dearly-bought immunity might last?

However, upon questions as grave, studied by men who maintain their respectability, we must look with calmness and without prejudice; doctrines and systems ought to be offered with moderation, without being exposed to be contradicted by new facts; but they ought to include only what is rigorously demonstrated. It is this incontestable demonstration, then, which I demand; and, to induce M. Sperino to give it to me, let him remember that Turin was the land of Lagrange, one of the most illustrious representatives of the exact sciences, and that he his compatriot owes me mathematical precision, otherwise I should say to him "*si non è vero*, non *è ben trovato*."

Yours, &c. RICORD.

P. S.—My colleague, M. Puche, has just practised seven successive inoculations; the last as active as the first!

THIRTY-THIRD LETTER.

My Dear Friend,—You have had the goodness to communicate to me a letter which M. Auzias Turenne had addressed to you relative to what I said in our last conversation upon *syphilization* and *syphilism*. You have desired that if I answered the letter of M. Auzias, my answer should appear at the same time as his. Your motives are justifiable; they will be understood without farther explanations, by every serious reader. You believe in progress, and you encourage it without reluctance, even under its boldest manifestations. But you do not give up your right of examination, nor your right to a wise and prudent reserve, and I congratulate you upon it. When a question as serious as that which is to occupy us is advanced, there is danger of our not attacking it in a direct manner; and it is foolish to believe that it will be stifled by a disdainful silence.

Let us examine, then, these new doctrines which M. Auzias propagates; but first give him the opportunity for this new exposition of his ideas.

To the Chief Editor of l'Union Medicale.

The same deleterious principle taken from the syphilitic pus which produced chancres, when inoculated by means of a lancet upon the arm of a soldier long a prey to syphilis, which proved rebellious to all other treatment, produced two venereal ulcers, and was followed by the cure.

PETIT RADEL.

Mr. Editor,—Just ideas exist, as well as good men. They gain by becoming known. Now, M. Ricord has given to *syphilization*, undoubtedly without intention, in your columns of 12th August last, a false appearance. Permit me, then, to explain the subject in a simple manner to your readers.

Syphilization is neither a virus nor a disease, such as vaccination or variola is. It is a state analogous to that in which an attack of variola places us. After having had the variola, we enjoy an immunity from it: so, after having undergone successively a sufficient number of chancres, we are *syphilized*, that is to say, insured against all the forms of syphilis.

Syphilism, on its part, is the *aptitude* to be *syphilized.* Without doubt all of us enjoy this to some degree. This, then, is a natural quality, while *syphilization* is a property acquired in virtue of this quality. Finally, we accept without difficulty the qualifying term *syphilizator*, sprung from the pen of M. Diday. In the same way we formerly said *circulators*, *inoculators*. This analogy is not unsatisfactory.

But afterwards come the words *saturation*, *impregnation*, and *infiltration*, when taken literally. We do not desire to be *saturated*, *impregnated* or *infiltrated* with the virus of syphilis, any more than with that of variola; in a word, we do not desire to be the focus of infection and corruption. What we pretend to say when we are *syphilized*, is, that we have undergone in a short space of time the evolution of syphilis, and are exempt from it, as we are from variola when we no longer have it. We shall accept every other rational explanation of *syphilization*, but we forcibly reject a theory which would be in the eyes of all a source of prejudice.

In order to make *syphilization* understood, I may suppose a traveller passing over the two sides of a mountain; first from the base to the summit, then from the summit to the base. This represents the person that we *syphilize.* The chancres correspond to the different portions of this route; thus the indurated chancre, mark of the constitutional syphilis, answers to the crest of the mountain, and *syphilization* to the termination of the journey. This traveller approaches through his first chancres the constitutional syphilis. This attained, he goes beyond it, through other chancres which lead him to *syphilization.* He ought not, then, in order to shake off constitutional syphilis, to stop in the middle of his journey.

Every body, before having been *syphilized*, is susceptible to constitutional syphilis, but it is avoided by the most part of those who have chancres, inasmuch as they do not get as far as this, or because they go beyond it. We can, without danger, give constitutional syphilis to whoever has not had it, as we can preserve every one from it.

We fully understand, by what I have just said, that it is not possible to arrive at *syphilization*, without passing through constitutional syphilis. The essential point is to pass through it sufficiently quickly by means of rapid inoculations, in order that it may not have the time to injure our organs. The indurated chancre, then, is nothing else than the symptoms of an arrest in this period, truly inevitable, but which we can render as short as we desire. Consequently we say, by the leave of MM. Dubois and Ricord, "*He who wishes to have syphilis can have it.*" But we add, *non bis in idem.* There is perhaps an exception for those whose parents have had syphilis, and who, from this cause, may be hereditarily refractory to it. A certain degree of syphilization in the parents would be, by the strongest reason, a source of immunity to the children.

Thus am I compelled by facts and reasoning to admit that there is only one virus which produces, according to its particular condition or according to the condition in which the orgasm is found, sometimes the simple chancre and sometimes the indurated. If M. Ricord, as he gives us reason to suspect, ceased to hold high and firm the *banner* which Hunter has placed in his hands, and upon which is inscribed "unity of virus," I should seize it boldly by the staff, so convinced am I that in its folds is found the truth. Yes, there is only one syphilitic virus, and this unique virus is, however, not protean. But it acts differently according as the organism is influenced by such or such a reaction, or according as this virus is itself at a different degree of concentration. I fear that it may be overlooked, as the ancient chemist did a simple body in its various combinations.

Be no longer astonished 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, that there exists more than one virulent cause. One sole virus in different forms, and on organs differently modified by it, easily explains these apparent contradictions.

There is no longer any need of admitting a particular virus in order to explain the phagedenic condition. In order to account for a notable diminution of syphilism, a diminution under the influence of which this phagedemic condition is found, it is sufficient to assume the intervention of the scorbutic, herpetic or cancerous influences, the abuse of alcoholic liquors or of mercury, or finally an inflammation, or some such cause more or less well understood.

Theory is here in accordance with practice, to indicate the means of combating these *anti-siphilizing* tendencies, or to teach us to leave them to be dissipated by time. Let me not be misunderstood here, for in spite of the astonishment of M. Ricord, we have not the phagedenic condition to fear, when we intentionally *syphilize*, and when we know how to manage the virus. We understand now that syphilization is acquainted with, and that it explains those chancres which we oppose to it and which surpass in force those which have preceded them. Cannot each one recognize the influence of the modifications which the organism has undergone during the interval between these chancres, or the intervention of a virus less attenuated in its force than that which had previously acted?

Is it possible to state precisely the number of chancres necessary for *syphilization?* No, because it would be necessary to take into consideration too many conjectures for the solution of this problem. The number of chancres ought doubtless to vary according to their seat, their size, their duration, and moreover their mode of succession, according to the state of health or of the anterior syphilitic contamination of individuals; according to the idiosyncrasy, or, better, the absolute syphilization of these; according to the intervention of mercury, of alcoholic liquors, of various organic excitants, &c.

Thus, for example:

1st.—Successive chancres syphilize more, their number being equal, than simultaneous chancres. But to obtain complete *syphilization*, solely by successive chancres, would occupy too much time. This is why I advise that the inoculations should be brought together and multiplied towards the end, inasmuch as we run no farther risks of inflammation. For we may say, in parodying an old adage, *Il n'y a que les premiers chancres qui coûtent.*

2d.—When an individual has constitutional syphilis, it requires fewer chancres in order to *syphilize* him, other things being equal, than to *syphilize* those who have not. But let us be careful how we forget that the constitutional syphilis is a cause of ruin to the organs, or in other terms, that the *syphilitie diathesis* may engender a *syphilitic* *cachexy.* Now this cachexy may be, in its turn, a source of the phagedenic condition, that is to say, of extreme diminution of *syphilism,* especially when there has been in the treatment a prolonged or a recent intervention of mercury.

3d.—Mercury favors the progress of the chancre. It is then desirable that the persons whom we *syphilize* should be exempted from the influence of this agent. But as its action is transitory, while syphilization, even incomplete, is persistent, we can resume inoculations after an interruption which the presence of mercury in the economy has occasioned.

4th. — Alcoholic liquors, fatigue and excesses of all kinds, internal inflammations, vices, the impoverishment of the blood, &c., are exciting causes in producing the phagedenic condition, or the bubo. Is there any need of insisting upon the importance of removing or suffering these influences to pass away?

In the midst of so many causes, which may act together or separately, we are far less able to fix the number of chancres necessary for *syphilization,* than we are to say absolutely, for example, how much opium was necessary to put one to sleep, or how much wine to intoxicate one. But we may, without fear of being deceived, diminish, by three quarters, the numbers too liberally advanced by M. Ricord, and which M. Sperino does not explicitly question in his treatise. And why leave in obscurity such phrases in the treatise as these :—"*In 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. Sperino might in other respects be singularly reduced in making the inoculations one by one, excepting towards the end, where this discretion would be no longer necessary.

Dispense also with my telling you precisely how many years this immunity ought to endure. How long a time does vaccination or variola preserve us from the variola? We do not know, as regards either of these two preservatives, which we have however for a long time studied! How could we be better instructed as regards syphilis? But I am sure of being within bounds, in stating the time of this preservation to include the entire period of youth. I draw this conviction from different sources, the principal of which are experiments already old, and observations which I possess. What would prevent syphilitic re-vaccinations, supposing that they might become necessary? These re-vaccinations would be reduced to a very few inoculations, since they would have no other end than that of prolonging an immunity anteriorly acquired, and which would not have been entirely exhausted.

I do not propose to *syphilize* those who are forever free from contagion. It would be folly to wish to cause a building to be insured against fire, which could not be burned. Let us apply, on the contrary, this method to those who are much exposed to syphilis, and especially to those who are attacked by it in different degrees. The disease itself is the commencement of preservation and of cure. Our vaccination has this which is valuable, and I would say marvellous in it, that it produces its benefits *before, during and after*.

Reduce the number of chancres given by M. Sperino, in commencing by making only one inoculation each time, at an interval of from eight to ten days. But towards the end, when you would produce only chancres without strength, make several inoculations at two or three days of interval, and even oftener. The essential point, then, is to proceed quickly. And afterwards do not be astonished if you do not see induration. It has not the time to be produced, because you glide over the constitutional syphilis, as it were, of which the induration is only the indication, and one might say the first sign.

For *syphilizators*, the induration is not the cause, it is but the effect; and should you destroy by the iron or fire this witness of the general contamination, you would not change this last in any respect. When we syphilize a person very rapidly, we do not see the indurated chancre, although we certainly make him pass through the constitutional syphilis.

I will go farther; you have been able in some cases to destroy chancres before the induration has manifested itself, and when even the constitutional syphilis has already existed, and perhaps some cases of this kind have been brought against your theory upon the indurated chancre, which is otherwise valuable. Thus *syphilization* itself explains facts which overthrow your doctrines.

A few words, now, upon the *syphilized* subjects of M. Puche. They are not under my care, although I see them almost every day. I should not have spoken of them, if M. Ricord had not first brought them up. This is an initiative step which I take kindly from him, because it furnishes me the occasion to bring out two facts entirely confirmatory of my assertions. In fact, in one of those *syphilized*, the *syphilization* has proceeded without obstacle, and in the other, the result would have been similar, if he had not been submitted to a mercurial treatment at the same time with the inoculation; and for proof, I will say that the suspension of this treatment has cut short the shackles which the *syphilization* met with.

Our inoculations are not only preventive, they are also, above all, curative. This results from the fact, that we do not arrive at *syphilization* without passing more or less rapidly through constitutional syphilis. Now when the organism has not suffered for too long a time from the action of the virus, we are still in time to allow this organism to enjoy the benefits of *syphilization*.

I should fear, Mr. Editor, to abuse your patience if I insisted upon the conditions of the seat of inoculated chancres; but you will understand how chancres placed upon the arm, or abdomen for example, must occasion less pain and present less inconvenience than chancres placed upon the penis.

M. Ricord demands instantly a *syphilized subject in close combat*. His wishes shall be more than gratified, for the *syphilized* person which I shall oppose to him will be also a syphilizator. Let M. Ricord, then, put himself on guard. He will see if he has to do with convictions which are growing feeble.

And let him well understand that it is not simply a question of a revision of the syphilitic constitution, as he thinks, but of a radical revolution. Yours, &c. AUZIAS TURENNE.

August 22, 1851.

In the strange letter which M. Auzias communicates to the Union Medicale, and which is rather to my address than to yours, he accuses me of having opposed *syphilization*, and of having voluntarily given it *a false air*. If syphilization has not to start with, the appearance of a truth, it certainly is not my fault, but surely that of M. Turenne. I leave those as judges who love science. Voltaire said one day to the sister of the king of Prussia :

Souvent un air de vérité
Se mêle au plus grossier mensonge.

Well, I shall say to M. Turenne, whose good faith I have never suspected, if all that he advances in his letter is the expression of truth, we must invert the two verses of Voltaire.

Great discoveries, it is said, have often been taken for folly. Salomon de Caus was shut up at Bicêtre. All which leaves the track of well known truths, all which we cannot refer to established laws, is frequently taken for extravagance. We are sometimes doubtless wrong, and history records great and much to be regretted injuries. But does this mean, that the more an idea is absurd, eccentric, and at first sight irrational, we must so much the more accept it without examination, without criticism, and so much the more quickly as it is contrary to experience and to acknowledged facts which it has not yet explained or destroyed? Is it necessary that we should follow the idea blindly, because it appears *very dangerous*, without knowing into what abyss it may conduct us? No, and at the risk of being deceived, without condemning to the stake or to the prison those whom we believe either heretics or fools, we must set up a wise reasoning, not for the sake of preventing the progress, nor even for the sake of applauding all revolutions, which often overthrow more than they build up.

One strange thing, my friend, is that, while for more than twenty years I have labored in order to establish the points of doctrine which are the source, the generating idea of what M. Turenne does to-day, the men who have had so much *black* ink at the end of their pen against my experimental researches, and a point so sharp against the *unicity* of the syphilitic diathesis, a truth to-day incontestable, have no more reflections to make to the following proposition of M. Turenne, which comprises all the others :

If you suffer from syphilis, it is because you have not taken enough of the virus.

In fact, if one consults M. Auzias respecting a chancre, he says to you, return to the source, and return again until you can take no more. If you have no more force nor courage, he gives you chancres until you have enough ; how much, he does not know, because that he has an infinity of conditions, of which he is ignorant, and in virtue of which, *syphilism*, or the aptitude to contract chancres, may be more difficult to satisfy ; it is possible that it may require ten, thirty, forty, fifty, sixty, or more, but take courage, and you arrive without much inconvenience at the desired end ; for these chancres will be placed upon some regions with which one has little to do ; upon the abdomen, for example, in prostitutes, or upon the arms of those who do not make use of them.

But in multiplying thus during one or two months and longer the sources of the infection, do not fear that they infect you, that they infiltrate you with virus, that they impregnate you with it ; this would not be the business of syphilizators, they do not wish that you could believe that they place syphilis in the blood. It is sufficient that you know that you are *syphilized*, that you have undergone a general modification which has destroyed your *syphilism* forever, without the virus penetrating you, without its being mixed with your humors. M. Auzias is sure of this, for he has followed it in its peregrinations, and you are to judge of the fact.

Suppose that you all, without exception, who, like all the animals of creation, enjoy syphilis, that is to say, the great prerogative of being able to contract syphilis for the purpose of afterwards protecting yourself from it, represent a mountain with two sides, one side to the east, and the other to the west, and a chancre wishes to climb the first side of the mountain of . . . Venus. If it is alone, it rests at the foot of the mountain, where it may die without descendants; if, on the contrary, other travellers of its *kind* come to its aid in its route, to give it a lift, it may arrive at the summit; but if it is abandoned there, and we do not aid its descent down the opposite declivity, as certain monkeys do, from tail to tail, according to the spiritual fable of M. Viennet, it is forcibly arrested, it becomes indurated, and sets fire to the syphilitic mountain, which then vomits forth the lava under the different forms of constitutional syphilis, with which you are familiar. But if its progress is not impeded, or if it is taken up again after a stop, and even after an *eruption*, the traveller, fatigued and overcome in the second half of the route, carries away with him the evil which he has done, and dies in the valley of Jehoshaphat, to await the last judgment of—— experiment.

However, my friend, in this ascending journey, whatever M. Turenne may say, who does not maintain that the virus penetrates the economy, that it infiltrates itself by the way of absorption, thus to infect it after the manner of poisons, it may leave its footprints in the soil, be caught first in the neighboring lymphatic glands, then hollow then hollow out a furrow more profound, so as to become indurated, if it is arrested, and produce general symptoms. Does it follow another route, when it does not become indurated? No, since in order to dislodge a primary indurated chancre, we make the syphilizing chancres follow the same route, and necessarily so; for otherwise there would not be the chance of meeting the first and of overthrowing it.

Now, how many chancres are necessary in order to arrive at the summit of the mountain and to overturn the constitution? How many are necessary afterwards in order to establish order in the plain? I have told you M. Auzias knows nothing about this, neither does he trouble himself much about it; he is not so far advanced as the man who was asked how many rats' tails would be required to reach from the earth to the moon, and who answered, only one, provided it was sufficiently long. Well, daily observation will show M. Auzias that many individuals, a very great number even, have only one chancre; that all the solitary chancres do not become indurated; that the syphilitic diathesis is not in the inverse relation with the number of primary symptoms, and that all the individuals who have only one chancre have not from this alone constitutional syphilis. Far from it; nothing is more common than to see individuals with the symptoms of general syphilis, and who have had at different times, more or less approaching each other, sometimes in the course of one or two months, numerous *successive* chancres,—10, 15, 20 and more,—provided that among these, there is one which has become indurated, or, if you prefer, one which infects, and which then has, as you know, some particular characters, and gives to the economy a certain disposition, the analogy of which we find in variola, and which prevents the production of another similar symptom giving place to the same consequences.

If with a certain number of chancres we ought always to have constitutional syphilis; if with a *determined* number we ought to enjoy immunity from it, all would be said; but observation has already answered this. When with a single non-indurated chancre you had no constitutional symptoms, you could say, there is already syphilization, as there is vaccinia with one single puncture, a single vaccinal pustule. But this does not thus happen, as we have seen, since we may inoculate again, and the ulterior chancres may be followed by empoisonment, by the syphilitic diathesis.

In order to arrive at syphilization, some weeks, some months are necessary, while we know beyond doubt that the chancre infects and becomes indurated at the end of a few days only; and that less time is necessary to arrive at the secondary manifestations, than is required to prevent them.

Chancres, says M. Turenne, are cured the more quickly in proportion as we multiply them, and as there is syphilization. This proposition is not maintainable; we must often invert it, and the inoculators of to-day, who have combated those of former times, are well convinced of this. In some cases the chancres of inoculation have been much more serious than those from which they have been taken. It is not rare to see a single chancre healed without special treatment, in three, four, five or six weeks; if art intervenes, especially if we have recourse to the mercurial treatment against the indurated chancre, we succeed still more quickly. Does syphilization make greater strides?

The diminution of intensity in the successive inoculations, in some of those of my colleague M. Puche, in which the pus for inoculation has always been taken from the patient himself, may be attributed to a progressive weakening of the virulence up to the moment when the chancre arrived at the period of reparation, and can no longer furnish inoculable pus, as I have already demonstrated, and taught for twenty years. Here the grain is bad, or fails; later, it is the soil which will give out.

What is certain, what all observers have established, is, that there is a moment, sooner or later, when all the chancres become cicatrized, and this almost at the same time, no matter whether there is only one or a great number, the last as quickly as the first, and this often without our being able to refer the cure to the remedies employed, and sometimes this happens even in spite of the remedies. What is, then, the mechanism of this cure? This cannot be the syphilization in all cases, and according to yourself, since this takes place with one or many chancres; and after the cure, it is not true that all individuals are refractory to new inoculations. What we observe here for the primary symptoms, we observe also often for the secondary symptoms, which after having lasted a certain time may disappear alone and simultaneously, *without the necessity of new contagions*, and without syphilization being able to explain it. What happens here is observed in many other diseases; it is an effort of nature to disembarrass itself of what is not assimilable, of all which is foreign to her; it is a work of elimination, of reparation, of repulsion more or less general, and especially in the homogeneous tissues, having the power, at a given moment, to prevent new effects from being produced, as it proceeds to destroy those which already exist.

To this *vis medicatrix*, art often comes in aid, not in augmenting homœopathically at a high dose, the morbid principle which it should combat, but on the contrary, in removing and in striving to destroy it. It is thus that in certain forms of syphilis we have recourse to powerful auxiliaries, to medications almost specific, and to mercury particularly, which, like all the great powers of this lower world, has been by turns enthroned and proscribed.

For instance, after the restoration, in which the Academy has been well pleased to recognize my participation, and which succeeded to the physiological revolution in which the existence of the virus had been denied, and consequently, the efficacy of mercury, observe that the power of this medicament is anew placed in question by the revolutionary *syphilizators*, who like the physiologists their predecessors, accuse it even of the evil which it pretends to cure. Is it possible to maintain still a similar language in 1851, in presence of the innumerable patients in whom we see syphilis develope itself without their having ever taken an atom of this medicine, and stop and disappear immediately upon its being properly administered?

It is true, that this therapeutic agent is not equally efficacious against all the forms of syphilis; that there are some, even, that it aggravates, which I teach with many other writers upon syphilis, and the form which it injures the most frequently, is the non-indurated chancre, the only one which M. Auzias appears to me to have inoculated up to this time, and of which consequently it ought often to prevent the cure, not in augmenting the syphilism, but in altering the constitution, and in such a way as to favor the progress of every ulceration, of the chancre as well as the scrofulous or scorbutic ulcer, and in producing even ulcerations *sui generis*.

It is no longer to mercury, according to M. Auzias, that we must have recourse to cure syphilis, but to syphilis 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 the sacred writings, which spoke of this before M. Alex. Dumas.

In fact, Percy, cited by Petit Radel, thinks that we may apply to the treatment of syphilis the doctrines of Bordeu, and that we ought to cure chronic and rebellious cases of syphilis in making them re-pass through the acute stage by renewing them, as some persons still advise those who have chronic discharges. It is thus that Percy inoculated his patient whom the inoculations did not cure; but whom a mercurial treatment more methodical and better managed, disembarrassed of an evil, which ought to have increased, according to M. Auzias, the mercury here neutralizing the benefits of syphilization.

M. Auzias reproaches me for not having said all, in citing M. Sperino. As to my approximation to the number of the inoculations which he ought to make, I still maintain that I am correct. As to the phagedenic chancres which new inoculations have not prevented from curing, there is nothing astonishing, and which does not happen every day.

I have said, and I still maintain, that "it is not every one who wishes it, that can have syphilis."

Finally, I am accused of abandoning the banner of Hunter, upon which is inscribed, among other things, the unity of the virus. I have already made to you my profession of faith, and shown the colors of my banner; I shall not return to it. I will only say to you, that if what I have taught in my lectures for a great number of years is to be verified, viz., that syphilis so analogous to variola, especially since I have shown the unicity of the virus, ought also to have its vaccine; and that if the assertions of M. Auzias were demonstrated, it would become probable, that the virus furnished by the non-indurated chancre would be different, or a modification of that which produces the infecting chancre which becomes indurated, and that the first would be to the syphilis, what vaccinia is to variola, influencing the economy by a local effect without general manifestations, and preventing the other from afterwards acting either locally or generally.

As you may see, all which precedes is serious, very serious, and merits the greatest attention. To encourage the young to multiply the symptoms of primary syphilis, is to encourage them to return to the source from which they have taken it. To say to those who have constitutional syphilis: "go on, rest quiet, let the secondary and tertiary manifestations come, be careful of employing the remedies reputed efficacious; when you wish, you shall be cured by giving you new chancres;" is something too serious for those who are placed at the advanced posts, and who have a certain responsibility, not to demand facts in place of theories, which up to this time nothing justifies, but which everything, on the contrary, appears to condemn.

1st.—I demand, then, of M. Auzias, that he show us some *syphilized* persons; they are all ready, he says; so much the better, I shall then be convinced that one can become refractory to inoculation.

2d.—I demand the limit of the immunity to which M. Auzias does not appear to attach a great importance, but which the syphilized ought much to count upon. M. Auzias ought to know something of this limit, for it is not observations of the day before that should be presented under similar circumstances. I demand, then, the oldest cases, and this with good reason.

3d.—I demand of M. Auzias that he produce at will indurated chancre upon the first individuals who come, and that he arrest some of them at will by syphilization; that he allow others of them to progress as far as the secondary symptoms; that he shall afterwards destroy these by his inoculations.

4th.—That he present to us, both before and after these inoculations, some patients affected with constitutional syphilis at its different periods, and cured by syphilizing inoculations, and I will accept the revolution in which I shall have taken the first part.

Up to the present moment, my friend, your journal, so wise, so critical, ought not to accept works like those of M. Auzias, except with extreme reserve, without guarantee, I was going to say without encouragement, for in bringing to mind the evils which happened to the physiological school, the adepts of which were as convinced and as honest as our laborious confrere, M. Auzias, we tremble before the terrible consequences which clinical observation, acquired science and reason, ought to make us dread.

Yours, &c. RICORD.

THIRTY-FOURTH LETTER.

MY DEAR FRIEND,—It is a very long time since I wrote you my first letter; it is also a long time since you received my last, and however agreeable this correspondence may be for me, it may no longer please you, as happens with everything which is continued too long. It is not, however, entirely my fault, but that of the times and of circumstances; for I remember one of your aphorisms:—*pleasure is only pleasure when it is rare and short.* If my letters have caused you any satisfaction, it is because they have possessed at least one of the conditions of your programme.

The hope given out by syphilizators, of seeing syphilis, some day, disappear from the table of pathology, and consequently the necessity of omitting in the treatises upon therapeutics the useless pages which treat upon the anti-syphilitic remedies, have arrested my attention for a moment. Why continue the history of a disease which ought no longer to exist, and why speak of a treatment, which would thus have no longer any application? I was about then to say to you two farewell words, and to stop there, when a visit to the hospital convinced me that whatever might be in reserve for syphilization, the present had still sufficient that was deplorable upon this point, to induce us not to remove anything from our classical books, and to rest persuaded that syphilis, alas! was neither dead nor dying.

Finally, in waiting until the idea of syphilization, born of my school, which has also prophesied a vaccine, should be demonstrated by syphilizators; until we arrive at proving that syphilis up to this day, has been calumniated by all the writers on the subject, both ancient and modern; until it shall be recognized, that in place of being one of the greatest plagues which has ever struck humanity, syphilis is, on the contrary, a boon from Heaven, let us once more occupy ourselves a little with what remains to us of this plague, or of this boon.

As regards the prophylaxis, I said to you in my letter before the last, that it was impossible to believe in the pus or the blood of tertiary symptoms, as a preservative inoculation, and that syphilization as an experiment ought to be diligently studied before being taken for serious.

I will say to you upon this subject, that at last, a *courageous* pupil in medicine has been presented at the clinique of the Hospital du Midi, who has submitted himself to experiments, and who, within three months, has suffered to be made and has made himself more than sixty inoculations, the traces or the cicatraces of which we see, and one of which still, upon the twenty-first day, presented the characters of the ecthymatous chancre. I ought to give you an account of the result of the experiments, which were to be continued at my clinique, but since the first publication of this letter in l'Union, this pretended syphilized individual has no longer presented himself at my examination.*

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* But instead, we have had the observations of Dr. L., who presented himself to the *Societé de Chirurgie*, and which throw a terrible responsibility upon the heads of those who extol the doctrines which they themselves ought to be compelled to undergo. Here are the words in which l'Union Medicale published what passed at the meeting of that learned Society, Nov. 12, 1851.

"We receive from M. Musset the following communication, and transcribe it literally."

"Dr. L. was presented to the Surgical Society by M. Musset, interne of M. Ricord, in order to submit to the observation of this learned Society the results of experiments undertaken for the purpose of testing the ideas given out upon syphilization.

In waiting until Dr. L. himself gives *in extenso* the account of his own observations, not yet completed, here are the principal results to which he has already arrived. Dr. L. has never had either chancres or blennorrhagia.

In the months of December, 1850, and January, 1851, he inoculated himself ten times upon the penis, each time after the interval of a week, thus producing 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.

July 2d.—He inoculates himself again upon the left arm, and the consequence of this is an indurated chancre.

Three months after, that is to say, Oct. 1st, an exanthematous, and soon a papular syphilitic eruption declared itself, accompanied by an

This would have been one and the first observation, the value of which you may conjecture; we do not appear to have others, at which I am not astonished, inasmuch

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enlargement of the posterior cervical glands. A few days afterwards, some mucous tubercles appeared upon the tonsils. Dr. L. does not submit to any treatment.

Oct. 17th.—An inoculation is practised upon the left arm by M. Auzias, in presence of M. Ricord, with pus taken from a chancre dating twenty days, existing upon a patient who had been himself inoculated with pus taken from a pretended syphilized person who had arrived at about his sixtieth chancre.

Oct. 24th.—M. Ricord practises two inoculations, the one upon the left arm, the other upon the mucous membrane of the prepuce, with pus from a phagedenic non-serpiginous chancre, existing upon a patient in his wards.

Oct. 25th.—Dr. L. inoculates himself upon the same arm and upon the penis with pus taken from the first chancre.

Oct. 28th.—Two inoculations are made upon the left arm, one with the pus of the first chancre, the other with that of the fourth.

Oct. 29th.—Two inoculations are made with the pus of the fourth chancre.

Oct. 30th.—Two inoculations are made upon the arm with the pus of the first and second chancres.

The number of the inoculations amounts thus to eleven.

1st.—Although ten inoculations have been made, this has not prevented the eleventh from becoming indurated, and from being regularly followed by constitutional syphilis.

2d.—The new successive inoculations which have been made in view of syphilization, have all succeeded.

3d.—The chancres have not been in the least degree in proportion to the inoculations made. Thus the diameter of the successive chancres have been indifferently greater or smaller than those of the chancres which they have preceded or followed.

4th.—The largest number of chancres inoculated have taken on the phagedenic form, as is often seen in individuals who, having constitutional syphilis, contract new chancres.

as such subjects must necessarily be very rare. In fact we must submit ourselves to similar experiments, in order

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5th.—It is to be remarked that the severest forms come from the syphilized individual of M. Auzias, who has arrived at his sixtieth chancre.

6th.—The phagedenic, non-serpiginous condition has not depended upon the source from which the pus had been taken, for the greatest number of chancres which have been produced by pus coming from the syphilized person has taken indifferently the phagedenic form, while that among the three chancres produced by pus furnished by a patient in the wards of M. Ricord, affected with a phagedenic non-serpiginous chancre, one alone has taken the phagedenic form.

7th.—The phagedenic condition of the first chancres has not been mitigated by the chancres which have followed and which have become phagedenic in their turn.

8th.—The phagedenic condition has then appeared to depend upon the general state of the patient, influenced by the seat of the chancre; for while the greatest number of chancres inoculated upon the arm have taken this form, the chancres inoculated upon the penis the same day with the same pus have remained very limited, and have proceeded quickly on to cicatrization.

9th.—The successive inoculations made for the purpose of syphilization, and which have taken on a progress so grave, not only have not favorably influenced the symptoms of constitutional syphilis, but on the contrary these symptoms appear to have acquired a new intensity in proportion as the chancres of inoculation tended to the phagedenic condition.

10th.—It must be remarked that while all the inoculations made with the pus of primary ulcers have been followed by positive results, inoculations of secondary symptoms belonging to the gravest forms, and in all their intensity, have remained without effect.

11th.—The observations of the courageous and learned Dr. L., who will publish them by-and-by, with all their details, ought already to be of a two-fold instruction to those, who extolling the doctrines that lead to results which we have just seen, have not the courage to experiment upon themselves."

to have more confidence in the doctrine than he who teaches it, and who *does not set us the example.* It has been said that what deterred some from being inoculated, or of making it known that they had undergone this, was the fear that this might injure them in the world, if they wished to contract marriage. This is perhaps true, and I do not contest the justice of this apprehension; but what astonishes me is, that these benefactors of humanity should suffer from fears so vulgar. The school of the prudent Fontenelle is not dead, my friend, and there are yet some who do not open their hands which are full of truths.

However it may be, and in order to return to my own grounds, in the present state of science, the best means of preventing constitutional symptoms consists in destroying the primary symptoms as soon as possible, as I have already told you in speaking of the chancre. But when we arrive too late to be able to count upon the abortive method, must we in all cases hasten to have recourse to a general specific treatment? It is a very long time since I responded in the negative to this important question. The infecting chancre is the rarest symptom. Under other circumstances, whatever may be the number, the duration or the repetition of the primary symptom, the constitutional infection does not take place, and a treatment becomes then not only useless, but it may be sometimes hurtful.

Some specialists, convinced like myself that the greater part of the primary symptoms are thoroughly and speedily cured spontaneously, by attention to hygiene or to some simple medicaments, are of opinion that we should wait, before having recourse to a specific energetic treatment, until we have some proofs of the general poisoning, and that the treatment should be commenced only against the secondary symptoms. Others, who recognize the necessity of this treatment as soon as the chancre presents the characters upon which I have insisted, do not advise its administration until general symptoms have manifested themselves, not only in order to demonstrate its actual necessity, but above all in order to make patients understand that the treatment ought to be a long time continued.

As for myself, when I have an infecting chancre to treat, I have recourse, and that too as soon as possible, to the specific treatment, that is to say, to the mercurial.

The mercurial treatment may prevent the constitutional symptoms, or simply retard them during a time, which is difficult to limit, between months and years. There are no practitioners who have not seen patients who after having been treated, have enjoyed all the benefits of excellent health during 10, 15, 20, 30 years, and who at last present, either for the first time, or as a relapse, some symptoms characteristic of syphilis. In presence of facts of this kind, unfortunately so numerous, how can we do otherwise than to admit the persistence of the diathesis compatible with apparent good health; how can we conclude in all cases upon an absolute destruction of the acquired syphilitic disposition, as some speculators so thoughtlessly do?

What would give certainty that we could destroy the diathesis by a good medication, which, in fact, ought not to be impossible, would be some well-authenticated, well-detailed and well-analyzed observations of individuals, having had indurated chancres twice or oftener, and who have presented each time the series of constitutional symptoms in the natural order which we at the present day recognize. Now these cases, which are not perhaps impossible, but which I have not met with, up to this day, are for strict observers yet to be found, in spite of what some persons may say who are little versed in the study of syphilis.

Therapeutists who respect themselves, may say that they prevent or cause constitutional symptoms to disappear in a great number of cases, without its ever being permitted them to affirm that these symptoms will no longer be possible. There is neither a form, a daily nor absolute dose of a remedy which always gives immunity, whatever may be the accessory care.

It is here especially necessary that the profession, I was going to say, the trade, should respect science. It must learn to say, that we have only as regards this matter, some probable calculations; for those of Hunter, which have a sort of mathematical pretension, are far from being true.

To adopt a treatment only until the disappearance of the symptoms, is the method which allows the most chance for the symptoms to return. To insist upon the continuance of the treatment, after these symptoms have disappeared for as long a time as it has required to overcome them, does not conduct to more satisfactory results. For it is often too much or not sufficient. Finally, salivation as a measure of treatment presents still more inconveniences, and is less of a guarantee than the other methods.

Six months of treatment at a daily dose which influences the symptoms that we have to combat, and which indicates by its well-known physiological effects, after that they have been destroyed, that the medicament still acts, constitutes, to-day, the rational treatment, with which many practitioners are content, and which appears to afford the cures the longest sustained.

But whether we administer a treatment against the primary symptoms alone, or whether we have recourse to it in order to combat the secondary symptoms, as I have said, the treatment may derange the time of the appearance and the order in the succession of the symptoms. More powerful against the secondary than against the tertiary symptoms, mercury sometimes prevents the first from being manifested, in permitting the latter to show themselves; it is thus that after the chancre treated by mercury, a first constitutional manifestation may consist in an exostosis, and thus form for some who know how to count only on their fingers, a secondary symptom out of a tertiary one, as if it had only this character which decided its nature; in the same manner and under the same influences of treatment the secondary symptoms may manifest themselves after the tertiary, and thus afford material food for criticism, the strength of which, you understand.

But all this, you know, my friend, is far from being disorder, it is only the effect of art, as I have told you, and demonstrates its power; where the disease is left to itself, this never happens.

I will still add, that my colleague, M. Cullerier, believes that this order is so inevitable, that treatment cannot interrupt it. Thus for him, the symptoms ranged in the class of tertiary symptoms are always preceded by secondary ones; but observation does not permit me to accept this, since it does not conform to what the influence of treatment may produce.

The manner in which I have understood the evolution of syphilis, the methodical classification of the symptoms which I have traced, has permitted me to have recourse to a rational treatment, and to administer mercury in those cases only where it is useful. At one time this remedy was too much rejected by some, and too prodigally used by others. Thus, this constitutes the best treatment which the Academy of Sciences has been pleased to recompense.

Thus, I believe that I can say that the iodide of potassium, at first given in the general treatment of syphilis, and which, for this reason, gave therapeutical results so uncertain, sometimes so contrary, or at least so little satisfactory, has been by my clinical studies more especially reserved for the series of symptoms which I have called tertiary, upon which it exerts a very powerful action. We can at the present day sum up in the following manner the therapeutics of syphilis:—

1st.—Abortive treatment applied to the chancre as soon as possible.

2d.—Mercurial treatment reserved for the indurated chancre and for the secondary symptoms.

3d.—Iodide of potassium in tertiary symptoms.

4th.—Mixed treatment by mercury and the iodide of potassium against the late secondary symptoms, when tertiary symptoms exist at the same time.

Permit me here, my friend, to close this series of my letters; permit me also, in thanking you for the kind reception which you have given them, to believe that every time that an opportunity shall present itself, you will be pleased to again afford me the hospitality of your Journal.

Yours, &c. RICORD.

ANALYSIS.

ANALYSIS OF M. RICORD'S LETTERS.

M. Ricord commences his letters by complaining, and very justly too, that in spite of the teachings of twenty years, he has not yet succeeded in making his doctrines upon the important subject of venereal disease, fully known and understood. However, he is not yet discouraged, nor is he indignant with his opponents, but is still ready to explain and to teach. He pays due honor to the great masters who have gone before, and also to his cotemporaries. He wishes it to be distinctly understood that he is not of the opinion of so many, who consider ancient and modern syphilography as unworthy of attention, but, on the contrary, considers it as a branch of pathology replete with useful and interesting subjects for research and study.

At the time when M. Ricord entered the Hospital du Midi, everything pertaining to syphilis was doubted—its cause, its effects, its therapeutics—confusion reigned supreme. Consequently, the first thing which he proposed to himself, was to ascertain whether there really was a special cause, a virus—or whether all venereal diseases sprung from a common cause?

Two means of investigating this inquiry were offered to him. The one, was the simple observation of phenomena, such as his predecessors had practised, and which had conducted to such different opinions. It was evident, then, that this mode of investigation would only lead to confusion and uncertainty.

The other was more in accordance with the demands of modern science, and which would seem to conduct to incontestable results ; this was, experiment. Therefore, deciding upon this method, he imposed upon himself the following conditions, viz., to take the syphilitic virus from a known source—to place it upon some region easy to observe, and then to note its effects.

But experiment, he says, had already been tried, and had conducted to different conclusions. He did not know at that time why this was the case, but he has discovered since. Nevertheless, he was not to be discouraged nor led away by the great names of Hunter or of Bell, but was determined to experiment anew, and to take upon himself the responsibility of the results.

Two methods of experimenting were open :—The inoculation of a healthy patient from a diseased one, or inoculations made upon a diseased person himself. The first method was out of the question, as M. Ricord thinks that it is unjustifiable. The second method remained ; but were there any dangers or inconveniences in its practice ? And in case there were not, would it conduct to any conclusive results ?

As to the inconveniences or dangers, we every day see that it is rare that the primary symptoms are isolated ; that they multiply themselves with great rapidity, and that the gravity of the disease bears no relation to the number of these symptoms. This being the case, in such a grave question, it was justifiable to do artificially, what nature constantly does. Moreover, strict clinical observation has proved that the constitutional disease bears no relation to the number of primary sores existing at the same time ; neither does one chancre more, add anything to the chance of infection, if we know how to conduct the experiment. Neither does the extent of the surface ulcerated, have any influence upon the production of secondary symptoms. A very small chancre exposes one to a general infection as much as a large one, and a large one no more than a small one.

As to the seat of the inoculation, clinical observation had shown, in spite of the opinion of Boerhaave and others, that this could have no influence upon the production of consecutive symptoms. They had maintained that venereal sores contracted in any other way than by the genital organs, were always followed by much graver symptoms.

M. Ricord terminates his first letter by drawing the following conclusions from the history of the disease, from clinical observation, and from preceding experimenters: that in thus experimenting, he did not increase the disease; he did not augment the gravity of the disease already existing, and he did not expose the patient any more to the chances of a consecutive infection.

The author adds, that in order to make what he had already said upon experimenting in his first letter, complete, he must call attention to the fact of endeavors having been made to inoculate animals with virus taken from man, either for the purpose of obviating inconveniences which might result from inoculation practised upon man himself, or to resolve the curious problem of the transmission of syphilis to animals. Hunter and Turnbull had made these experiments without success, as also M. Ricord and others; but lately M. Auzias Turenne has thought that he had succeeded, but M. Ricord considers his success as only illusory.

Direct observation and experimenting upon the patient himself, were the only resources left for the author.

The first thing to be done was to obtain a source upon which he could rely, and by which he could direct his investigations. He could no longer rely upon the histories of their cases as given by the patients themselves. He was not contented with taking a morbid secretion coming from the genital organs of the female, and attributing all venereal diseases to it, no matter what its source might be. Modern science demanded more.

He did not wish to adopt the common conclusion that effects followed from the cause. He says, "who cannot be surprised, that, in a question like that upon venereal disease, where ignorance and fraud are such frequent causes of error, and in a disease which is almost always a proof of immorality, even the most judicious observers are led to believe the stories of patients, and to invoke the morality of their testimony! Is there anything more deceptive than testimony in such a matter, particularly as regards women?"

Babington wished to overthrow the law laid down by Hunter, that when there is neither pus nor puriform secretion, the disease cannot be communicated, so that the infection is impossible before the appearance of a gonorrhœa or after the cicatrization of a chancre. In order to do this, he relates two stories of husbands communicating disease to their wives without being actually diseased themselves. M. Ricord tells him that if he had taken the pains to assure himself of the true state of things, he would have found that the infecting cause was not in the genital organs of these candid husbands.

The morality of the testimony of patients, then, was not to be relied upon, but the cause of the disease must be sought for at its source; that is to say, in the female genital organs, in their most internal as well as external portions.

Before the entrance of M. Ricord into the Hospital du Midi, physicians, he says, were content with the most superficial examination of women, with merely separating the external organs. If there was no lesion to be seen, every secretion coming from a more internal part was considered as blennorrhagia. "My predecessors appear to have placed the pillars of Hercules of chancre at the entrance of the vagina."

This would not satisfy him, but he must bring the speculum, which had lately been exhumed by M. Recamier, to his aid in the diagnosis of syphilitic diseases, a purpose to which it had not yet been applied. This enabled him to examine all portions venereally affected, and to state precisely the state of the tissues.

Thus being prepared, he commenced the study of blennorrhagia.

Does blennorrhagia have a specific cause? Hunter had taught that inoculation with the pus of a chancre produced chancre. If, then, blennorrhagia has a specific cause, our author said to himself, the muco-pus, if inoculated, will produce similar phenomena. But in order to ascertain this satisfactorily, it was necessary to take the muco-pus coming from a perfectly simple blennorrhagia, and from tissues perfectly free from ulceration, and here, the speculum was of great service; in fact, without it, no such experiments could have been made. The result of numerous experiments induced M. Ricord to lay down the following proposition:

"*Whenever muco-pus is taken from a non-ulcerated mucous surface, the results of the inoculation are negative.*"

All who have made similar experiments have arrived at the same conclusion.

Some writers have thought, with Hunter, that blennorrhagia was a form of syphilis peculiar to the mucous membrane. But the experiments just cited refute this opinion. We shall see, by-and-by, that the virulent pus of a chancre, placed upon a mucous surface, will produce upon it a true chancre.

He then draws this conclusion: "Blennorrhagia, inoculation with the muco-pus of which, gives no result, does not recognize the syphilitic virus as its cause."

Does blennorrhagia, he asks, absolutely different from syphilis in its cause, in its form, consequences, &c., depend upon a special virus? Nothing is more likely to produce a blennorrhagia than the muco-pus furnished by certain inflamed mucous membranes. But if we examine very rigidly the causes which have produced the best marked cases of blennorrhagia, we shall find that the virus is wanting. Nothing is more common, in fact, than to find women who have communicated blennorrhagia the most intense, in whom upon examination, we find nothing but a uterine catarrh scarcely purulent. Very often the catamenia have been the sole cause—and sometimes we find as exciting causes, errors in diet, fatigue, excessive coition, the use of certain drinks, as beer; certain articles of food, as asparagus. Hence arises the opinion of patients, very often expressed, and very justly too, that they contracted their blennorrhagia from a perfectly healthy woman.

Upon this point, M. Ricord remarks that he is aware of all the cause for error, and that he is fully on his guard, but that he feels himself justified in sustaining this proposition:

Women frequently communicate blennorrhagia without being themselves affected.

Women, he says, communicate twenty blennorrhagias where they contract one; and this is explained by the fact that the female genital organs are subject to many discharges which are not venereal, and which still are the source of a blennorrhagia for the male. A woman may cohabit with her husband without communicating any disease; but should another have intercourse with her, he may become affected with blennorrhagia. This is explained by the fact of the former having become habituated or acclimated, as it were, to the secretion of the woman.

When we study blennorrhagia without preconceived ideas, we are forced to confess that it is often produced by most of the causes which bring about inflammation upon other mucous surfaces. Yet, for blennorrhagia, as for every other inflammation, the predisposition is necessary —that great unknown principle, which dominates over all pathology. Without this, the disease would be far more common than at present. An experience of twenty years permits M. Ricord to affirm, that with the exception of those cases where there is chancre in the urethra, it is often perfectly impossible to ascertain the cause of a blennorrhagia.

The most special cause for the disease, is the muco-pus furnished by inflamed genito-urinary mucous surfaces.

This appears to M. Ricord a more rational view of the subject, than to refer blennorrhagia to a sort of demi-virus, imagined by M. Baumès, which opinion, neither observation nor facts support. M. Ricord's opinion is, that blennorrhagia is entirely different from syphilis as regards its causation, and he affirms that when Harrison and Hunter produced blennorrhagia with the pus of a chancre, either it acted after the manner of simple irritants, or else it produced an urethral chancre. And if inoculation as proved that the cause or causes of blennorrhagia, *whatever may be its seat* in the two sexes, differ from the virus which inevitably produces chancre, the consequences of blennorrhagia ought to differ from those of chancre; and yet, many cases of constitutional syphilis are attributed to blennorrhagia.

Next, as to the incubation of blennorrhagia. A great difference of opinion has existed and still exists on this point. The term of incubation has been fixed between some hours and fifty days—and even five months in one case. It is not so with other virulent diseases where incubation is incontestable, and where the period may be accurately fixed, as in variola, in vaccinia, scarlatina, &c. There are no certain limits for blennorrhagia. The author does not deny that there may be a longer or shorter period between the cause and the appearance of the symptoms of blennorrhagia, but he asks if it can be properly called a period of incubation, like that which exists with the variolic or vaccinal pus? He explains this period existing between the cause and the effect, by the particular disposition or susceptibility of the affected tissues. There is no more incubation here, than there is between wetting the feet and the appearance of a coryza. In those cases where blennorrhagia does not appear until a long time after one is exposed to the presumed cause which has produced it, is it not more rational to admit another unknown cause, than that pretended incubation which nothing explains or justifies? Is it not so with almost all inflammations? Can we always go back to the direct cause of a pneumonia or of an arthritis?

Without doubt, sexual intercourse is the most frequent cause for blennorrhagia in the male; but we should fall into strange errors if we referred all cases of blennorrhagia in the male to a virulent cause.

Much has been said upon the specific seat of blennorrhagia. Hunter placed virulent blennorrhagia identical to chancre, in the fossa navicularis; Graff placed the seat of this same in the follicles which surround the female urethra; and so of others.

Our author says that strict observation has proved to him that the most exposed portions of the mucous surfaces are the parts most easily affected. But, at the same time, it must be admitted that the urethra in the two sexes is more often affected than the other portions of the genital organs, as a consequence of sexual intercourse. So that, as a general rule, a woman with an urethral blennorrhagia may be considered as having contracted it from a man affected with that complaint.

But does this fact favor the idea of the existence of a virulent contagion?

No, M. Ricord explains it by the fact, that the pus furnished by the urethra, is the most irritating of all varieties of pus for certain mucous surfaces.

Some contend that blennorrhagia is never seated in the female urethra, while others admit this form alone. Our author, from strict observation, admits every variety upon all the mucous surfaces. If we examine the lesions in the mucous tissues produced by blennorrhagia, we shall find nothing which simple inflammation cannot produce. Sometimes it is a simple erythematous condition of the membrane, the dry clap of some authors; sometimes we have the catarrhal form, in its different conditions, mucous, muco-purulent. Again, we may have true phlegmonous complications, producing chordee and abscess along the course of the urethra. But whatever may be the condition of the tissues, there is nothing which can be compared to the syphilitic symptoms, properly so called.

Are the consequences of blennorrhagia to be compared to those of syphilis? There are some analogies, but also some marked differences.

One of the first consequences of blennorrhagia, and one which resembles the effects of syphilis, is bubo. But in the first place, this is much more rare after blennorrhagia than after chancre. Then, again, it never follows any other blennorrhagia but that which is seated in the urethra, in the two sexes. When bubo does exist, it is purely inflammatory, and has very little tendency to suppuration; and when this does happen, the pus *is never inoculable.*

Blennorrhagic ophthalmia occurs only during an urethral blennorrhagia. Is it possible to confound this ophthalmia with syphilitic iritis? Can we establish the least resemblance between blennorrhagic rheumatism and syphilitic affections of the bones? And with regard to the cutaneous eruptions, M. Ricord is greatly astonished that any one with a knowledge of diseases of the skin, could find a similarity between cutaneous affections, the result of remedies given in the treatment of blennorrhagia, and the affections peculiar to syphilis. There are marked differences between them; among which may be cited the pruritus attendant upon the roseola produced by copaiba, &c., which is not present in syphilitic eruptions; and moreover the effects in the first case rarely outlast the cause which produced them, more than a week.

What medical man, at the present day, could confound blennorrhagic epididymitis with syphilitic sarcocele?

All that our author has thus far said relates to simple blennorrhagia, which we may or may not consider as the product of a particular virus, but of a virus entirely different from that which syphilis produces. According to many authors, this blennorrhagia may bring about secondary symptoms perfectly identical with that which the chancre produces; and it is incontestable that many patients affected with constitutional syphilis, attribute this condition solely to blennorrhagia.

All this is easily explained; other conditions exist besides what we have already spoken of.

Experiment and pathological anatomy have proved that the urethra, and the deep and concealed portions of the other genital mucous surfaces, may be the seat of the chancre. It is because the concealed chancre was not recognized that the doctrines of Balfour, Bell and others, fall to the ground.

The doctrine of the existence of chancre in the urethra being acknowledged, virulent blennorrhagia cannot be doubted, as this is the effect of chancre.

This idea is not new in science. Mayerne, in the seventeenth century, attributed urethral blennorrhagia to ulcers in the canal. The author has exhibited before the Academy of Medicine two morbid specimens which presented urethral chancres at different depths, which had been diagnosed before death by inoculation. Thus the urethral chancre is a confirmed fact.

But it has been objected:

"That the existence of the urethral chancre cannot explain all the cases of constitutional syphilis, which seem to be the fruits of blennorrhagia."

"That the number of urethral chancres is too small, in comparison with the cases of constitutional syphilis with blennorrhagic antecedents."

"That there are cases of blennorrhagia where it has been impossible to verify the presence of the urethral chancre, and which have been followed by constitutional symptoms."

M. Ricord agrees to all this, but says that his concession is only apparent, as he goes on to prove.

Concealed chancre constitutes the exception, in comparison with the immense number of cases of blennorrhagia. We must bear in mind how extended this disease is, and if we take a large city and allow only one case of concealed chancre in one thousand, yet we still have a large number of blennorrhagias which would be followed by constitutional syphilis.

And again, in practice we see those patients whose syphilitic affection has been preceded by chancres in the urethra; and as those patients accuse the blennorrhagia as the sole cause, this makes a great impression upon the mind of the observer, who sets it down as a formidable objection to the doctrine of the non-identity of blennorrhagia and syphilis.

The urethral chancre is rare, but the number of syphilitic affections, the consequences of it, does not appear rare. And why? because we see again only patients, thus affected; and if we establish a strict diagnosis between those cases of blennorrhagia followed by constitutional symptoms and those which were not, we should see that the apparent frequency was only illusory.

Again, have all possible precautions been taken to guard against error in these cases of constitutional syphilis attributed to blennorrhagia? M. Ricord thinks not, since he often sees medical men who are content with a diagnosis given by the patient himself.

Blennorrhagia is a malady which leaves behind sad reminiscences; consequently, when we question patients, it is of their blennorrhagia that they speak. They do not know the importance of the chancre, which when it infects the economy, is often very small and cicatrizes spontaneously; therefore it has passed unperceived, or the patient has paid little heed to what he has considered as a simple excoriation.

Those who entertain the opinion that a simple blennorrhagia may give rise to a syphilitic infection, forget that there are other ways of entrance for the virus than by the genital organs. The whole surface of the body lies equally open to its entrance. Our author cites examples of chancres seated in unusual regions, and which for this reason were not diagnosed as chancres.

Thus, the objections made against M. Ricord's doctrine fall to the ground, and he still believes:

With Giztanner, that "syphilis is often caused by chancres and buboes, and that it very rarely follows a blennorrhagic discharge."

With Swediaur, "that syphilitic symptoms are rarely manifested after blennorrhagia."

With M. Rayer, "that secondary cutaneons eruptions after blennorrhagia are rare; that we observe them much less often than after superficial and deep venereal ulcerations."

These opinions agree very well with the rarity of the *concealed* chancre as compared with blennorrhagia.

From the fact that chancres have been submitted to a methodical treatment, that is say, to a mercurial one, it has been thought that any constitutional syphilitic symptoms which might arise, should be attributed to a blennorrhagia coming on after the chancre. Our author asks, "what is the treatment which we can call methodical?" M. Vidal has recently published the opinion that with one hundred and ten of Dupuytren's pills all symptoms of syphilis would be forever done away with. M. Ricord has seen a great number of patients return to him, who not only have taken the one hundred and ten sacramental pills, but one hundred and fifty and more.

Is it not more satisfactory, he asks, to admit that the cause has not been recognized in those cases where syphilis has really succeeded to a simple blennorrhagia? He affirms that in the great majority of cases the blennorrhagia is simple and benign, but that there is also a virulent blennorrhagia, which is the case when a chancre exists in the urethra.

In the diagnosis between virulent and simple blennorrhagia, two conditions are necessary.

The first, that the diagnosis should be absolute; and the second, that it should be rational.

The former can only be obtained by artificial inoculation. Whenever muco-pus furnished by a mucous surface gives the characteristic pustule of chancre, we can affirm, no matter what has been the duration of the disease, that it is virulent, that there is a chancre somewhere, this alone being able to give the positive results of inoculation. Care must be taken not to be misled by taking the pus of a blennorrhagia, instead of that of the chancre (as this complication sometimes exists), for the result will be negative. We must also bear in mind the length of time which the discharge has continued, for there is a period, as is afterwards shown, when the ulcer passes into a state of simple ulceration, ceasing to furnish specific pus. If by inoculation the results are positive, it affords us the most absolute diagnosis possible. If, on the contrary, the results are negative, they conduct us to a rational diagnosis.

In framing a rational diagnosis, we must take into consideration the following conditions, a knowledge of which will enable us to form it correctly.

In the first place, the urethral chancre, which can never be of any extent, can furnish only a very small quantity of virulent pus. In the indurated chancre, it sometimes amounts to almost nothing—insufficient to spot the linen of the patient. On the contrary, when we have to do with a very abundant discharge, we can be well assured that it is produced by something different from chancre. Thus the nature of the secretions is of great assistance in forming our diagnosis. The secretion resulting from a chancre in the urethra is much more purulent than mucous; it is ordinarily bloody, sanious; but we must be careful to ascertain if this has not been produced by some caustic injection or by the introduction of some foreign body into the canal, or that the bloody matter has not been expelled with the last drops of urine, which would show cystitis to be the cause of this appearance.

As a general rule it is in these cases of blennorrhagia which are the least painful and violent, that we ought to fear most the presence of the urethral chancre. And as to the duration of the discharge taken as a symptom, it is not those cases which are most tenacious that we ought to dread. Our author, by one or two interesting cases, satisfies us that the source from which the disease has been taken, cannot furnish any value to the diagnosis.

Buboes are very rare in simple blennorrhagia. When they do occur, they are acute, terminate easily by resolution; or when they suppurate, it is simple pus which they furnish. With urethral chancre it is otherwise, as we shall see when we come to speak of indurated chancre.

At any rate, if by any means we have been unable to make our diagnosis, we may rest assured that if the blennorrhagia is virulent, six months will not pass without the appearance of the constitutional symptoms, if the infection has taken place, which is sure to be the case if the chancre is indurated.

It is important, then, to know if the chancre existing in the urethra is indurated or not. If there is no blennorrhagic complication with the chancre, the patient scarcely suffers in urinating, although the jet is smaller and twisted on account of the diminution of the canal, and the erections are not painful when the chancre is seated in the balanic region. In order to ascertain the presence of the ulcer, we must exercise a pressure upon the urethra from the dorsal to the under surface, exactly as if we wished to make the meatus gape open. If we do this in the contrary sense, from right to left, the induration cannot be felt.

As to the treatment of blennorrhagia, many still persist in giving a mercurial treatment, believing it to be syphilitic. Some, although admitting the existence of virulent blennorrhagia, pretend that they cannot distinguish it from the simple benign blennorrhagia, and consequently give a mercurial treatment. Hunter was of this number.

Others, regarding it a light form of syphilis, advise a demi-treatment. M. Ricord, in answer to this, says, alas! there is nothing to be trifled with in syphilis; either it exists, or it does not; and consequently, so far from any half way treatment being employed, it should be as complete as possible. If syphilis does not exist, what is the use of an anti-syphilitic treatment?

He strongly advises the abortive treatment in blennorrhagia. He says that it is incontestable that most of the accidents attendant upon this disease, do not manifest themselves before the end of the first week, or at the commencement of the second, and that generally it is still later, and that in a great majority of cases it is only where there has been no treatment or a very insufficient one, that these accidents arise. So far, then, from the abortive treatment producing any accidents, it is the sure method of preventing them; it is the prophylaxis against consecutive accidents. The injections, constituting the most important part of the abortive treatment, so far from producing strictures, constitute the most certain treatment against them. The quicker an urethral discharge is arrested, the less we have to fear from alterations in the structure of the tissues. The abortive treatment should be applied immediately upon the appearance of the disease. The commencement of the disease is known, its termination and consequences are always uncertain.

Thus in his letters on blennorrhagia, our author strives to show that if this disease has a special cause, it is not always possible to distinguish it from the common causes of inflammations of mucous membranes; that it is entirely different from the cause which produces syphilis; and that its consequences and proper treatment are different.

M. Ricord next commences the subject of syphilis, by remarking that the questions, whence and through whom did syphilis commence, are forever insoluble. All that we know for certain, is, that the disease, as we see it at the present day, is never developed spontaneously in man, but that it is always transmitted—and, moreover, that we never meet with it in any other animal.

What strikes every one who studies the history of this disease, is the similarity in the descriptions as given by the ancient authors, and by those anterior to the epidemic of the fifteenth century, compared with the descriptions of what we term the primary symptoms at the present day. What was wanting to ancient observers and historians, was the more exact knowledge of the relations between the primary and secondary symptoms.

Was the leprosy of the Greeks or Arabs similar to the leprosy of our day? In no wise, for their leprosy was communicated by sexual intercourse, while ours is certainly not.

Our author begs to be excused from discussing the true nature of the epidemic of the fifteenth century, or its origin. He only undertakes to say a few words upon the opinion generally entertained, that it was brought from the New World.

In order to have brought about an epidemic upon so large a scale, it would have been necessary that all or nearly all the sailors of Christopher Columbus should have been infected with syphilis. It would have been necessary that the primary symptoms should have remained in the state of progress or of specific *statu quo*, susceptible of furnishing the contagious pus, during a very long voyage, which was not then accomplished by means of steamers. Then these infected sailors, arriving at Lisbon and Bayonne, did not first infect the women of those places (and it is not probable, judging from the general habits of sailors, that they were continent upon arriving at port) ; but they passed on into Italy, and to whom did they communicate the disease ? We cannot say ; all we know is, that in the midst of three armies, French, Spanish and Italian, a disease already known since 1493, raged furiously.

We find that the venereal symptoms of the present day are infinitely more like those which the ancients have described, than like those of the epidemic of the fifteenth century.

M. Ricord remarks that in studying the description of this epidemic, its mode of transmission, the gravity of the symptoms, and the predominance of the constitutional infection over the local symptoms, which are either wanting or which pass unperceived, he is struck with the fact that all this appears more like acute glanders and farcy than syphilis. We must remember that our knowledge of these two last diseases is very recent, and yet the aptitude of man to contract them has always undoubtedly existed. May not many patients affected with the glanders have been taken for syphilitic !

The mode of transmission of the epidemic of the fifteenth century ought to strike us. The malady was often communicated by the breath, which is entirely different from the propagation of syphilis, which requires immediate contact. May not this epidemic be considered as a mixture of the venereal disease as known to the ancients, with the glanders and farcy, which diseases are so easily and spontaneously produced among horses, particularly in time of war? Then do we know what the glanders is capable of producing, transmitted from man to man, or what its hereditary influence may be? Herein lies a great and interesting subject for inquiry.

M. Ricord finally adopts the conclusion of Voltaire, that syphilis is like the fine arts, the inventor and origin of which no one knows.

He comes now to determine the source, whence the specific cause, the syphilitic virus, is derived. After long observation and study, M. Ricord has succeeded in demonstrating that one alone of the syphilitic symptoms furnishes regularly, under certain conditions, a purulent matter, which in its turn re-produces the same morbid poison, and that without limits. This lesion, source and origin of the secretion, which, if placed under favorable circumstances, inevitably produces the phenomena which we are to study, has preserved the name of chancre.

Artificial inoculation has shown that it is the chancre alone which furnishes the pus by which positive results are obtained; and moreover that when these results were obtained with matter not taken directly from the primary sore, this secretion was furnished by surfaces which could not be inspected. No one now can deny the possible existence of a concealed chancre, which satisfactorily explains the few cases where positive results have been obtained by matter taken from an apparently non-ulcerated surface.

It has been said that inoculation did not serve in any way to prove the existence of the specific cause of syphilis; that with any kind of matter, the same results could be obtained, which M. Ricord pretends to produce only with that of the chancre. He shows that what is depended upon in order to support this doctrine, is fallacious, and unworthy of scientific observation. He says that he has inoculated the same individual, and this, too, many times, with a variety of pus, and that the pus of the chancre alone gave positive results.

Another objection has been made to inoculation, viz., that it does not prove any thing as to the nature of the cause, from the effects which it may produce upon an individual already infected; and that in inoculating a patient with the secretions furnished by himself, we could arrive at no conclusion, inasmuch as every wound would become syphilitic.

Herein lies a strange error. Rest assured that no wounds or lesions would become syphilitic, unless they came in contact with the virus. Bleed syphilitic patients and apply leeches as much as you will, if you protect the wounds from the virulent contact.

In order that the results should be inevitably obtained, of course the virulent matter should be taken from a chancre at a certain period, that is to say, at the period of progress, or of specific *statu quo*. If we take the pus from a chancre during the period of reparation and of cicatrization, we shall have only that which is simple and harmless, and our inoculations will be negative. If the chancre still exists as a chancre, *its pus is inevitably inoculable.*

The pus of the chancre does not always present itself with the same characteristies. It may present all the known varieties of pus or of muco-pus. It may be acid or alkaline; contain animalcules or not. These different conditions, however, belong only to its vehicle, and do not change at all the *nature* of the pus, which remains always the same. However, putrid pus is found not to be inoculable—gangrene destroys the virus.

The virulent matter, in order to act, has no need of being recently secreted and warm. Preserved as we preserve vaccine matter, it acts equally as well. There is no need of physiological action or of an orgasm of the part which is to furnish the contagion. All that is necessary, is a simple excoriation, a solution of continuity, in order that the effect should be inevitably produced. There are no refractory constitutions, as in variola or vaccinia; the most perfect equality exists before a lancet charged with virulent matter.

There is really no difference between the natural contagion and the artificial inoculation of syphilis. The chancre may develope itself anywhere upon the body, upon the whole internal or external integument which is accessible, without the need of any special functions or physiological condition, either on the part of the portions which furnish the infecting matter, or of those which become infected. Other conditions are necessary for the contagion.

If we examine the parts which become affected we shall find that it is those parts which present the conditions most favorable for mechanical lesions; we shall find that it is where the follicles are numerous and large, into which the virulent matter can introduce itself, that the primary sore developes itself. The chancre, as was before remarked, may develope itself any where upon the body—no physiological conditions in the genital act are necessary for its production; a simple solution of continuity suffices.

In every malady *incontestably* contagious, we find that the traumatic conditions predominate, and that art may repeat what nature does. Thus vaccinia inoculated, does not differ from ordinary vaccinia. The same may be said of glanders, variola, malignant pustule, &c.; and why should syphilitic virus be an exception to the common rule?

But it is said that the chancre is not the only contagious syphilitic symptom. There are some secondary syphilitic symptoms, whose contagion the lancet has not yet discovered. Thus the mucous tubercles are considered by many as contagious, and consequently transmissible. M.

Ricord has always obtained negative results in his experiments with these tubercles. In one experiment which he cites, he was much surprised in obtaining positive results with the secretion from mucous tubercles ; but on close examination, he found a chancre amongst them at the specific period. Afterwards, new experiments made with the secretion of the tubercles at a distance from the chancre, gave negative results.

In the observations which have been cited of mucous tubercles which have communicated syphilitic symptoms, no account has been made of the time which has elapsed since the infecting coitus. Generally it is several weeks, so that it is impossible to determine the true nature of the symptom which was the source of the contagion. Many forget or do not know that by a succession of phenomena easy to observe, the primary sore passes locally *(sur place)* from the condition of an organ of virulence to that of a secondary symptom ; furnishing no longer the specific pus.

Many writers on syphilis give, as a proof of the contagious nature of mucous tubercles, the fact that they may be successively developed upon the portions of the integument which are contiguous to those where the first developed themselves. For instance, if from one side of the anus the tubercles gain the other side, many say it is the result of contagion. But they forget the cause which produced the first tubercle, viz., the constitutional infection, a condition which may produce a second and third mucous tubercle, for they do not all appear at the same time. As to their situation, this cannot come at all in aid of the doctrine of contagion. If there is contiguity in those portions of the skin where these mucous tubercles appear, we must also remember that here likewise the acrid secretions are greater, and that the skin has a tendency to the *mucous transformation*, as about the genital organs, anus, &c. The chancre may take on the appearance of the mucous tubercle, at the period of reparation ; and if we add to this the fact, that the chancre may be developed in unusual places, where this transformation may be more rapid, as upon the lips, tongue, nipple, &c., we shall see how easy it is to be deceived.

Many physicians judge a chancre to be a secondary symptom, because they happen to find it in some unusual place, as in the mouth. This is certainly a grave error. Our author cites examples of constant intercourse between parties affected with mucous tubercles, without anything being communicated, and he is firmly convinced by experiment and observation that they are not contagious.

But it is as regards the transmission of the secondary symptoms from the nurse to the child, and *vice versâ*, that this question is important. The fact of this transmission is generally admitted. Hunter, however, denied it.

If we examine, our author says, the basis upon which the opinion of the contagion of secondary symptoms, communicated from nurse to child and *vice versâ*, rests, we shall be surprised at the little value of facts, and with how little, very serious men are content. In the cases of this description, which M. Ricord has seen both in public and private practice, he has been able to discover the regular point of departure of the disease, the primary chancre, either upon the nurse or child—and in those cases where he did not find the primary cause, the infants were not presented to him until five or six months after being put to nurse. The nurse, at the moment of taking a child, may be laboring under the syphilitic diathesis, which nothing yet indicates. This fact leads M. Ricord to remark that sufficient care is not taken in the examination of the nurse before employing her. Even if a careful examination is made, the diathesis may still exist, when all trace of primary or successive symptoms have passed away, as especially happens with chancre upon the neck of the uterus.

The child may be born with hereditary syphilis ; neither nurse or child have yet anything apparent, when after a certain lapse of time, secondary symptoms make their appearance. These may manifest themselves in the child before or after they do in the nurse, or *vice versâ*, so that the consequence is, that the first one affected, accuses the other, when in reality there is merely a coincidence; there being no connection between the two affections.

It sometimes happens that nurses contract syphilis whilst they are nursing, and generally by the genital organs. While in this condition they infect the child by means of their fingers contaminated by the virus. They even infect their husbands, and then the evil is always referred to the *rotten* Parisian children.

One quite common mode by which inoculation is effected in the case of nurses, is the conveyance of the virus by their fingers soiled with the secretion from the parts affected, to the nipple. They pull upon the nipple, which is more or less cracked, and implant there a chancre. Thus the infants become inoculated. Again, the individual who may draw off their milk by suction, may have a chancre upon the lip, and thus communicate the disease.

An infant at the time of parturition may contract chancres from the mother, and then communicate them to the nurse. Or the child may become affected by strangers upon whom no suspicion falls; a curious example of which fact our author gives. In all cases, with care and perseverance we can discover the source of the syphilitic symptoms. At any rate, we see how much reserve and prudence is necessary, before accepting the contagion of secondary symptoms as a demonstrated fact.

M. Ricord does not absolutely deny this mode of transmission of syphilis, but says that this fact has not yet been proved, and that it will never be, unless it is by means of inoculation.

But some authors pretend to have proved by inoculation that even the secondary symptoms are contagious.

M. Ricord answers this by showing that in some of the experiments cited, there was no proof that the pustules from which the matter for inoculation was taken, were syphilitic. Next, that in other experiments sufficient care was not taken to preserve the punctures made by the lancet, from the accidental inoculation of the virus to be found so abundantly in a venereal hospital.

Wallace, who has made many experiments, has only twice succeeded in inoculating secondary symptoms, in which cases the results were not satisfactory.

But there can be no exceptions ; either secondary symptoms are inoculable, or they are not. Experiments, up to this day, have satisfactorily proved that they are not.

But it is said, these experiments having been always made upon patients already infected, they may be perfectly successful if made upon a healthy person. M. Ricord says, that this might be advanced by those who entertain his doctrines, but certainly not by those who profess that so far from constitutional infection preventing a new infection it is only necessary to make a simple wound, in order that this should take on a syphilitic character.

As to the transmission of syphilis to animals, all experiments have given negative results. Experiments have been recently made by M. Auzias Turenne, wherein he asserts that he has transferred the syphilitic virus from man to the monkey, producing a primary sore. Our author, after devoting some space to the discussion of this subject, satisfactorily shows that, in the first place, the pretended chancre was not a true chancre, as we see it in man, but merely the puncture of the inoculation slightly inflamed, and holding the pus which had been conveyed to it, and which was still inoculable on being re-conveyed to man. In short, that the monkey served only as a soil for transplantation. M. Auzias gives his ideas in a letter to the author, which is of interest.

Next, M. Ricord lays down the following proposition, which appears to him incontestable :—

"The chancre (primary ulcer), at the period of progress or of specific *statu quo*, is the only source of the syphilitic virus (morbid inoculable poison)."

He then goes on to give the pathology of chancre, as clearly shown by artificial inoculation, which is the repetition of what nature does.

In the positive inoculations, from the moment of the insertion of the virus, the evolution of the phenomena commences—there is no incubation. He explains the cases of pretended incubation by the fact that if the virus, in cases of contagion by the ordinary manner, be placed upon surfaces more or less denuded, or even perfectly healthy, it requires a longer or shorter time for them to receive the virulent action. Add to this, the carelessness of patients about themselves, and the want of satisfactory observations, and we can easily explain these seeming periods of incubation.

If the inoculation fails, the puncture sometimes becomes a little irritated, but this soon passes away. It must be remembered, however, that there are for syphilis, as well as for vaccinia and variola, *false pustules*. These not having been properly understood, have given occasion for some authors to say that they have obtained positive results from pus coming from other sources than the primary chancre. But with ordinary care, these *false pustules* may be distinguished. They are but slightly developed; generally, they consist only of simple bullæ, beneath which we find a superficial vesication of the skin. Occasionally a more extensive inflammation ensues, but their progress is always very rapid, and the cure is equally rapid, without the intervention of any treatment.

When inoculation succeeds, it is always by a pustule that it commences. This experiment can be tried at any time.

The primary sore, however, presents numerous varieties, either at its commencement, during its progress, or later.

Generally the chancre commences by a superficial or deep ulceration—although this ulceration does not always destroy the entire thickness of the mucous membrane or skin. Sometimes it commences by taking on the form of an abscess. Whatever may be the form under which it does commence, these varieties have no influence upon the ulterior form which these ulcerations take. This point is important, as it bears upon the question of the unity or plurality of the syphilitic virus.

M. Ricord says, when the inoculation is made upon the patient himself, the ulceration which follows the inoculation takes on the same form and offers the same varieties as the primary symptom which furnished the inoculable pus. Thus, if it is an indurated chancre from which the pus has been taken, the new ulceration will be the same. The same may be said of the phagedenic chancre, &c. We are not entirely satisfied that one form of chancre in an individual may produce a different form in another person, although observation seems to have shown this. We can suppose that the individual who has a different form, may have contracted it from a different source than that which he accuses.

M. Ricord believes in only one syphilitic virus. The great variety which the primary ulcer presents at the period of progress, and which may be classed thus—simple chancres; inflammatory chancres with a gangrenous tendency; phagedenic chancres; indurated chancres;—may be explained by causes entirely foreign to the specific cause. Thus, he cites the effect of alcoholic drinks, particularly in hot weather, upon the simple chancre, rendering it phagedenic; also the effects of improper hygiene, the use and abuse of rancid mercurial ointments in the dressings, &c. Add to these, also, the influence of a previous syphilitic diathesis.

He comes now to speak of those conditions which are the most important and interesting, and which preside over the *induration* of the chancre. A knowledge of this induration is no new thing. Some pretend to trace a description of this even to Galen. However, it is to Hunter that the honors of this discovery are now given; and yet, neither Hunter nor Bell were acquainted with all the value of induration. This induration, which may line and border the chancre, merits all possible care and attention on the part of the practitioner.

All chancres do not become indurated; on the contrary, it is a small proportion, and if the doctrines of our author are correct, their number will go on diminishing.

As to what individual cause, or what condition is necessary for the production of this induration, we do not as yet know. If we interrogate age, sex, temperament, habits, we obtain no solution to the question. But this is what a long observation teaches:—

"GENERAL RULE.—A patient who has once had an indurated chancre, will not have another."

It is probable that there are exceptions for this law, as for vaccinia and variola, but they are very rare.

When there is an indurated chancre, there is necessarily constitutional syphilis. The syphilitic *temperament* is established. The indurated chancre is to syphilis, what the *true* variolic pustule is to variola; what the *true* vaccinal pustule is to vaccinia.

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

As was before remarked, the syphilitic diathesis being acquired, any new chancre which may be contracted does not become indurated, and this immunity against a new general infection is, also, according to M. Ricord, transmitted hereditarily. Hence, this must have an influence upon the diminution of constitutional syphilis.

At what epoch does this induration, which constitutes the principal character of this variety of chancre, commence? This is an important question, for the moment that the induration commences, the disease is no longer only local. It is difficult to determine this, for patients do not present themselves, generally, till long after the contagion, which is easily explained by the fact that the indurated chancre is essentially indolent, of slow progress, and suppurating but little, and may often even pass unperceived. However, under circumstances in which it has been possible to arrive at anything precise, it is never before the third day that the induration is manifested. In every case, it is always in the course of the first or second week. It would appear also certain, that if a chancre exists more than three weeks without induration, this condition will not take place.

Induration is a precocious phenomenon.

This feature of the chancre being so important, it is very necessary that it should be perceived and understood. Consequently, M. Ricord gives us the means by which it may be distinguished from other kinds of induration, produced by local applications, &c. The study and appreciation of this is very important to those who would properly understand the modern doctrines upon syphilis. Our author has fully treated this subject in his letters, which should be attentively perused.

He next speaks of the cicatrization of the chancre, the mode by which the different varieties heal. He speaks particularly of the fact before mentioned, that after a chancre has infected the economy, it may undergo a transformation locally *(sur place)*, and take on the appearance of the mucous tubercle, thus apparently justifying the opinion that secondary symptoms may be contagious and infect the constitution.

The chancre once cicatrized, never returns. If a new inoculable chancre shows itself after its complete cicatrization, it is the result of a new contagion.

M. Ricord is aware of the many difficulties of establishing a correct diagnosis of chancre in many cases, therefore he says that the only positive pathognomonic character of the chancre at the period of progress or of specific *statu quo*, is found in the pus which it secretes, and which is inoculable.

He devotes a letter to the prophylactic treatment against syphilitic diseases, and to medical police, which is instructive and interesting.

He now comes to the treatment of the chancre, and first speaks of cauterization, against which treatment many have serious objections.

We cauterize the bite of the snake, of the rabid dog, dissection wounds, &c., and why should we not do the same when we deal with syphilitic virus? The chancre is always at the commencement a local affection, which art can correct; and even without the intervention of art, may remain local. And even when it is to infect the economy, this result is not immediate, but there exists a sufficient interval between the cause and the effect, which gives us the opportunity to destroy it.

What do we gain by cauterization? First, We prevent the constitutional infection. Second, We prevent the production of buboes. Third, We prevent the progress of the primary sore, the consequence of which may be the loss of precious organs, and of greater or less deformity. Fourth, We destroy a focus of contagion.

The objections which many make against cauterization, he fully shows are very foolish and unworthy of the truly scientific.

If we destroy the chancre early, by the abortive treatment, that is to say, from the first to the fifth day, we are almost safe from any constitutional infection. At any rate, when we are too late, cauterization will shorten the duration of the primary sore. We must be careful not to consider the age of the chancre as dating from the moment when the patient perceived it, but from the coitus which must have produced it.

Directions are given as to the proper manner of cauterization, and the importance of doing it thoroughly. Also the best caustics to be used.

M. Ricord next proceeds to the important subject of bubo. First he discusses the opinion of bubo appearing primarily *(d'emblée)*, i. e., without any other symptom having preceded it. If, as some maintain, the simple deposit of the virus upon non-denuded surfaces was sufficient to produce buboes, without any antecedent symptoms, these buboes, which are the most rare, would be the most common, for the circumstances in which healthy parts come in contact with those which are contiguous, are extremely frequent.

But his opponents ask, why may not the syphilitic virus penetrate the economy without solution of continuity, as is the case with mercury by simple frictions?

If this be the case, he answers, with mercury, is it the same with caustic potash? Do we see the virus of the snake or rabid dog enter the economy without a wound? Certainly not. Let us not invoke false analogies.

One curious fact is, that the partizans of the theory of primary buboes *(d'emblée)*, have never cited any examples of their occurrence anywhere but in the inguinal region—when we know that the true syphilitic bubo may be found elsewhere. Neither do they take sufficient care to diagnose between the syphilitic bubo, and that which may be produced by other causes, as, for instance, by scrofula, &c. Thus, our author denies the primary bubo *(d'emblée)*, and says, with Hunter, that in most cases, if we take proper care, we shall find a small chancre to be the cause of all the trouble.

M. Ricord, after having denied in the most absolute manner the existence of the primary bubo *(bubo d'emblée)*, goes on to explain his ideas upon bubo.

In the largest sense of the word, venereal symptoms, whether virulent or not, blennorrhagia and chancre, may give place to *sympathetic* glandular enlargements or buboes. These are the only ones which can accompany blennorrhagia, and are of a nature essentially inflammatory, easily giving way to antiphlogistic treatment, and in those rare cases where they suppurate, never yielding an inoculable pus. The sympathetic bubo is by no means a specific symptom; other causes besides venereal diseases may give rise to them.

The specific bubo, distinct from other glandular diseases, can be only the product of virulent venereal affections, that is to say, of syphilis. It is the mediate or successive product of the contagion, or the result of the constitutional infection, which constitutes two kinds, perfectly distinct and very important to understand.

The first kind of syphilitic bubo contains two varieties almost always confounded together.

The first variety of the mediate bubo is that which follows the non-indurated chancre. This bubo of *absorption* is not inevitable. We may even say that there are more non-indurated chancres without bubo, than otherwise. This variety of bubo is the termination of the direct lymphatics, whose extremities are bathed in the pus of the chancre. This relation of the lymphatics is necessary; and when it does not happen, buboes do not occur. Thus we may understand why they occur so often in consequence of chancres about the frænum, and why they do not occur after inoculations upon the superior region of the thigh. The bubo accompanying the non-indurated chancre, not only never precedes it, but never occurs till after the first week, and under certain circumstances not till much later.

With the non-indurated chancre, the bubo is most generally solitary, when there is only one chancre, and never affects *but the superficial glands*, so that the division of buboes into superficial and deep, cannot apply to virulent buboes.

The bubo accompanying non-indurated chancre is inflammatory, acute, and tends almost inevitably to suppuration. Accompanying the bubo containing specific pus, there may be more or less *common* inflammation of the neighboring parts, going on to suppuration, but yielding only simple pus, which is not inoculable. Care then should be taken, in inoculating from a bubo, to take the pus from the gland which is virulently affected, for this alone will furnish positive results.

The second variety of *mediate* bubo is that which succeeds to the indurated chancre, and which is very important to study and understand. It differs as much from the other variety of bubo, as the indurated chancre differs from the non-indurated. This bubo is generally more precocious than the other, coming on usually during the first week, and almost never delaying beyond the second week. With the indurated chancre the bubo is inevitable, and we never see it follow at a late period.

The author says that he never saw a chancre *specifically* indurated, without the symptomatic enlargement of the neighboring glands. This is so regular and characteristic, that it may serve to indicate the nature of the chancre which has preceded it, when this latter has already disappeared, or when it is concealed in some deep region.

Chancre is alone the cause of this form of glandular enlargement.

Inflammation of the lymphatics may accompany this form of bubo as well as the other. Certain characters which he describes mark this form of lymphangitis.

The glands upon the side corresponding to the chancre, are generally much more affected than upon the other. The infection is very rarely confined to one gland. In the great majority of cases, numerous glands are affected, which we may call the glandular *Pleiades*.

Our author particularly insists upon this point in his clinical lectures, as well as in these letters, viz., the presence of the numerous small indolent indurated glands, pathognomonic of the indurated chancre—so different from the one large inflamed gland accompanying the non-indurated chancre. Moreover, the bubo of the indurated chancre never yields *specific* pus—it does not inoculate.

This variety of bubo is already a symptom of secondary transition, of which we shall find the continuation in the constitutional buboes, or in the enlargement of the posterior cervical glands, constituting the second kind of syphilitic bubo, upon which he speaks afterwards.

Twenty years experience permits M. Ricord to offer the two following propositions:—

1. Every bubo furnishing inoculable pus, is never followed by constitutional symptoms.

2. The multiple indolent bubo, the consequence of the indurated chancre, is a further and sometimes the only proof of the constitutional infection, when we have not been witness to the induration of the chancre.

The subject of bubo, as laid down in these letters, is extremely worthy of careful study, as, according to our author, it is by them alone that we have sometimes to decide whether our patient is to expect secondary symptoms or not.

The therapeutics of bubo next engage his attention.

As to the constitutional symptoms, M. Ricord again affirms that they can only be the consequence of the chancre, or be acquired by way of inheritance.

"*No constitutional syphilis without chancre, or without father or mother having been infected.*" This is a truth more consoling than that doctrine which makes syphilis the indomitable enemy of mankind.

But does the chancre always bring about the general infection? Daily observation shows that the *indurated* chancre is alone inevitably followed by the constitutional symptoms. The induration is the proof of the general poisoning, and in a measure the first secondary manifestation. Whereas, the true non-indurated chancre with the bubo containing specific pus, never infects the constitution. These propositions are absolute; but in order to establish them, a very strict diagnosis is necessary. Thus we see that it is perhaps the largest number of chancres which do not infect the economy.

It has been asked, how can it happen that a poison should be placed in contact with the circulation without this becoming contaminated? In answering this, we must bear in mind the numerous cases where the inoculations of variola have failed; those where it was no longer possible to vaccinate; the numerous observations upon malignant pustule locally destroyed, &c. Why should not the syphilitic virus, even less active, enjoy the same privilege?

The constitutional infection is dependent neither upon the number, extent, nor absolute duration of the chancre. But what interval elapses between the chancre and the first secondary symptoms? This is what clinical observation teaches in this respect, when no treatment called specific has been made, and the disease has been abandoned to itself:

"*Six months never pass, without manifestations of the syphilitic infection supervening.*"

This is an inevitable law; and even six months is a very long time, for generally the secondary symptoms appear within the first six weeks.

In certain works on Pathology the constitutional syphilis is not considered as giving rise to a well-characterized diathesis; it is thought that it is not systematic and does not pursue its course with order. M. Ricord asks if there is any other general condition where more specific symptoms manifest themselves, where they are repeated and are more regularly transmitted by way of inheritance? But in order to appreciate this regularity, we must observe the disease in the state of *nature*, without the intervention of treatment. Then do we see the symptoms which follow in succession, and which differ according to the time of their appearance, by their seat, their form, their duration, and termination, by their influence upon generation and progeny, and also by their greater or less obedience to such or such a medicament.

We can never express these shades, so well marked, by the term acute and chronic stage; for each may be acute or chronic without changing in any degree the other characters upon which the classification is based. The absolute duration of the malady is not the only cause of the differences in the seat and form of the symptoms to which it gives rise. Thus the roseola which for some is an acute symptom, may exhibit itself several times in the course of the first and second year of the affection, while the osseous affections which the same persons would range among the chronic symptoms, show themselves, in some cases, during the first five or six months.

In the next two letters the author criticizes a treatise published by M. Waller of Prague, upon the contagion and inoculation of secondary symptoms. Therein are contained the opinions of M. Ricord upon this subject, and there he meets his opponent upon his own ground, analyzing thoroughly the very cases which M. Waller brings forward to sustain his arguments.

M. Ricord next resumes the subject of the manifestations of constitutional syphilis. The first symptoms which are manifested, after the syphilitic diathesis has been established (and the period which elapses until such manifestation, may be considered, in a measure, as a period of incubation), are a greater or less disturbance in the sanguineous system. There is a diminution of the globules of the blood, producing chloro-anæmia, which is often exceedingly well pronounced. Accompanying this, or even before the appearance of any other symptom, there are troubles in the vision, general muscular weakness, neuralgic pains in the head, and rheumatismal pains in the limbs. These neuralgic pains sometimes fail to appear; but when present, they have common characteristics. These consist in intermittent nocturnal pains, which manifest themselves particularly under the influence of heat, especially that of the bed. Those patients who turn night into day, and *vice versâ*, equally experience this. The author then describes more particularly the character of these pains.

At this period of precocious secondary symptoms, one of the most constant manifestations is an enlargement of certain glands, to which we may strictly give the name of secondary buboes—and these merit particular attention, as they are somewhat characteristic of this period. This affection rarely fails, and is one of the first proofs of the infection. It comes on sometimes at the third week, but more frequently after the sixth. This enlargement of the glands is indolent, *mutiple*, and *necessarily symptomatic* of the indurated chancre. Its seat of predilection is the posterior cervical region. These glands never acquire a large size; they never suppurate, at least never *specifically*. You will never meet with these after a certain epoch of the infection, either with the late secondary symptoms or at the tertiary period, unless they have been previously in existence.

The alopecia, so far from being a late symptom and one of a long standing and grave infection, is, according to our author, one of the very first symptoms of constitutional syphilis. As to the affections of the skin and mucous membrane, the earlier they appear, the more the forms are superficial, generally disseminated, and more or less confluent. If we follow the syphilitic evolution, we shall see with what regularity and constancy, the exanthematic eruptions of a rubeolic or erythematous form, show themselves.

As the infection grows older, the symptoms which it produces, and which tend to become more and more grave and deeply rooted, seem, by a sort of compensation, to become also less numerous. While the early secondary symptoms attack the superficial mucous membrane of the lips, cheeks, and the borders of the tongue, the tardy symptoms attack the tongue more deeply, the velum palati, the parts behind the posterior pillars, the pharynx, &c., where they produce grave alteration, and terrible ulcerations.

When the periosteum and bones are affected, the pains precede or accompany the attack. These true osteoscopic pains are entirely distinct from those accompanying the secondary period. They are seated in the superficial bones, are fixed, and do not have that moveable *rheumatismal* character ; they are nocturnal, but are not exasperated by the heat of the bed ; but they are increased by the touch. A swelling, a tumor of the periosteum or bone, is formed where these pains are seated.

Our author terminates the thirty-first letter by a summary of the three periods of the syphilitic infection, with their accompanying symptoms.

Constitutional syphilis is certainly one of the greatest calamities to which humanity is exposed, and yet fortunately it is quite rare, and does not attack all those who expose themselves to it. This *inaptitude* the author has found in certain idiosyncrasies, and the observation which teaches him, that one does not have the constitutional infection twice, permits him also to believe that since syphilis is hereditary, under certain circumstances, this inaptitude acquired by the parents, and which renders them indemnified, may be transmitted to their children. Hence, endeavors have been made to impress upon the economy a general disposition equivalent to that which vaccinia or a first variola produces, in preventing the general infection and its consecutive effects.

In our endeavors to arrive at so desirable a result, a certain reserve is necessary, as well as great care and prudence.

M. Ricord next discusses the subject of syphilization.

It has been advanced by M. Auzias Turenne, from experiments made upon animals, that we have it in our power to render individuals refractory to the direct and immediate action of syphilitic virus, and thus ensure them against constitutional symptoms. He pretends to have observed, in making successive inoculations upon animals, that the last became progressively less and less marked, and of shorter duration, until finally they had no effect. M. Auzias explains this state of things by the supposed fact that there is a sort of infiltration of the syphilitic virus, producing a modification in the economy, similar to what is produced by vaccination, and which he terms syphilization or syphilism. M. Ricord says that he has seen many patients, too many, in fact, who had had chancres at different periods, without the last one being less grave than the first ; and moreover, that the non-indurated ones which had previously existed, did not prevent one of the last from becoming indurated.

In the observations which have been made, the pus which served for inoculation came from non-indurated chancres, and in the only case that the pus coming from a primary sore which did determine a constitutional infection, the healthy individual upon whom the experiment was made, had an indurated chancre, and a general infection. After all, what can we think of a preventive method, which, in order to insure one from contracting a chancre, the risk of which one does not inevitably run, exacts that he should first receive from twenty to sixty chancres, and without knowing how long this immunity may last ?

M. Turenne writes a letter to M. Ricord explaining his views more fully, an analysis of which we do not think it important to give here. M. Ricord also gives his views upon the subject. Suffice it to say, that the subject of syphilization, after having created quite a stir among the French hospitals, has lately received its *quietus*, for the present, at least, at the hands of the French Academy.

We come now to the treatment of constitutional syphilis. Our author says that, according to our present knowledge of syphilis, the best preventive against the constitutional infection consists in destroying the primary sore as soon as possible. But if we arrive too late to count upon the abortive treatment, must we hasten to prescribe a general specific treatment? No, the infecting chancre is the rarest symptom—and when this does not occur, there is no danger of the general infection, and a mercurial treatment is rather hurtful than otherwise.

Some are of opinion, that inasmuch as the primary symptoms are often easily and spontaneously cured, we should wait until secondary symptoms actually make their appearance before having recourse to a specific treatment. Our author says that when he has an indurated chancre to deal with, he has recourse, as soon as possible, to the specific, that is to say, to the mercurial treatment. This treatment may prevent or simply retard the constitutional manifestations during a period which is difficult to limit, between months and years.

There are no practitioners who have not seen patients, who, after having been specifically treated, have enjoyed excellent health for a period of ten, twenty, and thirty years, and who, either for the first time or as a relapse, have presented the characteristic symptoms of syphilis. Thus we are forced to admit the persistence of the diathesis, compatible with apparent good health—and how can we conclude upon the absolute destruction of the syphilitic disposition when once acquired?

To continue the treatment only until the disappearance of the symptoms, is a method which is the most likely to be followed by future trouble—neither does the continuation of the treatment after the cure of these symptoms, for the same length of time that it occupied to obtain the cure, give any more satisfactory results. Neither is salivation any guage. But, the author says, six months treatment at a daily dose which has a marked effect upon the symptoms that we have to combat, and which effect, we are convinced still continues after their disappearance, constitutes the most rational and satisfactory method. He then gives his ideas upon how and when mercury and the iodide of potassium should be given—and finishes his letters by summing up the therapeutics of syphilis.

TREATMENT

OF

VENEREAL DISEASES.

32

EXTRACTS FROM M. RICORD'S

CLINICAL LECTURES ON THE

TREATMENT OF VENEREAL DISEASES.

BLENNORRHAGIA.

Blennorrhagia is a disease which is at first, and which continues almost always, definitely local. Certain complications sometimes develope themselves in the neighboring parts, by the continuity or contiguity of tissues; but the sympathetic effects at a distance are much more rare. The number and gravity of the symptoms which follow, are in proportion not only with the intensity of the disease, but also with its duration.

Blennorrhagia does not attain its greatest intensity immediately, nor does it pass through certain regular stages.

The abundance of the suppuration depends upon the degree of inflammation present. The pretended dangers of the repercussion of the discharge or of its rapid cure is perfectly chimerical — we may establish the contrary proposition, which is, that the quicker we arrest the disease, the sooner we are free from consecutive accidents.

It follows, then, that the treatment should tend to prevent the development of the disease, to diminish the intensity of the symptoms, when we cannot arrest it at the commencement; and in all cases to shorten its duration as much as possible.

Abortive Treatment.— When there are no symptoms of acute inflammation, we may employ injections of the nitrate of silver. This substance has an excellent effect upon inflamed mucous surfaces. With these injections, either cubebs or copaiba should be administered internally.

If there already exists too much inflammation or pain, or if these symptoms are manifested under the influence of the injections, the abortive treatment is no longer applicable. The injections should then be abandoned, but the internal remedies continued. In addition to this, the palliative treatment must be insisted upon; repose, the recumbent posture as far as possible, the use of the suspensory bandage, strict attention to diet by the avoidance of all stimulants, particularly of certain liquors, such as beer, &c.; the use of demulcent drinks in large quantities; free evacuations from the bowels by the aid of simple purgatives or of enemata; the use of the warm bath, continued for some time; local baths, lotions, fomentations and cataplasms according to circumstances.

In the application of leeches, which under certain circumstances have a most beneficial effect, care should be taken to place them as near the seat of inflammation as possible, taking care to avoid, when chancres also exist, any point which the virus might attain. Great care should also be taken not to place them upon any loose cellular tissue, such as upon the penis, scrotum, &c., for fear of œdema and gangrene.

When the inflammatory stage has passed, the antiphlogistic treatment should be given up, and the diet may become a little more tonic. The general baths should also be abandoned, which may only serve to keep up the discharge.

The injections, which were no longer applicable after inflammation had commenced, may again be resorted to. They should be always cold, and should be made in such a way as to extend through the entire length of the canal.

In the use of the nitrate of silver, M. Ricord advises the following injection:

℞
Argenti Nitratis, gr. ij.
Aquæ Dest., ℥ viij.
M.

The daily number of these injections depends on circumstances; six per diem are ordinarily sufficient. If the discharge becomes bloody, then the injections should be suspended. The proportion of nitrate of silver may be augmented gradually, if it fails to have the desired effect upon the discharge.

M. Ricord makes use of the two following formulæ as an abortive method, or after the inflammatory stage has passed.

℞
Zinci Sulphat., } āā ℈ ij.
Plumbi Acetatis, }
Aquæ Rosæ, ℥ viss.
Ft. injectio. M.

℞
Zinci Sulphatis, gr. xviij.
Plumbi Acet., ℈ ij.
Tinct. Catechu, } āā ʒ i.
Vini Opii, }
Aquæ Rosæ, ℥ viss.
M.

In the use of these injections, three should be employed per diem, allowing the liquid to remain in the urethra from half a minute to a minute.

The only evil results from injections, depend upon their mal-administration, or upon their failing to act upon the membrane.

In the use of the internal remedies, copaiba or cubebs, we are to be guided by their effects upon the various constitutions, and the choice of the two substances depends upon circumstances. They may be administered separately, or combined.

BALANITIS.

In the treatment of balanitis, or external blennorrhagia, a superficial cauterization with the solid nitrate of silver, followed by the interposition of dry lint between the surface of the gland and the prepuce, generally gives the best results. Where the gland cannot be exposed, the stick of nitrate of silver may be passed beneath the prepuce, followed by simple injections of the acetate of lead, black-wash, &c.

In the female, when the disease has attacked the vagina, the acute stage requires the antiphlogistic treatment, and the use of emollient and sedative injections. After or before the acute stage, the parts should be cauterized with the solid nitrate of silver, making use of the speculum for this purpose. Lint should be interposed to keep the sides of the passage from coming in contact. Astringent injections are also useful, employed three or four times a day.

When the female urethra is also attacked, the same treatment is applicable which was advised for the male.

CHORDEE.

Camphor and opium, administered internally, are the most powerful sedatives that we have to combat the painful erections. Cataplasms and fermentations may also have a beneficial effect.

ORCHITIS.

M. Ricord is of opinion that the testicle is rarely or never the seat of acute inflammation, the consequence of blennorrhagia, but that, on the contrary, it is the epididymis which is affected. In this affection, general bleeding, if the habit is very plethoric, may be sometimes necessary. The application of leeches upon the perineum, or upon the inguinal region, with the general antiphlogistic treatment, is all that is in most cases required. If there is effusion into the tunica vaginalis, it should be evacuated by puncture, whatever may be the stage of the disease.

Compression by means of adhesive plaster should be made use of, to reduce the swelling after the acute stage has passed. Under certain circumstances, where the compression is not applicable, the use of mercurial ointments is indicated.

GLEET.

Nothing is more common than the persistence of certain discharges, the result of blennorrhagia—presenting themselves under the form of a drop at the meatus urinarius—more or less strongly marked, according to circumstances. In cases rebellious to the general treatment, and in which no alteration of the tissues exists, injections of iodine are very useful, in the proportion of a drop of the tincture of iodine to an ounce of water, increasing or diminishing the dose according to the circumstances. M. Ricord has also found the protiodide of iron, in the proportion of five grains to the ounce of water, very useful in cases of long standing.

The passage of the bougie is also in some cases followed by the best results. In many cases the instrument may be smeared with the mercurial ointment, and forced into the urethra. Most ordinarily, the morbid discharge is much increased under this treatment, the instrument acting as an irritant. When the discharge finally ceases, under this treatment, we may in most cases be sure that we have obtained a definite cure.

The diet, in these cases, should be rather tonic and generous than otherwise.

Our success in cases of this character generally depends upon our causing the chronic stage to pass into the acute one.

BLENNORRHAGIC OPHTHALMIA.

Every cause susceptible of producing ophthalmia, is a predisposing cause of blennophthalmia in the individual who is suffering from blennorrhagia. The muco-pus of this disease has a great tendency to the production of the blennorrhagic ophthalmia—in fact, this disease only follows after an urethral blennorrhagia. It is very rare in the female, as are blennorrhagic discharges of the urethra.

This malady is brought about in two ways.

1st—By the contact of the muco-pus upon the conjunctiva. An actual contact is necessary in order that this form of the disease should take place. Generally, one eye alone is affected, commencing by an itching of the lower eyelid and followed by all the other symptoms of common ophthalmia. The secretion of the Meibomian glands is augmented, being at first muco-purulent, and afterwards purulent. Inflammation of the lachrymal passages never supervenes upon this disease.

2d—By sympathy, or by metastasis, as it is usually termed. We may be easily convinced that the metastasis of a blennorrhagic discharge is entirely imaginary. So in epididymitis, the repercussion of the discharge as productive of this, is also imaginary.

We may style this form of blennorrhagic ophthalmia, sympathetic, and then we shall be safe from discussion. This form is rarely met with before the second week of the blennorrhagia, and generally later, when the disease is upon its decline. Both eyes are ordinarily attacked at the same time. The conjunctiva is attacked, but there is less œdema and tumefaction than in the other form; on the other hand, however, there is an injection of the sclerotic coat. Accompanying this form, there is also what may be termed a blennorrhagic iritis, where the disease does not attack the parenchyma of the iris, as in syphilitic iritis, but the serous membrane. We observe also much less frequently, the deformities of the pupil in this form of iritis; neither is the color of the iris changed. Once cured, we may consider the disease as not liable to return. A circumstance which aids us in our diagnosis, is the presence of the articular pains, dependent on blennorrhagia.

This form of ophthalmia is far less acute than that produced by contagion, and its progress less rapid; for in the latter, the most fearful ravages are made in the course of twenty-four to forty-eight hours.

Treatment.—The local treatment is the most important. This consists, when there is no ecchymosis, in cauterizing with the nitrate of silver. M. Ricord prefers the solid stick, sharpened to a point. It should be done by first reversing the superior lid, cauterizing it thoroughly and rapidly; next the lower lid, and finally the ocular conjunctiva. A solution of the same substance in this proportion,

℞
Argenti Nitrat., ℈ v.
Aquæ Dest., ℥ j.
M.

is better applicable for children. After these cauterizations, some oily substance should be applied to protect the action of the caustic upon the cornea. If the tumefaction and other symptoms diminish, it is not necessary to have recourse again to cauterization; but in the contrary cases, they should be made twice every day. In order to combat the ecchymosis, and to relieve the stricture produced by this, deep incisions should be made into the parts. When the ecchymosis becomes of a phlegmonous character, deep incisions are necessary, and even the use of the curved scissors may be requisite.

The presence of the pus in the eye aggravates the malady; consequently frequent bathing is necessary. The heads of poppies make an excellent collyrium.

With the local treatment, a strict antiphlogistic regimen is to be insisted upon. Leeches should be applied upon the course of the jugular vein, and not upon the temples. The naso-labial region is an excellent one for this purpose.

Saline cathartics should be vigorously employed, and should be administered at once. Compresses wet with the same liquid that we bathe the eye, should be laid over the organ. Frictions of belladonna about the orbit to mitigate the pain and sensibility, foot-baths, &c., should enter into our course of treatment. It is useless to have recourse to anti-blennorrhagics, for in the blennorrhagic-ophthalmia the result of the contact of the pus, the suppurating discharge from the urethra is of no consequence, while in the form of the disease from sympathy, it is very necessary to arrest this discharge.

Vesicatories should never be applied during the acute stage; but when the inflammation has become chronic, they may be applied behind the ears. The granulations should be treated by means of collyria of sulphate of zinc, rosewater, sulphate of copper, &c.; and if there is yet pain, a little laudanum may be combined with them.

BLENNORRHAGIC ARTHRITIS.

This is a quite frequent complication of blennorrhagia. It never occurs without inflammation of the urethra; so that blennorrhagic inflammation of the vagina is never followed by blennorrhagic arthritis. It occurs most frequently in young subjects, and particularly in those inclined to a lymphatic or rheumatismal habit — and is more often met with in the male than in the female. All the causes which predispose to rheumatic affections, predispose to this complication. A blennorrhagic discharge quickly suppressed, has no influence whatever in producing arthritis. The discharge from the urethra and the arthritis, often exist and proceed together.

Blennorrhagic rheumatism rarely makes its appearance before the second or third week. The anti-blennorrhagics which have been accused of having produced this affection, have no effect whatever upon it.

Seat of the Disease.—It is rather the serous articulations which are attacked; most frequently the knee, and afterwards the tarsal articulations, the sterno-clavicular, &c. One articulation, in most cases, alone is attacked, so that the affection is mono-articular. It is true that several of the joints may be painful, but one alone is actually the seat of the disease.

Ordinarily, this form of rheumatism is unaccompanied by fever. The pain is aggravated by movement, the joint becomes tumefied, and soon there is fluctuation in proportion as the knee swells. The skin remains of its normal color. All the complications which follow common rheumatism, may be present in this form, although they are generally more rare, and much less intense. M. Ricord has observed pericarditis in consequence of blennorrhagic arthritis.

The progress of the disease is rapid, but without fever. It is not in the nature of this affection to proceed to suppuration, but it may be the occasion of the suppuration of *white swellings.*

The Diagnosis of blennorrhagic rheumatism is not always easy; but the fact of its being often mono-articular, sub-acute in its progress, without fever, the fluctuation in the articulation easily displaced, and terminating often by resolution, without leaving any traces, although it may lead to all the affections of the articulations — all this conducts us to the formation of a correct diagnosis.

The Prognosis is not grave; it only assumes a serious character when complications exist.

The Treatment — absolute repose, and a perfect relaxation of the muscles. The blennorrhagic discharge should never be reproduced; it is even more advisable to arrest it. Opiate and camphorated frictions, emollient cataplasms, fomentations, &c., should be employed. Antiphlogistics, if inflammatory symptoms appear, although in the majority of cases we should not have recourse to them. Vesicatories *camphorated*, may be applied to the knee. Internally, diuretics may be administered — such as tincture of colchicum and the nitrate of potassa. In fine, all the means employed against the maladies of the articulations are here applicable.

[NOTE.—We are firmly convinced that there is no specific treatment for the cure of blennorrhagia. A course of treatment applicable in one case, and followed by a speedy and definite cure, in another case proves entirely useless. There is perhaps no disease which is more annoying and more unsatisfactory for the practitioner to treat, than the disease in question. The use of injections we are satisfied is in most cases highly beneficial; but we are also of opinion that their use is greatly abused, only serving in many cases to keep up a discharge which it is our aim to overcome. We are indebted to M. Perdrigeon, formerly interne of MM. Ricord and Velpeau, for a course of treatment which has proved highly satisfactory in arresting the disease both in its acute and chronic stage.

The usual antiphlogistic regimen is to be pursued.

As an abortive treatment, when no symptoms of pain or inflammation are present, one injection should be made of the following.

℞

Argenti Nitrat., gr. xviij.
Aquæ Dest., ℥ i.
M.

This often proves sufficient to destroy the morbid action of the disease at once. Internally, the pulv. cubebæ should be taken in the dose of one ounce per diem, divided into three doses. If diarrhœa is produced, the combination of small portions of pulv. aluminis is sufficient to arrest it. The dose of cubebs may be diminished as the discharge is arrested, but should be kept up for several days after all disappearance of the disease.

Where the disease has become chronic, the following course of treatment is to be pursued.

The regimen to be more or less antiphlogistic, according to circumstances.

The cubebs to be taken as in the abortive treatment just mentioned.

The following injection to be taken on the evening of the *third* day on going to bed, after commencing the Pulv. cubebæ.

℞

Argenti Nitrat., gr. x.
Aquæ Dest., ℥ iss.
M.

This injection to be repeated on the morning of the 7th day, and again if necessary, on the 10th day.

In most cases we are persuaded that injections into the urethra are not properly administered by the patient—who fears that there is danger of its passing into the bladder; whereas the difficulty is to make the liquid arrive at the seat of the disease, which in chronic cases occupies the deep portions of the urethra. The syringe should be filled once only, and in order to secure the passage of the liquid to the desired portion of the urethra, it should be *stroked* down by the repeated movements of the right hand upon the dorsal surface of the penis, while the left hand is engaged in retaining the injection within the urethra. The injection should be allowed to remain from half a minute to a minute, and then suffered to escape by the spasmodic movement of the muscles.

M. Ricord speaks of the use of mercurial ointments passed into the urethra. We have found most satisfactory results often follow from the use of the double mercurial ointment, besmeared upon the end of a small *olive-shaped bougie*. The passage of the instrument should be made as quickly as possible, in order that the ointment may arrive at the deep-seated portions of the urethra. D. D. S.]

SYPHILIS.

ABORTIVE TREATMENT OF CHANCRE.

Whatever may have been the duration of a chancre, cure it as soon as possible. The cauterization, in destroying the specific ulceration, prevents the virulent action of the pus upon the glands. In those cases which are the most precocious, the specific induration may come on the second or third day. M. Ricord does not know of an example where the chancre which has been thoroughly cauterized within the first three days of its appearance, has been followed by constitutional symptoms.

Excision and Cauterization.—Excision is sometimes a good method of treatment, when we can take in a sufficient amount of the neighboring tissues about the primary sore; but cauterization is preferable. Nitrate of silver may be employed in those ulcerations which are very superficial. Vienna paste also constitutes a good caustic, but the acid nitrate of mercury is preferable to all others. It acts both deeply and very quickly. The dressings should be in proportion to the amount of suppuration.

We should make use of charpie or lint. Greasy substances are worthless in dressing the primary ulcer. Mercurial ointment is detestable. Astringent dressings are the best, inasmuch as these act by hardening the ulceration.

The aromatic wine and the solutions of iron, form an excellent means of reducing a chancre to its most simple condition. Care should be taken to always wet the dressing before removing it. If the granulations become too exuberant, the best application is the nitrate of silver. A mild regimen should be pursued. No stimulants given. Warm baths, often repeated, are very excellent.

If the chancre has resisted this treatment, and has become inflamed, the antiphlogistic treatment should be pursued, without, however, the use of leeches. If it becomes necessary to apply them, they must be placed at such a distance that the pus from the chancre shall not come in contact with them; and to prevent them from becoming inoculated, astringent dressings may be applied. Emollient cataplasms, fomentations, warm baths, antiphlogistic regimen, saline cathartics, complete the treatment. If a gangrenous tendency manifests itself, we must diminish the antiphlogistic treatment, and have recourse to a stimulant one. If gangrene takes place in the region of the prepuce, excise the gangrenous parts, and do this before the gangrene stops spontaneously, because if this is not done, it soon attacks the gland itself and all the neighboring parts.

PHAGEDENIC DIPHTHERITIC CHANCRE.

The first thing to be done with this form of chancre, is to neutralize the ulceration, which may be done with an active caustic, as the acid nitrate of mercury, Vienna paste, or the actual cautery. As these ulcerations are very painful, the best sedative is the use of the nitric acid, first etherizing the patient. The solutions of iron are also useful, as also opiate solutions and emollient fomentations. We must change the dressings often, and continue them as long as they appear beneficial. At the same time we must search for the cause of the phagedenic condition; and if it depends upon a scrofulous or syphilitic diathesis, or upon a constitution affected by mercury, our treatment should be directed to these varied conditions. Care should be taken of the digestive organs.

INDURATED CHANCRE.

Cauterization never produces specific induration. Cauterize before the appearance of induration, and you always prevent it from occurring.

In cauterizing the chancres already indurated, you cause this condition to disappear, but you have no effect upon the constitutional symptoms. The number of the chancres has no effect upon their induration.

The dressings should be made with dry lint. The mercurial dressing is very salutary. The exuberant granulations should be repressed.

The following pomade serves to hasten the resolution of the ulcer.

℞
Hydrargyri Ammoniati, gr. xviij.
Adipis, ℥ j.
M.

A mercurial treatment should be employed against the indurated chancre, in order to prevent secondary symptoms. The more the induration is well marked, the more chance there will be that the constitutional symptoms will be serious. The influence of the treatment upon the induration will show the influence of the treatment upon the individual himself.

BUBOES.

Acute Bubo. — Combat the inflammation, and as a prophylactic measure destroy the chancre as soon as possible. Complete repose, antiphlogistic regimen, leeches, cold applications, and compression, all have their beneficial effects. Emollient cataplasms, and gentle frictions with belladonna, or with mercurial ointment which acts as an antiphlogistic and antiplastic, may be used.

When the bubo suppurates and the pus is virulent, it should be opened early, by means of numerous punctures, even in some cases before the pus is formed, in order to limit its extent. The openings should be made in the longitudinal sense of the bubo, and in that portion where it has the most tendency to extend. M. Ricord makes his incisions parallel to the direction of the inguinal fold, or region. The extent of the incisions should be as small as possible, consistent with a free discharge of the pus. When the skin is yet firm, with a chance of its afterwards closing over where the punctures are made, these last should be multiple. When the suppuration is virulent, and the skin has become thin, we should make a large incision; we should do at once what nature will do at a later period. The incisions once made, they should be kept open, if the bubo is merely phlegmonous; if it is virulent, they will remain open spontaneously. A virulent bubo, producing specific pus, is a glandular chancre, and demands the same treatment as the chancre which preceded it.

Indolent Bubo. — This species scarcely requires a local treatment. We may make use of mercurial frictions, or apply the mercurial plaster; sometimes blisters, followed by mercurial ointment mixed with belladonna.

Compression is only applicable to the sympathetic buboes. When the indolent bubo has resisted all other treatment, caustics are applicable, of which the Vienna paste is the best; but care should be taken to avoid stigmatizing cicatrices.

Mercury, given in the case of bubo following the non-indurated chancre, is injurious.

TREATMENT OF CONSTITUTIONAL SYPHILIS.

We cannot destroy the syphilitic diathesis by a mercurial treatment if it is established, and once established we can only prevent the manifestations of it. The mercurial treatment, M. Ricord repeats, should never be made against the non-indurated chancre. The presence of the catamenia ought not to prevent us from continuing the mercurial treatment. If the woman is *enceinte*, we should treat her specifically so much the more. This treatment prevents abortion, and we may commence it at all periods of pregnancy. The treatment with patients should commence at once when they present themselves; but the season when the temperature is most equal is desirable, and is an excellent adjuvant to the treatment. We must prevent patients from exposing themselves to cold and dampness. Exercise in the open air, and amusements, are also very necessary.

If there are other more active diseases concomitant, we must pay attention to them. We must also take into consideration that with certain individuals mercury is extremely injurious.

Mercury acts very beneficially when it is applicable, and very injuriously when not applicable. It acts powerfully on hematosis, as an alterant and antiplastic.

In continuing this medicament, we arrive at the mercurial diathesis, which is marked by a febrile movement, accompanied by a small feeble pulse. The mercury attacks the fibrine, while syphilis attacks the globules of the blood. At the moment when mercurial intoxication arrives, albumen has been found in the urine. If the mercury impoverishes the blood by altering its fibrine, it improves it by adding to its globules. We should never consider fever as the indication of the beneficial action of mercury, but rather as an accident.

Upon the mucous membrane, mercury acts in two ways, locally and generally. When we employ the corrosive sublimate locally, it seizes upon the albumen of the tissues, and a decomposition takes place.

Salivation is not necessary. We must strive to ascertain the relative dose, the dose which will cause the manifestations of the disease to disappear. Diarrhœa often supervenes, which is an intestinal salivation, and this happens particularly with children, inasmuch as the mouth does not present the conditions necessary for the mercurial stomatitis. This diarrhœa should be arrested at once.

Mercury acts upon the skin as it does upon the mucous membranes, producing abundant sweats. This sweating is to the skin, what the diarrhœa is to the intestinal canal. Sometimes a miliary eruption takes place, but this is a complication without gravity. Mercury has sometimes a peculiar effect upon the skin, promoting and even producing ulcerations (Mercurial phagedenism).

Every pathogenic effect of mercury should be considered as a contra-indication of its employment.

Mercury acts upon the nervous system, producing trembling similar to that of old age; sometimes paralysis, and then again a kind of delirium tremens. But this is rare.

Mercury has been found in the secretions, in the urine, saliva, milk, &c. It is incontestable that all the grave symptoms which have been attributed to mercury, are due to syphilis.

The Modes of Administration.—The best method of giving mercury is by the digestive passages. The medicament must be absorbed, and the mucous membrane of the intestines presents the most ready passage. We should not have recourse to the skin, unless the other method is contra-indicated—and when frictions are made upon the skin, they should be made in different regions, in order to avoid irritating the parts.

Mercury should be given until the manifestations of the disease disappear; and the dose should not be augmented, unless the symptoms warrant it. If necessary, the dose should be *gradually* augmented; and there should be an interval of about eight days between the increased doses.

The medicine should be taken fasting, or three to four hours after a repast. M. Ricord prefers that it should be given on going to bed. If diarrhœa is produced, we must not count upon the specific effect of the medicine. In this case, opium is a corrective, and is indispensable with many patients. Constipation is a contra-indication in the administration of mercury, inasmuch as in patients thus affected there is a great tendency to salivation. The bowels must be kept open.

When mercurial frictions are made, they should be made in a certain temperature, from twenty to thirty minutes, and in the direction of the hairs. The parts should be always washed before renewing the frictions.

RULES.

1.—Always administer mercury internally, if the condition of the intestinal canal allows it.

2.—Apply it to the skin in the contrary cases.

3.—In those cases where the mucous membranes become irritated too soon, and where the skin is equally irritable, so that it is almost impossible to complete the treatment, we must strive to alternate it in a proper manner.

4.—If this cannot be done, in certain cases the inspiration of mercurial vapors may have a good effect.

5.—The sensible effects of mercury, whether good or bad, generally manifest themselves after eight days.

6.—As soon as we obtain an amelioration of the symptoms, we must stop at that dose, and not augment it unless we arrive at a period of *statu quo*.

7.—If mercury produces untoward symptoms, we must modify its employment, or suspend it altogether, because if the symptoms are not always aggravated, the cure is at least almost always retarded.

8.—When the accidents arising from the medicine have yielded, and the syphilis still persists, the mercury should be resumed, with the necessary modifications in its exhibition, which the peculiar nature of the accidents demands.

9.—The same inconveniences do not always again occur, after the medicine has been resumed, but there are cases where we are obliged to suspend and to resume it many times.

We can cure the symptoms for which we are consulted, but we have not yet succeeded in determining the length of time that they will remain without again manifesting themselves. The syphilitic diathesis is not removed, it is always persistent. We must never tell a patient that he is safe from all future manifestations. If a patient consults us as regards entering upon marriage, we must be guided in our advice by the length of time which has elapsed since the cure was effected; but we should never give an absolute guarantee. The only thing that we can say with safety and certainty, is, that the husband can never transmit constitutional syphilis to his wife.

The longer a treatment has been methodically continued, and without interruption, the more chance we have of neutralizing the diathesis. An uninterrupted mercurial treatment of six months, well conducted, ought to neutralize the effects of the diathesis. M. Ricord often finds this sufficient.

When we employ mercurial frictions, the mercurial ointment should be used. The mercurial plaster (Emplatre de Vigo) is excellent in the case of chronic enlargements, and also in the treatment of secondary ulcerations. The *Emplastrum de Vigo cum mercurio* should be employed in the secondary papular and scaly eruptions, still continuing at the same time the general treatment. The red precipitate is an excellent dressing, when we wish to stimulate and cleanse the indolent ulcerations. The red sulphuret of mercury (Cinnabar) may be employed, especially in fumigations. It is an excellent medicament employed against the secondary eruptions of the skin, and especially the dry forms, such as the papular, squamous, &c., and even against the indolent, suppurative, non-inflammatory forms—ʒ ij. to ℥ ss. to a fumigation, at the temperature of 110° to 133°, every day. The baths should be prolonged from fifteen to twenty minutes.

The mercurial vapors act injuriously upon the nervous system.

Calomel may be employed in frictions in the place of the mercurial ointment, or it may be administered internally. It has, however, the inconvenience of purging quickly or of salivating too soon.

The white precipitate is much used in the form of a pomade by M. Ricord, mixed with opium, in the primary ulcers which need a mercurial dressing—especially the indurated chancre.

The iodide of mercury, or protiodide, has the advantage of being better supported by patients, salivating less readily than corrosive sublimate. When it purges, opium should be combined with it. M. Ricord gives the preference to the following formula.

℞

Hydrargyri Iodidi, Lactucarii,	āā ℈ iij.
Extracti Opii,	℈ j.
Confect. Rosæ,	q. s.
M.	

Ft. 60 pil. At first one every night, and at a later period, one morning and night.

Plummer's pills are excellent, when the cutaneous and mucous eruptions are complicated with the common herpetic eruptions.

The turpeth mineral should be employed, under the form of a pomade, in the furfuraceous, squamous affections of the skin, which are not accompanied with inflammation; against the ephilides, pytyriasis, &c.

℞

Hydrargyri Sulphat. Flavi,	gr. xviij.
Adipis,	℥ j.
M.	

Corrosive Sublimate is one of the most active forms of mercury, especially the Liquor of Van-Swieten, of this formula—

℞

Hydrarygri Bichloridi,	gr. xviij.
Alcohol,	℥ iij.
Aquæ Dest.,	℥ xxx.
M.	

From one to five teaspoonsful a day.

The sublimate may be also administered in syrups—such as the simple or that of sarsaparilla.

℞

Hydrargyri Bichloridi,	gr. viij.
Ammoniæ Muriat., Sodii Chloridi, Ext. Lactucarii,	āā gr. xviij.
Syrupi,	O.j.
M.	

The pills of Dupuytren are thus composed.

℞

Hydrargyri Bichloridi, Ext. Lactucarii,	āā gr. viij.
Confect. Rosæ,	q. s.
M.	

Ft. pil. No. 40.

The corrosive sublimate is also advantageously employed in the form of baths, being an excellent local and general medicament in the proportion of ℥ ss. to ℥ i. to a bath, with sufficient alcohol to render it soluble. These baths are very efficacious against certain forms of prurigenous syphilitic eruptions, as well as in the common prurigenous eruptions. They also succeed very well in destroying the pediculi pubis.

There are certain secondary ulcerations of the throat which are very favorably influenced by gargles composed with the corrosive sublimate.

℞

Hydrargyri Bichloridi, gr. i.—v.
Decoct. Conii, ℥ vj.
M.

It is perhaps better, however, to touch the ulcerations with some lint dipped in the solution, because if taken into the mouth it blackens the teeth.

The preparations of gold have been highly recommended, but they are of no service. As for sarsaparilla, M. Ricord says that he gives it when the patient does not actually require anything.

With the mercurial treatment, M. Ricord employs the bitter tisanes, such as the syrup of gentian, &c.

He attaches great importance to some preparation of iron as an adjunct to the general treatment of syphilis, inasmuch as syphilis is a cause of anæmia, and therefore the economy demands some tonic. The tartrate of iron and potash is an excellent ferruginous preparation. A good generous diet, but not a stimulating one, is also requisite in constitutional syphilis.

Tertiary Symptoms.—The mercurial treatment is more or less applicable to syphilitic affections, from their commencement to their termination ; but as the disease advances in age, the beneficial action of mercury diminishes in order to give place to the iodide of potassium. This medicament has more influence upon the tertiary symptoms, than the mercury has upon the secondary symptoms. Administered against *all* venereal symptoms, this medicine may do more harm than good. Against the late secondary symptoms we may prescribe mercury and the iodide of potassium with much success.

The iodide of potassium acts decidedly upon the mucous membrances, and its most specific effect is coryza. These colds, so to speak, do not arrive at that state when muco-pus is produced. They constitute mucous œdematous fluxions, which extend to the eyes, and thus we have a sub-conjunctival suffusion. This sort of ophthalmia never arrives at a state of purulence.

In cases of syphilitic iritis, we ought never to make use of the iodide of potassium, for the fluxion increases the disease. Sometimes it produces bronchial disturbances, but without initial fever. It increases the appetite, favors digestion, and contributes to nutrition. Sometimes a species of salivation is produced similar to that attending pregnancy; but there is never any ulceration of the gums, nor the mercurial fœtor. Some patients complain of a pain in the large cul-de-sac of the stomach, accompanied by fever. Often a little diarrhœa is present, but without inflammation.

Upon the skin it produces a psydraceous eruption approaching to acne; very acute, but its progress much slower than common acne. The seat of the latter is upon the face, chest, &c., but the acne produced by the iodide of potassium is upon all parts, and very often upon the nates. If the acne vulgaris exists, this medicament serves to increase its volume and its activity. It favors hæmorrhage in anæmic patients, and is a powerful diuretic. It has a tendency rather to depress the pulse than to accelerate it. It is a powerful antiplastic. Sometimes in its administration we must combine medicines capable of re-establishing the properties of the blood. It acts powerfully upon the nervous system, approaching sometimes to intoxication.

Its Therapeutic Action.—We may administer the iodide of potassium and mercury at the same time, or one after the other. We may administer it in every form, but the best is the aqueous solution. When there is no necessity for haste, we may commence with five grains, and gradually augment the dose to ℈iiss.—the medium dose at which cures are obtained. M. Ricord has arrived at the dose of ʒiv.

We must continue the use of the medicine until all the symptoms have disappeared, and even two or three months after—giving directions to resume it if the symptoms are reproduced.

Bitter tisanes constitute the best adjuvants. A tonic regimen, plenty of exercise, and every thing that can fortify the constitution, are highly necessary.

When syphilis tends to degenerate into scrofula, the iodide of iron forms one of the best medicines that we can employ. The syrup of the iodide of iron may be employed with advantage.

The iodide of potassium should be given between meals, and the preparations of iron with the repast.

SARCOCELE.

The local treatment of this symptom is often useless. We may employ the common methods. Compression sometimes acts very well.

In the case of the *tumeur gommeuse* the general treatment is sufficient to effect a cure. If the tumors have suppurated, we must not hasten to open them, for the potash often causes these tumors to disappear. It is only when the pus is making too great ravages that it should be evacuated. These tumors once open, we may employ as dressing —

℞
Iodini, ʒ ij.—iv.
Potassii Iodidi, q. s.
Aquæ Dest., ℥ vi.
M.

This forms the best possible dressing that we can employ.

If these tumors are seated in the throat, gargles are important. It is necessary for the patient to feel only a slight pricking in the throat, in order to show that the remedy has a beneficial effect.

PERIOSTITIS.

In this affection, an antiphlogistic treatment is necessary if there are any symptoms of a phlegmonous condition, and the periosteum should be cut down upon too soon rather than too late. In the cases of indolent periostitis, the general treatment suffices; or combined with this the common vesicatories.

In the cases of periostitis where there is a tendency to plastic formation, the general treatment combined with mercurial frictions or with blisters, should be used.

OSTEOSCOPIC PAINS.

In these cases, the general treatment often suffices. There is no symptom which yields more quickly to the iodide of potassium than this. We may employ locally as a plaster —

Ext. Conii,
Emplast. de Vigo cum mercurio, } āā p. e.
Ext. Belladonnæ,
M.

Blisters also may be used.

EXOSTOSIS.

This affection should be treated in the same way as the periostitis with plastic tendency. But when exostosis has passed into the condition of *eburnation* we can do nothing; it is not painful. With the plastic exostosis, dressings with the mercurial ointment may be employed.

CARIES.

General treatment, and when there is any pus, give it exit as soon as possible. Remove all the dead portions, which act as foreign bodies. Caries of the maxillary bones is very common, and when both the tooth and the socket are loose, the tooth should be extracted, which is often sufficient. Sometimes a more serious operation is required. When there are any sequestra in the fossæ navales, they should be removed at once.

The syphilitic cachexy is that condition where the symptoms of the disease persist unceasingly. The remedies act badly upon the constitution. Loss of flesh, yellow state of skin, colliquative diarrhœa, &c., accompany this condition of the economy.

HEREDITARY SYPHILIS.

We cannot deny the inheritance of this disease on the part of the mother, if we can on that of the father. The more the parents are affected with secondary symptoms, the more disposed are the children to have syphilis ; while if the symptoms border upon the tertiary period, the children are more liable to scrofulous affections.

The mere existence of the diathesis is sufficient, in order that syphilis should be inherited ; it is not necessary that the parents should be under the influence of syphilitic manifestations.

Syphilis may produce sterility or abortion. M. Ricord has thought that he has remarked that abortion produced by the syphilitic influence occurred at an earlier period, when the father was tainted, than when the mother was. When the mother was constitutionally infected, the abortion occurred after the fourth month. It is through the circulation that the infant is contaminated.

Congenital syphilis is marked by the presence of the mucous tubercles. All children born of syphilitic parents are not necessarily affected—for these reasons. A treatment may intervene, and prevent the disease from manifesting itself. And then, again, every constitution is not fitted to acquire syphilis. It is during the first six months that the disease shows itself in children. From the moment of its birth, the infant commences to undergo external influences, and the accompanying causes of syphilitic manifestations.

Can it transmit the disease to its mother ? It is difficult to answer this question. M. Ricord has seen honest women, of undoubted morality, whose husbands were infected with syphilis, and who appear to have contracted syphilis from the infant begotten by the husband.

It is rational to suppose this mode of transmission, but it is not proved.

APPENDIX

OF THE

FORMULÆ OF THE "HOPITAL DU MIDI."

APPENDIX OF FORMULÆ.

ACUTE BLENNORRHAGIA.

Abortive Treatment.—Take *one* injection per diem, at two days of interval, of the following liquid —

℞
Argenti Nitrat., gr. xviij.
Aquæ Dest., ℥ j.
M.

ACUTE BLENNORRHAGIA.

Twenty leeches upon the perineum. Warm bath after the leeches. Demulcent drinks. Strictly mild diet. Wear suspensory bandage. Take every evening four of the following pills —

℞
Lactucarii, }
Camphor, } āā ℈ ijss.
M.

Ft. pil. No. 40.

BLENNORRHAGIA.

Cubebs.—Take every day, in three doses, the following powder —

℞
Pulv. Cubebæ, ℥ j.
" Aluminis, ℈ ij.
M.

BLENNORRHAGIA.

Injection of Zinc and Tannin.—Take three injections per diem of the following liquid —

℞
Zinci Sulphat., }
Acidi Tannici, } āā ℈ ij.
Aquæ Dest., ℥ vi.
M.

BLENNORRHAGIA.

Injection of Zinc and Lead.—Take three injections per diem of the following liquid —

℞
Zinci Sulphat.,
Plumbi Acetatis, } āā gr. xviij.
Aquæ Rosæ, ℥ vi.
M.

BLENNORRHAGIA.

Copaiba.—Take every day three tablespoonsful of the following emulsion —

℞
Copaibæ,
Syrupi Tolu,
" Papaveris, } āā ℥ j.
Aquæ Menthæ Pip., ℥ ij.
Aurantii Floris Aquæ, ʒ iv.
Mucil. Acaciæ, q. s.
M.

BLENNORRHAGIA.

Injection of Zinc and Catechu.—Take three injections per diem of the following liquid —

℞
Zinci Sulphat., gr. xviij.
Plumbi Acetatis, gr. xxxvj.
Vini Opii,
" Catechu, } āā ʒ j.
Aquæ Destillatæ, ℥ vj.
M.

BLENNORRHAGIA.

Gleet.—Take three injections per diem of the following liquid —

℞
Aquæ Rosæ Dest.,
Vini Rousillon, } āā ℥ iij.
Acidi Tannici,
Pulv. Aluminis, } aa gr. xviij.
M.

BLENNORRHAGIA.

Injection of a Solution of Iron Filings.—Take three injections per diem of the following liquid—

℞
Ferri Rament., gr. xviij.
Ferri Iodidi, gr. vj.
Aquæ Dest., ℥ vj.
M.

BALANITIS.

Lotion.—Use the following liquid as a lotion, three times per diem—

℞
Argent. Nitrat., ℈ iij.
Aquæ Dest., ℥ vj.
M.

This formula may be used as an injection in the same cases.

CYSTITIS.

Twenty leeches applied to perineum. Take every day, an enema of the decoction of poppy heads, with twenty drops of laudanum added.

Demulcent drinks.

ACUTE EPIDIDYMITIS.

Twenty leeches applied to inguinal region. Cataplasms with laudanum. Keep scrotum suspended. Recumbent posture. Very mild diet.

SUB-ACUTE EPIDIDYMITIS.

Make three frictions per diem with the following pomade—

℞
Unguent. Hydrargyri, ℥ ij.
Ext. Belladonnæ, gr. xxxvj.
M.

Cataplasm of linseed meal should be applied.

CHRONIC EPIDIDYMITIS.

Wear a suspensory bandage. Envelope scrotum in an Emplast. de Vigo cum mercurio.

ACUTE BUBO.

Twenty leeches upon the tumor. Cataplasm. Demulcent drinks. Strict diet.

INDOLENT BUBO.

Make three unctions per diem with the following pomade —

℞
Plumbi Iodidi, ʒ j.
Adipis, ℥ j.
M.

CHRONIC BUBO.

Place upon the tumor an Emplast. de Vigo cum mercurio.

SCROFULOUS BUBO.

Treatment by Iodine.—Take daily three tablespoonsful of the following emulsion —

℞
Iodini, gr. iij.
Olei Amygdalæ, ℥ j.
Emulsio Amygdalæ, ℥ iij.
Mucil. Acaciæ, q. s.
M.

BUBO AND CHANCRE.

Take daily three tablespoonsful of the following liquid —

℞
Ferri et Potassæ Tartratis, ℥ j.
Aquæ Dest., ℥ vj.
M.

Dress the ulcerations twice a day with lint dipped in the same liquid.

MUCOUS TUBERCLES.

Apply the following lotion three times per diem to the tubercles —

℞
Liquor Sodæ Chlorinatæ, ℥ iss.
Aquæ Dest., ℥ v.
M.

After each lotion, powder the tubercles with calomel.

VEGETATIONS.

Apply to the vegetations twice a day the following powder —

℞

Pulv. Sabinæ, Ferri Peroxydi, Alum Calcin,	āā ʒ j.

M.

CHANCRE.

Make three dressings per diem with lint dipped in the Vinum Aromaticum.

CHANCRE.

Make three dressings per diem with lint dipped in a strong decoction of Krameria.

INDURATED CHANCRE.

Make three dressings per diem with the following pomade —

℞

Cerat. Opiati, ℥ j.
Calomel, gr. xxxvj.
M.

SECONDARY SYMPTOMS.

Take every day one of the following pills —

℞

Hydrargyri Iodidi, Lactucarii,	āā ℈ iiss.
Ext. Thebaïque,	gr. xviij.
Confect. Rosæ,	ʒ iss.

M.

Ft. pil. No. 60.

TERTIARY SYMPTOMS.

Take three tablespoonsful of the following syrup —

℞

Potassii Iodidi, ℥ j.
Syrup Gentianæ, O.j.
M.

ALUM GARGLE.

Gargle the mouth three times per diem with the following —

℞

Eau de Laitue, ℥ v.
Mel. Rosæ, ℥ iss.
Aluminis, ʒ ijss.
M.

GARGLE.

Secondary Symptoms in the Mouth.—Gargle the mouth three times per diem with the following —

℞

Eau de Laitue, ℥ v.
Mel. Rosæ, ℥ iss.
Acidi. Muriatici, gtt. xv.
M.

GARGLE.

Tertiary Symptoms of the Mouth.—Gargle the mouth three times per diem with the following —

℞

Tinct. Iodini, ʒ j.
Potassii Iodidi, gr. xviij.
Aquæ Dest., ℥ vj.
M.

GARGLE.

Gargle the mouth three times per diem with the following —

℞

Decoct. Conii, ℥ vj.
Hydrargyri Bichloridi, gr. x.
M.

SALIVATION.

Take every day, ℈ iij. of the Flowers of Sulphur mixed with honey. A lemonade made of Nitric Acid for drink; e. g. thus composed —

℞

Acidi Nitrici, q. s.
Syrup. Simp., ℥ ij.
Aquæ, O.ij.
M.

Gargle the mouth three times per diem with the following —

℞

Eau de Laitue, ℥ v.
Mel. Rosæ, ℥ jss.
Acidi Muriatici, gtt. xv.
M.